Author comments:

We thank the reviewer for taking the time review the manuscript and provide thoughtful and constructive feedback. All comments and suggestions have been addressed, please see the below replies in blue italicized text.

Reply to anonymous reviewer (RC1):

This manuscript presents new tephra measurements in the upper ~20 m of two Mount Brown South ice cores from coastal East Antarctica spanning the recent ~40 years. The authors first conduct a broad, lower resolution investigation of tephra particles in the main MBS ice core before doing more high-resolution geochemical analysis on samples on an adjacent firn core to fingerprint specific eruption sources.

> *We thank the reviewer for the thoughtful feedback, and comments which will improve our manuscript.*

Overall, I think the methods and results are described well, and the geochemical data, and therefore ties to known volcanoes, are robust. While the authors identified a wide range of sparse tephras in their samples, the interpretation is focused (in my view, rightfully so) on the two clusters of tephra identified in the two samples with the largest number of shards. For these two events, the authors identify appropriate candidate eruptions and then logically rule out unlikely eruptions to conclude that Cerro Hudson and Erebus as the sources for the tephra layers they identify in 1991 and 1985, respectively. One aspect that could be better presented is the analytical uncertainty of the geochemical measurements- often these are included as crosses on the major element diagrams (bottom 9 panels of Figs. 7 and 9, for example). I'd suggest adding these uncertainties if possible, though the analytical error is in a supplementary data table.

> *We agree that a visual representation of the error involved with the measurements would be valuable to the reader. We will update the relevant figures to display analytical error.*

While the tephra data and attribution are robust, I did not find the modeling component or some aspects of the discussion to be as convincing and have the following questions/suggestions for those aspects of the manuscript:

First, the HYSPLIT modeling is used to develop air mass back trajectories for the two eruptions to show they are within the probable source region for the MBS site. Given that long-range transport is implicit to most aerosol records in Antarctica, I do not think that using such back trajectories to show that these volcanoes are within the source region is particularly useful since it seems plausible that nearly all volcanoes in the high latitude Southern Hemisphere could result in tephra deposition in Antarctica. To me, it seems the power of concretely attributing tephra to a specific volcanic source is the possibility for modeling emissions scenarios specific to that volcano. Since these eruptions are relatively well documented down to the specific eruption dates, I think running HYSPLIT in forwards mode with ash emissions for each eruption would be very useful to understand if modeled ash dispersion is consistent with the tephra identified at MBS. It may be worth mentioning that HYSPLIT back trajectories are air mass trajectories and therefore do not specifically consider aerosol transport/scavenging (which would impact tephra), but the forwards dispersion model can be specifically run for volcanic ash, though I am not sure how sophisticated the transport scheme is.

> *HYSPLIT was used in back trajectories in both cases, as this is a common way of investigating the sources of air masses received at ice core sites. While we agree that particle dispersion modelling ("ash mode") in HYSPLIT is a powerful tool for investigating ash dispersal, we decided against this for a few reasons. In the case of Cerro Hudson, there are existing satellite observations of the ash and sulfate cloud transport, which represents actual transport of the*

volcanic ash (e.g. Constantine et al., 2000). Additionally, other studies have used Lagrangian modelling to investigate the ash dispersal from Cerro Hudson (e.g. Kratzmann et al., 2010). We therefore made the decision to refer to these studies in lieu of repeating these analyses. We will update the text to more clearly describe our reasoning to not perform HYSPLIT forward trajectories or ash dispersal modeling for the Cerro Hudson eruption.

In the case of the Mt. Erebus, there was continuous eruptive activity at Erebus throughout a number of years in the mid-1980s, and with our sample sizes and the dating constraints of the ice core, we are not able to define an exact single eruption event or specific date that can be pinpointed as producing the tephra found at MBS. Therefore, the Erebus eruptive activity cannot be constrained well enough to glean useful information from HYSPLIT forward trajectories for this eruption.

We appreciate these comments and will add more descriptive text to the relevant sections to clarify our reasoning for relying primarily on HYSPLIT back trajectories.

Second, I did not find that most of the discussion was justified given the limited findings of the manuscript specific to these two modern eruptions. The ice core dating section (5.1) focuses on detailing the uncertainties about ice core sulfate records and presents tephra as a more reliable means of developing tie points. I strongly disagree with this. Volcanic synchronization of ice cores using sulfate has led to the development of extremely accurate ice core chronologies (the volcanic chronologies in Sigl et al., 2014 and 2015 being some of the best recent examples) despite some of the complications associated with these records. While tephra can certainly be a powerful tool to identify specific eruptions in certain ice core records, because it is an insoluble particle it will always be much more heterogeneously distributed in the atmosphere than sulfate and therefore have much more limited utility for synchronizing geographically widespread ice core records. Furthermore, low tephra counts, reworked sparse tephras, and large sample volume required for analysis can hinder cryptotephra work. Maybe this section would be more meaningful if the authors identified these specific tephras in all four cores at MBS as well as in other regions of Antarctica to show a viable widespread signal, but identifying these shards in a single shallow core does not justify this broader discussion of ice core dating.

