**Response to reviewer comments on "**Physico-chemical properties of the top 120 m of two ice cores in Dronning Maud Land (East Antarctica)….." **by Wauthy et al.**

We would like to warmly thank both referees for their very constructive comments which have greatly improved the quality of the manuscript. We provide here below detailed "one to one" response to all their comments and a separate version of the new manuscript with related track changes.

**Referee 2**

In this paper the authors present water isotope data and ion concentrations from the top 120 m of two ice cores drilled at adjacent ice rises in Dronning Maud Land, Antarctica. Both cores were drilled deeper but the whole data sets are not included in this paper. The data from the ice cores presented here cover about 250 years.

The main finding presented in the paper is that the annual records between the two coring sites are quite different despite their geographical closeness. However, on longer time spans the data agree well. These are not new discoveries, but it is nevertheless important to emphasize that short time periods are not necessarily reliable estimates of either SMB or any other climate indicators.

After decades of having the main focus on inland ice cores there has recently been more interest in recovering cores from coastal sites where accumulation is higher, thus with the possibility of obtaining annual records. The coastal ice cores presented here are not the first from this part of Antarctica. But to resolve the complexity of the spatial variability and the impact of various climate induced processes these data sets are important contributions to the understanding of the paleorecords from coastal ice core records from this region.

However, before these datasets can be fully utilized it is necessary with some restructuring and rewriting of the manuscript. Basically, there is some important background information lacking which makes it difficult to evaluate the robustness of the results. Below I have tried to summarize some of my major concerns that I hope that authors will consider for the next version.

**Major comments**

- A major problem with the manuscript is a proper description of the field area and previous work done. As far as I can tell there are two previous papers published including data from these core sites (Kausch et al., 2020 and Cavitte et al., 2022). However, the findings from these studies are not properly integrated in this manuscript. It is not until in "Discussion and perspectives" that this becomes obvious to me. I suggest having a separate "Background" chapter where all this information is included. This should also include information about the meteorology. I find a brief mentioning of an AWS quite far into the manuscript (line 246). Naturally, this information should be supplied at an early stage.
  We developed the section 2.1. to take these suggestions into account:
  - the procedure for the drill site selection is now addressed.

- the use of our datasets to provide preliminary dating for two previous papers (Kausch et al., 2020 and Cavitte et al., 2022) is now mentioned in this 2.1 “Field” section, but we have chosen to keep a more detailed discussion of the findings of these papers in the “Discussion and perspectives” section, in the light of the “Results” section on our own findings.
- a new paragraph now describes briefly the main meteorological information acquired with the AWSs and the temperature and snow accumulation records are shown in a new appendix.

- The dating section needs to be expanded and the error discussed. The authors claim that both cores are annually resolved but no evidence for this is shown. The volcanic chronology is fundamental for the chronology so I would like to see what is described as “the well-defined Tambora marker” (line 318) together with the stable isotope data in these cores to be convinced of the annual resolved dating. As a reader I am left with many questions regarding the chronology. Some examples of my concern are the selection of volcanic eruptions and indications of melt layers.
  We agree with the referee that we did not provide enough information on the dating procedure. We expanded the dating section by a complete description of our dating procedure in a new Appendix (Appendix D). This description clarifies our method by explaining it step-by-step and showing the main different cases encountered when dating and how these were dealt with. “The well-defined Tambora marker” (now lines 364-365 in the new version of the manuscript with track changes) is shown in Fig. 2 and Fig. F1. We have considered presenting the raw data used for dating in the form of figures in the Appendices, but we thought it would be inappropriate use of the journal space since the datasets are published online. Finally, Fig. 5 left and central panels show clear seasonality of the species used for dating, which, in our opinion, confirms the annual resolution of the species and thus of the dating.
  Melt layers impacts are discussed in our response to another comment here below.

- The SMB is the focus of the paper as I see it and thus there should be more emphasis on the error estimates. All the SMB presented in the tables should also come with the error estimate.
  As discussed in the original manuscript, uncertainties on SMBs are important, though tricky to quantify, and not systematically quantified in previous studies. We already gave a theoretical background on the three main types of uncertainties in our section 2.4.1, based on the work of Rupper et al., (2015). We have now built up on it by adding a text on the error estimates in the paragraph presenting the SMB results (section 3.2, lines 340-347 in the new version of the manuscript with track changes), as well as uncertainty ranges in Fig. 4. We also added the errors in the text and tables. For both FK17 and TIR18 records, the average uncertainty for the whole period is ±0.04 m.i.e. for SMB without correction and ±0.05 m.i.e for SMB with vertical strain rate correction.

- In general, the spatial differences between these adjacent ice rises highlight the challenges comparing regional trends using different time periods. I think the manuscript could be stronger emphasizing this. One example: line 65-75 where various records from DML are discussed and it becomes evident that it is crucial to compare the same time period when discussing data from different sites.

