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.

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 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.

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).

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.

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?

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.

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).