Thank you for the feedback. We agree that ice core chronologies based on volcanic sulfate records can be incredibly robust and accurate! Our intent is not to call into question ice core chronologies that rely on volcanic sulfate. We aim only to highlight the potential of tephra in ice cores as a strong signal in the case of the MBS array (for which sulfate-based volcanic synchronization was used in chronology development as described in Vance et al., 2024) and similar cores. We will rephrase the text to clarify that these cryptotephra horizons can be a valuable tool in ice core dating in addition to and strengthening the very robust sulfate-based ice core chronologies.

Additionally, while the heterogeneity of tephra deposition across the ice sheet is always a factor in tephrochronology work, we do not see this as an explicit limitation of what we aim to present in this manuscript. The finding of tephra from the two specific eruptions outlined in this work (Hudson 1992 and Erebus 1985) provide concrete tie-points for the ice core presented here (MBS-Alpha). While we only aim to provide concrete dating horizons for the MBS-Alpha core, there is a high likelihood that these volcanic horizons would also be seen in the nearby cores (Bravo, Charlie, and Main) due to their proximity with MBS-Alpha. While this work cannot speak directly to the dating of the MBS-Main core (or other ice cores across Antarctica), we do see value in discussing ice core dating more broadly, as this represents the first attempt at tephrochronology work using the MBS cores, and MBS represents a region that is demonstrably underrepresented in the broader Antarctic ice core landscape. We appreciate the feedback *provided here, and we will revise the text to clarify the positioning of this work as a proof-of-concept for the strong potential of tephrochronology to provide concrete dating tie-points for the remaining MBS-array cores in the future.*

Lastly, sections 5.2 and 5.3 seemed almost too hypothetical to be meaningful. Both sections describe how a longer MBS cryptotephra could be insightful and potentially linked to large-scale climate and teleconnections, which doesn't particularly relate to the findings of this study.

We disagree that the atmospheric teleconnections of the MBS site are not relevant to the study, as our understanding of these teleconnections is what informed our decision to use the MBS cores as a tephrochronological archive in the first place. These teleconnections (and associated large-scale meridional atmospheric transport to coastal East Antarctica) play a significant role in what aerosol/particle material gets transported to MBS, and similarly, the likelihood of the MBS cores to be a strong potential tephra archive. We will expand the text of these two sections to better describe the validity and relevance of the MBS site's teleconnections to the lower latitudes.

Additionally, the authors do not even show that the tephra record from the main MBS record is valid given the limited amount of available sample. All tephra results in this manuscript are from the shallow core where they were able to obtain larger sample volumes, so the sampling approach presented in this study is not applicable to the main MBS core. Overall, I suggest the authors reframe the discussion to be more pertinent and specific to their results presented in the manuscript.

While the study did use MBS-Alpha due to the large sample volume readily available for this work (part of a PhD project), perhaps we neglected to provide the required nuance to our description of the limitations in availability of MBS-Main material. The limitation in available material at the time of the study was in part due to where the cores were located at the time the study took place. While analytical work was undertaken at the University of Copenhagen as part of a PhD study, there is an additional remaining archive located in Tasmania. The use of the smaller samples available in Copenhagen at the time was chosen both based on sample availability and as a preliminary screening and proof of concept for better understanding the potential of the MBS array cores for tephra studies. While we only present geochemical analysis of the MBS-Alpha core, based on the sampling conducted here, we encourage future tephrochronology work from the remaining archive sections of the MBS-Main core.

While the 2 cm^3 it is a small sample size, it is not uncommon for ice core tephrochronology studies to rely on samples of a similar size for tephra sampling (e.g. ~2 cm^3 in Cook et al., 2018). We agree that the small quantity of sample availability from MBS Main may present potential limitations to future tephra studies from this core, however we do not see this as detracting from the validity of our findings. This investigation of tephra in the MBS core array represents one of the first studies investigating tephrochronology from a millennial-scale ice core drilled in the coastal East Antarctic region. MBS, together with the Law Dome ice cores, provides insight into a region of Antarctica underrepresented by other ice core studies. While, as you say, the minimal sample remaining may limit a future tephrochronology study of MBS-Main (though, as indicated above, 2 cm^3 is not an uncommon sample availability for ice core tephra studies), we present this work as an argument for the importance of the region for future tephrochronological studies.