We agree that the same time periods must be compared when discussing the SMB variability, we thus added a sentence (lines 551-553) that emphasize this. The same approach is used when comparing specific time windows in Tables 2, 3, G1 and G2.

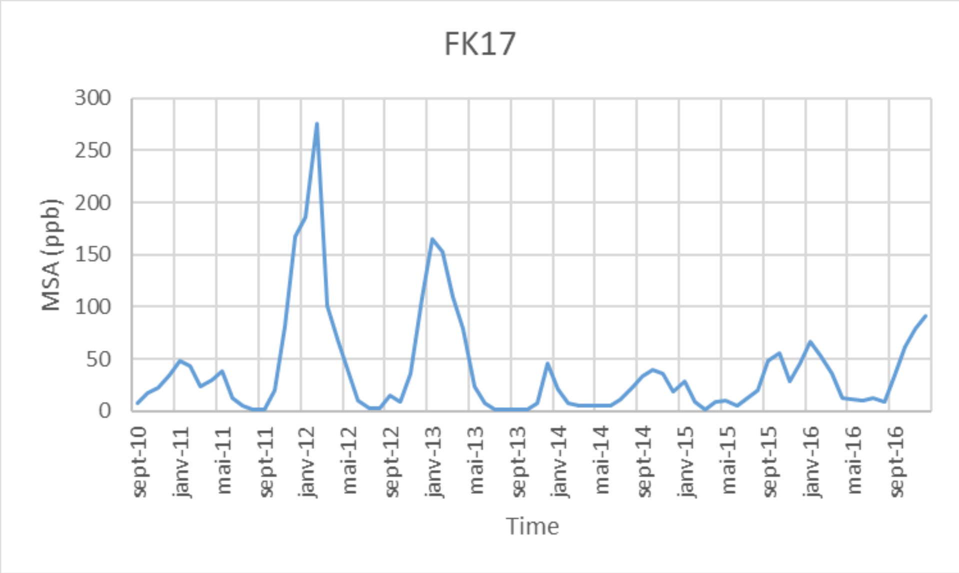
line 110. References to the radar measurements are lacking. I assume that the GPR and deep radar measurements were performed prior to the drilling and helped determine the positions of the cores?
Now lines 111-114. The position of the cores had been identified first roughly using REMA and then, more precisely, as the local highest elevation point of the ice rise in the field using GNSS data. This information has been added in the section 2.1.

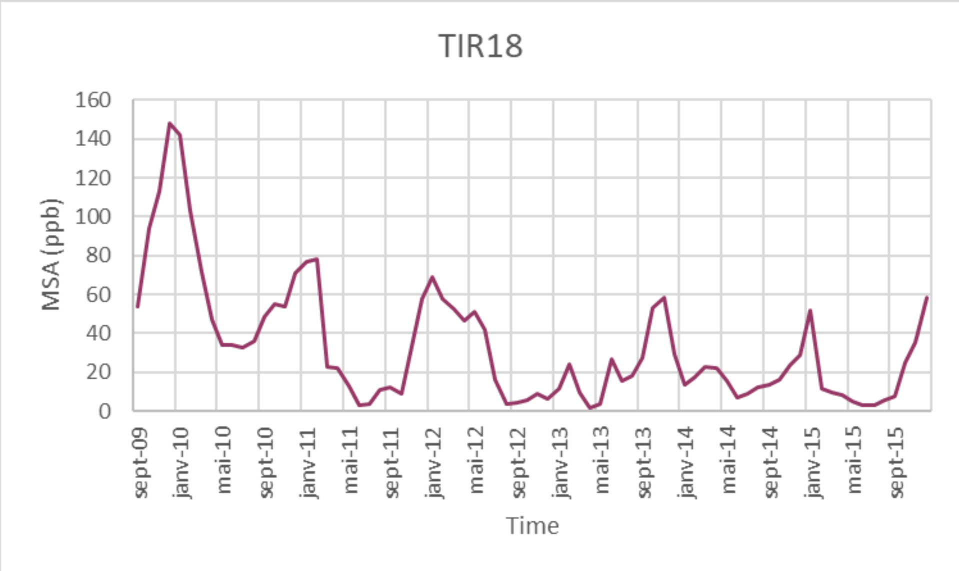
line 204-205. I wonder what "our previous dating" is referring to? This is one example of where it is not clear if these data have been presented in a previous paper.
Now line 234. The "previous dating" referred to the manual dating (based on relative dating and adjusting with absolute age markers from volcanism) described earlier in the paragraph. We thus replaced the expression by "above-mentioned dating procedure".

