Review of "Air Mass Origin Effects on Antarctic Snow Isotopic Composition: An Observation and Modelling Study" by Petteni et al.

The authors present a study analyzing the stable water isotopic composition of snow samples collected in East Antarctica. These measurements are complemented with back-trajectory calculations, ERA5 reanalysis data, and output from two models (LMDZ6iso and a snow metamorphism model). Together, these datasets are used to address the key question of what climatic information is preserved in the stable water isotopic composition of firn and ice cores, particularly in low-accumulation regions such as the East Antarctic Plateau. This question has been addressed by a number of studies in the previous years highlighting the relevance of the topic presented in this manuscript. The manuscript fits into the aims and objectives of the EGUsphere, but I suggest revisions before publication.

Overall Comments

The study addresses two major aspects: i) the analysis of different air mass origins, and ii) the influence of post-depositional effects, particularly sublimation, on the isotopic signal of surface snow. However, only the first aspect is reflected in the current title. I recommend revising the title so that both themes are represented equally.

Overall, the manuscript is generally well written but would benefit from thorough proofreading before final submission. Figure labelling is inconsistent: in several cases, panels are labelled "left" and "right" in the captions but referred to as "a" and "b" in the text. Please adopt a consistent style throughout the manuscript (e.g., "a, b, c…"). In addition, several figure captions lack sufficient detail. For example, Figure 1 does not explain the abbreviations for the locations shown on the left panel, and the right panel is missing proper axis labels (is the x-axis in kilometres?). Please also cite any mapping software used (e.g., Quantarctica).

In the Results section, some passages read more like discussion, while in the Discussion section new results and figures are introduced. I recommend ensuring that results and discussion are clearly separated.

At multiple points, the manuscript states that "significant" differences or impacts are observed, but no explanation is given as to how significance was determined. Please clarify the methods used to assess significance and discuss the findings in relation to uncertainties. Similarly, model uncertainty is not considered when comparing observations with model output (e.g., Fig. 11).

Figure 11 underpins many of the conclusions, but several issues remain:

- Model uncertainty is not addressed.
- ERA5 precipitation uncertainty, which is well known, is not discussed.
- The "better agreement" between the return-sample variability and the LMDZ6iso output is claimed but not quantified; moreover, the modelled $\delta^{18}O$ values show no variability.

- Wind-driven redistribution, which is a key process on the East Antarctic Plateau, is not mentioned. This process could substantially influence accumulation patterns and isotopic signatures and should be discussed alongside sublimation.

I agree with the authors that sublimation may have a strong effect on the isotopic composition of snow. However, other depositional and post-depositional processes, especially wind redistribution, should not be neglected. I recommend expanding the discussion to include these processes and their potential impact on the results.

Specific comments (minor)

I only have two minor comments to the abstract.

- In l.17, it is written that depositional and post-depositional effects lead to large unceratinties in the use of stable water isotopes as proxy in Antarctica. The unceratinties of d18O as a proxy is strongly depending on the specific location in Antarctica with East Antarctica or low accumualtion regions having a much larger uncertainty than high accumulation areas in West Antarctica for instance. Please be more specific here.
- Secondly, when mentioning that LMDZ6iso captures the spatial variations accuratley, it would be nice to see a number showing how well the model captures the variability. It will strengthen the statement.

- l. 42f.: One sentence is not a full paragraph. Please incorpoarte this sentence into the following paragraph.
- L. 49: pleae add an s → distilation pathways
- L. 56f.: What is the difference between sublimation, water vapor and vapor diffusion?
- L. 66: I don't understand "that fell over the past decade". What do you mean here?
- L. 70: are you using all data from Masson-Delmotte (2008) or only a subset (e.g. the East Antarctic Plateau)?
- L. 77: please add brakets → Casado et al. (2021)
- Chapter 2.2:
    - Please carefully check the language in this chapter.
    - Please provide more details on the snow samples.Did you take several samples at a location or only one each time? Did you always take a surface and a bulk sample at the same location? Why do you have 85 surface samples but only 52 bulk samples?
    - We know that different labs show discrepancies when measuring the same samples. Have you performed an independent quality controll or something similar between both labs?
    - What is your uncertainty for d-excess values? It would be interesting to have a number for d-excess as well, not only $\delta^{18}O$ and δD, to compare this to d-excess variations.
- Chapter 2.4:

    - l. 130: I don't agree to call the output of ERA5 *snow accumulation*. Considering the mentioned depositional and post-depositional modifications, I would refer to *snowfall* or *snow precipitation* provided by ERA5.
    - L. 133: can you be more specific what you mean with *couple of months*?
- L. 161f.: Are you considering densification for the bulk samples? This might be relevant for the bulk samples on the plateau, considering that they contain up to 15 years of snowfall.
- L. 167: cinetic → kinetic
- L. 176f.: outburn/outboud → outbound; free-precipitation → precipitation-free
- L. 182: Looking at Fig. 2 the DC plot, I also see lines that are colored in blue and orange. Would that imply that not all 100% are originating from the Indian Ocean?
- L. 195: few → please be more specific if possible
- Table 1: can you mark Section 1 and 2 in the map of Fig.4 ? Are relationships with $R^2 > 0.5$ tested for significance?
- L. 254: Did you test for significant differences?
- L. 267ff.: For me, this reads already like discussion. You can consider to move this part to the discussion in Section 4.
- L. 285: how did you test for significance?
- L. 326 and Fig. 9: All plots with the new data show an R^2 of 0.9 but 0.8 is mentioned in the text. Please correct this.