NOTE to the Editor and Referees

Thank you for your inputs in this paper which greatly improved the quality of the final manuscript. Please note that the new modifications are in blue in the "track changes" version. The figures have also been adapted.

Referee #2

I would like to thank the authors for their careful consideration of reviewer's comments. I appreciated the responses. I think the additional paragraphs and information made it possible for the reader to evaluate the robustness of the results. It seems to me that the authors are fully aware of the weaknesses of their datasets but have tried to make the best of that. This is a valuable data set, nevertheless.

At this point I only have a few minor comments. I guess there are more technical issues that I might have missed but in general I think the paper is ready to accept.

Thank you so much for revisiting the manuscript. It clearly benefited from your first review. Here below our complementary responses to this second round of comments.

Line 207. How could you see that the potential melt layers less than 1 mm thick were bubble free?

Line 207. You are correct, this is a misuse of the "bubble free" word, we could clearly see that the largest lenses were bubble free, but this is more difficult to ascertain for the thinnest layers. We therefore changed the words "bubble free layers" for "clear ice layers" throughout the main text (lines 206-212) as well as in the appendix (lines 647-658).

Line 111. Change to "Pre-selected"

Still line 111. This has been done.

Line 480-520. Because nssSo4 and MSA are "closely related" I suggest merging these paragraphs to use the space more efficiently.

Now lines 480-524. We agree with the referee that these two proxies are closely related. We thus merged the two paragraphs, even though it does not appear in the text with track changes, but it is in the "final" version of the PDF without track changes.

Line 569. Missing "." after et al. in the two references

Now line 578. Thank you for seeing this, it has been adjusted.

Line 712. Change δ18O to correct format

Now line 726. It has been corrected too, thank you.

Referee #3

This paper presents datasets on accumulation rate, water isotopes and chemistry for the top parts of two ice cores from nearby ice rises. It is a slightly curious paper in that it goes well beyond the material one would expect in a dataset (such as on would find on Pangaea), and yet it doesn't really reach any conclusions. I guess this merely reflects my own uncertainty as to what a data journal accepts – I am happy that the paper allows the authors to describe in detail all the methods by which they obtained their data, including the dating procedure. However the paper appears to promise that papers interpreting the data are still to come – I am a bit mystified what these papers could include other than being repeats of what is here with a little extra speculation. The problem here is that the authors don't really see much they can interpret – the fact that the two cores show different variability and trends means that no large scale conclusions can be drawn, and it is very hard to tell whether recent trends are in any way unusual. Nonetheless, there is a lot of work here, and the data will certainly be useful as food for future compilations of multiple ice cores that may be able to discern underlying trends. I am therefore supportive of the data being published, with the caveat that I don't expect to then be asked to review a later paper that "interprets" the same data with the same methods and the same figures.
The authors have in general answered the main points raised by the initial reviewers. The only substantial issue not addressed is the question of the deeper data that has not yet been obtained: I accept the authors' point that it is already a huge task to produce the data they have. I just ask them, when they do obtain deeper data to ensure that it is easy at that point to find the complete dataset without having to find it twice.

Thank you very much for this insight. We will make sure to create a comprehensive, easy-to-find dataset when deeper data will be measured. We agree that we could have left this data paper at the strict stage of the dataset presentation, but we thought it is also part of a dataset paper to show the potential it has for future interpretation. Differently from the reviewer, we believe there are still analyses to be developed on the observed spatial and temporal variability and we are currently working on it (e.g. the role of extreme precipitation events and the use of back-tracking to explain the observed variability). We hope this will not prevent the reviewer from a future revision, if it happens to be so.

I have a few points where the authors should consider further edits.

Line 33. "The Antarctic ice sheet's future contribution to global sea level rise… is difficult to predict, largely because of the uncertainty and variability of the surface mass balance (SMB)". This is simply not correct – the main reason why the future sea level contribution from Antarctica is hard to predict is well understood to be due to uncertainties in ice dynamics, MISI, MICI, etc as co-author Pattyn has many times written. Please rephrase.

Line 35 of the "track change" file. We agree that we have been "over-focusing" on our wn topic. We changed the word "largely" to "partly" to take this remark into account.

Line 198 and Table 1. I am surprised not to see detection limit as an analytical parameter here. Maybe the concentrations are all well above the DL, in which case just say so to remove doubt.

Now line 204. We added a sentence stating that, indeed, the concentrations we measured are well above the detection limit calculated for each ion.

Fig. 2 – Just a comment that I agree with setting a limit of 3 sigma. Just look how many values are 2 sigma below the mean to see why 2 sigma is not a reliable indicator of a volcanic peak in these noisy coastal cores. Unfortunately this does mean that it's hard to identify clear volcanic peaks – in future work the use of S isotopes to confirm the volcanic nature of some of the peaks used to tie the dating would be worthwhile. (Nothing requested, just a discussion point from me to the authors).

We agree that 2 sigma is not reliable in these coastal cores. Thank you for the suggestion of using S isotopes, this will certainly be taken into account for future work.

Fig 4 and lines 335-340. The y axis is mislabelled in panels a,c,e: what you are plotting in thise figures is the layer thickness in ice equivalent, NOT the SMB. Only after the correction do you get to SMB. For that reason I actually see no purpose to panel e, nor to giving numbers in the text for "SMB without correction" which is some weird average of a trending layer thickness and is not SMB in any sense. Please re-cast this text and figure y-axes at east.

Now lines 349-359. You are right, we should not label the panels (a), (c) and (e) with "SMB", we changed the y-axis for "annual layer thickness" in the figure but also in the text. However, we think it's important to keep the panel (e) since it allows a comparison with panel (f) and clearly shows the impact and necessity of correcting the annual layer thicknesses for vertical strain rates, which is rarely done in the literature presenting longer-term accumulation records from ice cores. We added a sentence to make this clear in our manuscript (lines 353-355 "This comparison highlights the crucial need to correct the annual layer thicknesses with the vertical strain rates when analyzing potential long-term trends in ice core records.").

Fig 5. When you discuss MSA seasonality, we would expect to see it peak in summer near the surface and winter deeper down. You even discuss this in your response to reviewers but I don't think you clarify that here. Wouldn't it be better to show separately the seasonal cycle for the top and then separately for a section deeper down where movement has taken place?

To highlight the migration of MSA from peak in summer to peak in winter previously discussed with the reviewers, we have added a few words in our main text (line 536). However, discussing seasonality in detail is not the aim of the dataset paper (see comments above) so we leave it to the reader to have a look at the variation of seasonality based on the available MSA records and age models.

Line 485. "thorough discussion of the processes involved should however be built on fluxes data rather than concentrations". For a site with such high acc rate this is simply not correct. Most chemistry will be wet deposited meaning that the concentration (not the flux) is reflecting the atmospheric concentration. Only for sites with low acc rate, where dry deposition dominates does the flux become important.

Now lines 524-526. Thank you for rising this point, we deleted the mentioned section as well as the sentence in the following paragraph.

Line 500. Regarding the mechanism of MSA movement you may like to quote the excellent paper by Osman et al (Osman, M., Das, S. B., Marchal, O., and Evans, M. J.: Methanesulfonic acid (MSA) migration in polar ice: data synthesis and theory, The Cryosphere, 11, 2439-2462, doi: 10.5194/tc-11-2439-2017, 2017).

Now lines 535-545. Thank you for suggesting mentioning this very interesting paper and their findings. We added a few sentences summarizing these in our manuscript. The reference has also been added (lines 868-869)