line 260-272. As already mentioned, I have issues understanding the process developing a robust the age model. The allocation of peaks as volcanic induced should always be treated carefully both due to spatial coverage and the time lag. Therefore, the double peak of 1809/1815 often just called "Tambora" is an extremely valuable time marker in Antarctica. I would like the authors to add information about this both in the text and in a figure.
As mentioned before, this has been clarified by the extended description of the dating procedure in the Appendix D. The Tambora marker is shown in Fig. 2 and Fig. F1. The "Unknow" volcanic eruption of 1809 was attributed to a peak in each core, but it is smaller than 3σ (2.1σ in FK17 and 2.7σ in TIR18), this is why it is neither showed nor discussed in our paper. Had we chosen a threshold of >2σ as in previous studies, these peaks would have been considered as significant.

line 329. Unclear to me what the expression "globally increasing SMB" refers to here?
Now lines 375-376. It meant that the SMB increased on average on the entire period but is affected by a decrease for the last 20 years. We changed "globally" by "on average" to make it clearer.

line 333. It is curious to see the different seasonality for the MSA peaks in these cores. Then I wonder which seasonality was found in the snow pit data? These data are not part of this paper which is unfortunate.
Now line 380. The shallow cores (no "snow pit data" were performed) were not measured for the major ions (only for water stable isotopes) but it should be underlined that only the first 1 to 3 years were missing from the main cores. The published datasets thus allow to study the seasonality of the first years of the main cores records (see graphs here below): it appears that MSA has clear summer peaks for 6 to 9 years before it starts to show migration towards winter layers. This is expected given the known potential for MSA migration (see lines 531-533 from the manuscript) and the low pace of such a mechanism.




line 410. Could melt layers have contributed to the difference in the stable isotope data?
Now lines 457-458. Melt layers impact on climate proxies have indeed raised an increased interest in the ice core community. We thank the referee for attracting our attention on this. We have used our detailed visual inspection of our cores to track down all potential "melt layers": the melt layers identified in our ice cores were usually very thin (<1 mm) and we do not expect these to have had a significant effect on the measured data having a resolution of 5 cm. However, this information was lacking in our text, we thus added a paragraph describing it (lines 206-214). We also looked for the potential impact of the presence of melt layers on the isotopic frequency distribution. The main findings are now summarized in the text and in an appendix with the figures illustrating the frequency distribution (Appendix C). The median $\delta^{18}O$ differences between samples with and without melt layers (0.16 and 0.27 ‰ for FK17 and TIR18, respectively) are insignificant compared to the seasonal isotopic ranges observed in both cores and also well below the difference observed between the IC12 record and the FK17-TIR18 records that discussed in the text (line 410 - now lines 457-458).

Table 1. The error estimates must be included here. I also wonder why suitable volcanic horizons were not used instead of specific years. That would reduce the error estimates.

Now Table 2. We added the error estimates in the table. We could indeed have chosen volcanic horizons as refence years, but we preferred working on specific time windows (200, 100, 50 and 20 years) to detect potential trends (and acceleration of trends) in different "historic" periods (the end of World War I and industrialisation, the World globalization since the 70's and the last 20 years characterized by important changes worldwide). However, since the datasets are published, it will be possible to consider other time periods (e.g. tied by volcanic eruptions) when comparing our SMB records to other datasets with similar tie points in the future.

Section 4.1. I think that the text about the sources for the ions does not belong here. These are more textbook information.
Section 4.2. We agree with the referee on the "textbook-style" of this paragraph. We however think it is important to briefly remind the sources of the species and their potential as proxies for a wider audience. We have therefore split this, reduced and moved the text in the appropriate paragraphs of the section dedicated to each specific proxy.

**Technical comments**

Title: Should be changed. I think it is too wordy and some of these expression does not quite make sense, i.e. an open window…?
We propose "Spatial and temporal variability of environmental proxies from the top 120 m of two ice cores in Dronning Maud Land (East Antarctica)".

line 100-109. This paragraph belongs in the "Introduction".
Now lines 114-125. Since we developed the section 2.1. to be a more general background section on the previous work done related to field expeditions (drilling site selection, AWS data acquired, resulting publications…), we believe it is coherent to leave the ice rises and ice drilling description there.

line150. Abraham et al. (2013):
Now line 167. This has been modified.

Fig. 1. The choice of colors, font sizes and placement for the names of the ice rises are not well suited. The reference for the Derwael ice rise should not be included on the map.
The font size and placement have been modified to improve the clarity of the figure. The colors have been chosen to be "colorblind-friendly" for the different colorblindness types using the Coblis simulator, with a lighter and a darker color and distinct tones. The reference for the Derwael ice rise has been deleted from the map.

line 96. Philippe et al., (2016)
Still line 96. This has been modified.

line 361. "wealthy datasets" it not a correct English word in this context.
Now line 408. We replaced this term by "information-rich" datasets. We thus changed "the wealth of these datasets" for "the richness of these datasets" (line 90).

Fig. 4. It is difficult to distinguish the lines from each other.
We have divided this figure into 2 parts with the SMB not corrected for vertical strain

rates on the left panels and the SMB corrected for vertical strain rates on the right panels. This allows for better visualization of the data, especially since the SMB uncertainties have been added as colored shadings.