Author responses for Referee1 – Jacob Chalif

In the paper "An annually resolved chronology of the Mount Brown South ice cores, East Antarctica," T.R. Vance and coauthors present four new chronologies for ice cores (three surface cores and one deeper core) from Mount Brown South (MBS) in East Antarctica. These chronologies were developed through a combined multi-researcher annual chemical layer counting and volcanic alignment approach, which is common in dating annually resolved ice core records. Preliminary analyses of ice core chemistry, in particular comparisons with Law Dome and an analysis of the seasonal cycle of certain chemical species used in the annual layer counting effort, are presented as well.

I applaud the authors for their comprehensive discussion of the methods used in analyzing the MBS ice cores. Not all ice core studies are so transparent in detailing their methods of analysis, but the authors do a commendable job of laying out their complete analytical regime, which involved multiple cores, institutions, and types of chemical and physical analyses.

Given the quality of the annual layers in the MBS ice cores, I completely agree with the method the authors chose to use to establish the MBS chronologies.

In Section 4.1, the seasonal cycles of trace chemical species are discussed. This discussion is foundationally important for their dating methodology, but the authors spend the majority of this section proposing a new mechanism to explain the seasonality of the fluoride signal. I do not think that the authors need to explain the origin of the fluoride signal to defend its use in their dating methodology, as the presence of its seasonal cycle is plainly evident regardless of its cause. That said, it is suggested that the fluoride seasonality is linked to sea ice seasonality and the behavior of East Antarctic polynyas. While I do find their hypothesis compelling, the authors do not provide sufficient observational or modeling results to support this hypothesis. I believe that it warrants a deeper investigation to be included in this study, especially given that the authors point out the many uncertainties in their interpretation, including (1) the difficulty in measuring fluoride due to its low concentrations, (2) the inconsistency of its reliability at different periods of the MBS record, (3) the volatility of fluoride, and (4) the existence of alternative explanations for the seasonal signal. Perhaps, as the authors suggest in Lines 394-5, a separate study examining sea ice proxies in the MBS cores would be a better place to introduce and test this hypothesis.

These are all fair points, and are raised in a largely similar fashion by the other reviewer of this manuscript. Given this, we will comprehensively revise this section to remove the bulk of the larger discussion around the sources of fluoride at MBS. We will re-investigate the sources and seasonality of fluoride in a separate manuscript as suggested.

Besides this one issue in Section 4.1, I found that the paper is well-written. This paper will be very useful for future analyses of the MBS ice cores, which will be a valuable archive of East Antarctic climate proxy records. Additionally, given the authors' thorough discussion of their methods, this paper will be a useful community document detailing the chemical analysis of and chronology development of annually resolved ice cores more broadly.

I recommend its publication after resolving the above point and the following minor points:

- Section 1: I appreciated the extensive discussion of site characteristics, but I wondered why the authors spent so much space discussing the wind characteristics? I don't believe that they ever returned to this later on in the discussion of the chronology development.

This manuscript was envisaged as not just a discussion of how the MBS chronology was developed, but also to have a general discussion around dating error and uncertainty, and its root causes. MBS is a high wind site (at least in comparison to other east Antarctic records we are familiar with) and we suspect this high

wind regime has a lot to do with the episodic nature of the accumulation at MBS (e.g. Jackson et al., 2023), the seasonal cycle of accumulation (Crockart et al., 2021), and the fact that there is likely to be frequent erosion events that will remove detail from the record (limiting the ability to perceive annual layers). Thus, understanding mean wind speed and direction during high accumulation and low accumulation periods, at least at the seasonal level, is critical to understanding where we might put a foot wrong in the dating process, so to speak. We wish to keep the wind characteristics discussion in the general site description section as we think it is highly relevant to understanding the root causes of dating errors, and the wind rose data is highly relevant to understanding the site features (e.g. sastrugi, dunes) that are evident in Figure 1b, as we do return to the discussion of how the site is likely quite episodic later in the manuscript.

- Figure 1, caption: I suggest the authors replace "fuschia" with "red" or "pink" and "cyan" with "blue" as these would be more universal color labels.

We will do this

- Line 111: It is stated that there is a mean sample resolution of 10 samples y-1. As resolution decreases with depth, it might be more useful to give a range of sample resolutions along the core (i.e., perhaps a mean sample resolution for a section of core near the top and a mean sample resolution for a section of core near the bottom).

We will do this

- Section 2.4: It would be useful to include a schematic of the CFA melter system used, either in the Appendix or as a main figure. If space limitations are a concern, I would suggest that Figure 3 could be combined with a CFA schematic into one new, slightly larger figure. See example CFA schematics in Figure 2 of Hoffman et al. (2022) or Figure 2 of Osterberg et al. (2006), among other papers.

We agree that a schematic of the CFA system would be useful, however the CFA system used for the MBS analyses was a modified version of that used by the Danish team in the past. As a result, two papers specifically detailing the CFA setup and datasets for both MBS impurities (Harlan, Kjær et al., in prep) and MBS isotopes (Gkinis et al., in prep) are being prepared and are close to submission. The modifications to the CFA system, which was used to process the MBS record therefore deserves a detailed study, including schematics of the new CFA setup which will be included in the Harlan, Kjær et al manuscript. It would not be appropriate to include those schematics here, as they are currently unpublished, but we can make reference to these upcoming papers in the manuscript.