Minor notes

Page 2 line 53: Very nitpicky, but I'd say a 295 m ice core is intermediate, not deep.
We will update our terminology accordingly.

Page 3 line 64-65: I don't think you justify why MBS would be more advantageous than other Antarctic sites for tephra studies. It seems like a coastal site could be even more complicated than interior sites as it would be more influenced by marine biogenic sulfur complicating the link to sulfur peaks and a coastal site presumably would be closer to exposed land and therefore sources of reworked tephra?

These are good points. While there may be challenges associated with the coastal site due to marine biogenic sulfur, we understand one of the biggest advantages of the MBS site (similar to sites like Law Dome) as a tephra archive is its strong atmospheric links to higher latitudes as compared to more interior Antarctic sites.

Page 4 line 75: Define IE (I don't think I saw ice equivalent anywhere else before this).

We will update the text accordingly.

Page 5 lines 87-89: what do you mean by this? Weren't all depths sampled in the MBS main core and used to guide more detailed analysis on the shallow core? How does that link to moisture sources?

Not all depths were sampled from MBS-Main. We used potential moisture transport as one of the criteria for our original sample selection from MBS-Main. We will update the manuscript to make this clear in the text.

Page 6 section 2.3- this seems like a very long-winded way to say that approximate timing of eruptions was guided by linear interpolation between austral summer peaks in sulfate/Cl and austral winter peaks in Na.

Our intention in section 2.3 is to highlight that although we do approximate ages guided by a linear interpolation between summer/winter peaks, the coastal nature of the MBS cores means that linear interpolation may misrepresent the sub-annual dating due to its inability to capture the seasonal scale accumulation variability at the site. We will revise the section for brevity and clarity to better communicate this.

Page 7 line 163- any particular reason that backtrajectories initiated at 1500 m AGL?

Back trajectories were initiated at 1500 m AGL, which at the MBS site, corresponds to approximately 3500 m ASL. A starting height of ~1500 above the ice core site was selected to reduce impacts on the trajectory due to interaction with the surface topography of the surrounding area and minimizes the chance of the long (10-day) trajectories "hitting the ground" and losing accuracy.

Page 16 Fig 7- Why is the polygon/region on the TAS diagram for Cerro Hudson a different shape on this plot than on Fig 6?

The polygon in Figure 6 represents the full range of literature values for Cerro Hudson products. The polygon in Figure 7 represents specifically products from the later phase of the eruption, when the composition of the erupted products shifted towards trachyandesite and rhyo-dacite composition. This is briefly described in section 4.2 (lines 269-270) and specified in the figure legend of Fig 7. We will add text to the figure captions to clarify this.

Page 17 line 306- how would sulfur isotopes allow for disentangling two coincident eruptions that appear as a single mixed sulfate peak in the ice core record? I'm not sure that is possible. I'd just remove the mention of sulfur isotopes or explain it.

Burke et al. (2019) present a method of distinguishing tropical and extra-tropical eruption signals based on sulfate isotope fractionation due on the stratospheric height of the sulfur injection. While Burke et al. (2019) do not extend this to directly compare the sulfate signals of the Pinatubo and Cerro Hudson eruptions, we see this method as a promising tool that could be applied to these eruptions.

Page 18 Fig 8- I'm not sure the AVHRR data appeared as intended. It seems extremely blurry.

This is the original resolution from the figure reproduced from Constantine, et al. (2000). We will do our best to update the figure with better resolution and clarity.

Page 21 Fig 10- Why the switch to trajectory frequencies instead of backtrajectories? I actually find this approach more useful than individual backtrajectories so I wonder how this would look on Fig 8.

Trajectory frequencies were selected for Figure 10, as we do not know an exact eruption date for the eruptive activity of Erebus that produced the material found in MBS, and thus ran back trajectories over a greater period of time (856 trajectories for Figure 10, compared with 44 in Figure 8). The visual representation of the 44 individual trajectories is much clearer, and better represents of the actual trajectory paths when plotted as individual trajectories as compared to trajectory frequencies. We are of the opinion that the use of trajectory frequencies in Figure 10 better captures the uncertainties associated with the eruption timing and ice core dating in the case of the Erebus eruption. We will add text in the relevant sections to better describe why the specific trajectory plots were presented as such.

Page 25 line 481- be consistent with the number of shards. These are different than Table 1 and those stated on lines 417-418.

This is a mistake and will be corrected accordingly.

References:

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