Review for Winton et al., Climate of the Past: *Drivers of late Holocene ice core chemistry in Dronning Maud Land: The context for the ISOL-ICE project*

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This manuscript outlines the development of the age scale and associated chemistry records for the ISOL-ICE project. The manuscript looks at standard kinds of atmospheric and index correlations to understand the climatology driving the site specific characteristics. The manuscript is clear and readable, and well referenced. I have a few concerns (outlined below) which will likely require a bit of extra analysis, however I recommend the manuscript for publication once these are addressed. I have selected major revisions, but it is somewhere between minor and major revisions as what I suggest to do is not too onerous and I doubt will change the conclusions hugely, but will make the methods more defensible and easier to interpret.

Major comments:

I think the abstract needs a final sentence or two explaining where this work fits toward the eventual quantification of natural variability in ozone. At the moment the opening of the abstract doesn't follow to the results and conslcusions section of the abstract very well.

What proportion of years in the accumulation record are less than 6 cm (the minimum to preserve the nitrate seasonal cycle cited at line 50)? What effect will low accumulation years have on the ability to interpret the nitrate record for ozone variability? Also, it might be good to explain a bit more the need for at least 6 cm per annum to preserve the nitrate seasonal cycle, and that Akers et al., 2022 cite 4-20 cm. Finally, its not clear to me how a sampling resolution of 0.3 to 0.5 years provides a robust seasonal cycle – that is only 2-3 samples per year. How will this affect how you interpret the nitrate record? Also I suggest you change sentence at line 50 to 'higher rates' or similar rather than 'increases'. Reading further – perhaps I have misunderstood the sampling resolution for different analytes, so maybe this could be clarified with a table (see comment below).

I'd recommend a schematic in the introduction that outlines how Winton et al. 2020 and this work sit in the larger frame of eventually being able to robustly interpret the stable isotopic composition of nitrogen in snow and ice as a TC0/ozone layer proxy. At the moment, its not totally clear from the abstract and the introduction what the prior work has done, what this manuscript does, and what the future work will be, and it would be good to have that clarified at the start of the manuscript.

Can you expand a bit on volcanic dates – specifically the delayed 'arrival date' of Huaynaputina (2 years) and Kuwae (8 years)? This seems a bit odd with an error of 3 years.

Lines 75-77 – but how do you do this faithfully with only 2-3 measurements per year? You need some caveats here that your sample resolution limits your ability to interpret sub-annually here. Perhaps detail what sample resolution you get in the surface cores compared to at depth. Now that I have got to the results, I see at lines 218-219 I might have missed detail on what the sample resolution is for different analyses? If so, perhaps explain with a table (as I suggest above) the sample resolution for each type of analyste both at the surface and at depth so the reader can clearly see where you get seasonal / sub-annual resolution and where you don't.

It isn't clear to me why you used a calender year (Jan-Dec?) mean for the ENSO indices – is that correct? If so, this would split any ENSO event in half and partition it to separate years. This doesn't make sense to me from a climatological perspective. II'd strongly suggest using a more appropriate seasonal split of either April-March or May to April (if you want to use a full 12 month mean). You could also use June to Feb. Alternatively, you could use June-November, as the ENSO oceanic and atmospheric anomalies that are relevant to high southern latitudes commence around May / June, and intensify through to early summer, by which stage any event is well established. E.g. see Crockart et al., 2021 for an example of different seasonal splits. Lines 210-214 – this may well need a re-write once you have more appropriate seasonal boundaries for the Niño 3.4 and SOI indices. For table A3 please add in caption what monthly boundaries you end up using for calculating the indices.

Figure 4 and Discussion. A dot on each map for the ice core site would be helpful to orientate the reader. As you state, there is not a huge difference between the magnesium and sodium maps (because the records are very highly correlated, especially winter spring). Do you need to present both? At the moment its hard to interpret any differences, especially for winter spring as they may be artefacts of analysis more than anything. Line 322 – what about orographic factors and relatively short sea salt aerosol lifetimes of a few days? Can you prove transport across the continent, versus say short-term episodic inputs of high sea salt loads that can't be teased out in your 6 month means? I'm not suggesting you have to prove this, but I think the across continent statement is a big one and lacks evidence in this context, especially given the statement about air mass / accumulation source at lines 459-461. Would that mean that any cross-continental sea salt trabsport was dry deposited?

Minor comments:

In the abstract, I'd recommend separating into two sentences the sentence about the development of the age model, and then the snow accumulation and ice chemistry records and correlations. These are two separate (fairly major) steps and deserve a sentence each.

Line 27 – over the last two decades.
Line 66 – independently derived – do you mean via layer counting and volcanic horizons?
Line 279 – figure 4b? also, what is an 'offshore low' and an 'offshore high'?
Line 369 – sea salt
Line 390 – what do you mean by a change in the DMS oxidation pathway?