- Line 228: The proper name for the "National Ice Core Facility" is the "National Science Foundation Ice Core Facility" (previously, it was named the "National Ice Core Laboratory," hence the confusion).

We will correct this

- Section 2.8, and Line 480, and Line 505: A simple schematic or table, even in the Appendix, illustrating the authors' 4-step dating method would be useful. I reread the dating section a few times and still struggle to understand when various chemical species were used in different dating schemes.

Thank you, this is a good idea - we will devise a schematic to assist the reader.

- Line 365: I believe the authors mean "DMS" (dimethyl sulfide) when they write "MSA". Algae produce DMS, not MSA, and DMS is oxidized in the atmosphere into MSA and/or non-sea-salt

sulfate, among other products and intermediates (see Figure 1 of Fung et al. (2022) for a nice overview of DMS oxidation chemistry).

Yes, We will correct this, thank you.

- Figure 5: It would be useful to include row labels ("Main core non-satellite era", "Main core satellite era", "Charlie surface core") at the side similar to the column labels at the top.

We will do this

- Figure 5: The y-axis limits on the left 3 columns are such that the seasonal cycle is very clear, but for fluoride the axis limits are much wider than the fluoride seasonal signal, making its seasonality stand out less. I am wondering why the authors chose to minimize the apparent magnitude of its seasonal signal?

The fluoride concentrations are very low to begin with, and it seemed disingenuous to not show the smaller stature of the seasonal cycle. All the y-axes start at zero for the other species, so it might be considered misleading to not have the same axes for the fluoride. This shows that while a seasonal cycle is present, the low concentrations mean that it may not be analytically detectable from one year to the next. We would prefer to leave the fig 5 axes as is.

- Lines 455-8: The authors suggest that a key difference between the MBS and Law Dome ice cores is that MBS exhibits clear seasonality of fluoride, but then note that there is no fluoride dataset from Law Dome. I may be misunderstanding this, but the authors should not call this a difference between the sites if there is no evidence for the lack of fluoride seasonality at Law Dome. If they mean that this is not a difference between the site characteristics, but only a difference in how ice cores from the two sites were dated, this should be specified.

That's fair and confusing as currently written – we will re-write this to be clearer. There is a very small dataset of fluoride from Law Dome, but it is not yet processed or worked up, so we cannot compare the pair properly yet.

- Line 472: couldn't the hypothesis that there was a "training" period for the researchers near the top of the core be eliminated by repeating the layer-counting, at least for the top section of core?

Yes, however the point of the comparison to the layer counted only effort was to give a kind of 'worst case scenario' for missed layers. We will re-write this section to be clearer that that is what we meant.

- Figure B1: Y-axis labels for each series would be very useful.

The units are micro Equivalents per liter. We will add this to the caption.

Typographical errors:

We will fix these typos (below)

- Lines 90: I believe the word "was" is missing between "weight" and "recorded"
- Line 132: "non sea salt component" should be changed to "non-sea-salt component" in order to maintain consistency with the rest of the usages of "non-sea-salt" throughout the paper
- Line 179: there is a closing parenthesis, ")", where there should be none after the word "capillary"
- Table 3, caption: there is a missing closing parenthesis, ")" after "(see Plummer et al. (2012)"

- Lines 349-51: The usage of “e.g.”’s are not consistent. In Line 349, e.g. begins a parenthetical, whereas in line 350, e.g. comes after a comma. I believe they both should begin parentheticals
- Line 361: “MBS-main” should have a capitalized “M” in “main”
- Line 442: There is an extra opening parenthesis, “(“, before “van Ommen”

References

Fung, Ka Ming, Colette L. Heald, Jesse H. Kroll, Siyuan Wang, Duseong S. Jo, Andrew Gettelman, Zheng Lu, et al. “Exploring Dimethyl Sulfide (DMS) Oxidation and Implications for Global Aerosol Radiative Forcing.” *Atmospheric Chemistry and Physics* 22, no. 2 (February 1, 2022): 1549–73. https://doi.org/10.5194/acp-22-1549-2022.

Hoffmann, Helene M., Mackenzie M. Grieman, Amy C. F. King, Jenna A. Epifanio, Kaden Martin, Diana Vladimirova, Helena V. Pryer, et al. “The ST22 Chronology for the Skytrain Ice Rise Ice Core – Part 1: A Stratigraphic Chronology of the Last 2000 Years.” *Climate of the Past* 18, no. 8 (August 10, 2022): 1831–47. https://doi.org/10.5194/cp-18-1831-2022.

Osterberg, Erich C., Michael J. Handley, Sharon B. Sneed, Paul A. Mayewski, and Karl J. Kreutz. “Continuous Ice Core Melter System with Discrete Sampling for Major Ion, Trace Element, and Stable Isotope Analyses.” *Environmental Science & Technology* 40, no. 10 (May 1, 2006): 3355–61. https://doi.org/10.1021/es052536w.