Review of "Surface processes and drivers of the snow water stable isotopic composition at Dome C, East Antarctica – a multi-datasets and modelling analysis" by Ollivier et al.

The authors present an extensive analysis of existing and new data of stable water isotopes from precipitation, surface and subsurface snow samples collected next to the Dome C research station in Antarctica over the period from 2017 to 2021. They complement this data with a modelling approach – the Snow Isotopic Signal Generator (SISG) – which tries to simulate the observed isotopic composition with different input parameters. Additionally, they analyse the output from an isotope-enabled GMC (ECHAM6-wiso).
The authors put a lot of effort into combining different isotope data sets from different campaigns. This data is very valuable for the community. I appreciate the dedication of this team to collect and measure these samples and to provide an in-depth analysis to the community. Such data and the knowledge that the community gains from it are a necessary step to better understand the post-depositional processes on ice sheets and the climatic imprint in ice cores.

<u>Specific comments (major)</u>

The manuscript reads lengthy in parts and explains very detailed the observations of precipitation, surface and subsurface snow isotopic composition as well as the modelling approach. In parts it is difficult to follow and to remember all details from the comparisons of different observations and the modelling results. The readability can be improved by shortening parts. Additionally, chapter 4.1 (*Contribution of precipitation to the snow isotopic composition*) shows even more new figures and information. To me, (part of) this section provides many results and does not necessarily discuss them. The authors might consider moving this (or part of this) part to the results section and focus on the comparison of their findings to other studies as well as on implications of their findings in the discussion (as they do from chapter 4.2 onwards).

Additionally, I would like to see comments or a chapter about the limitations of the study. Some limitations are mentioned in some parts, but I am missing a comprehensive analysis of the shortcomings and the reliability of their dataset and, especially, the assumptions on which their study is based on (e.g. isotopes represent local air temperature, scaling of ERA5 precipitation, large surface features and intermittent accumulation at their study site, etc).

- Figure 8, for example, show a large variability between consecutive samples. This can be related to a large spatial heterogeneity in surface features (e.g. Picard et al., 2019), likely causing variability and noise in surface and subsurface snow isotopic compositions. How do you explain these differences? How realistic is it that these two samples, 50m apart, sampled twice a week, represent the local weather/climate, considering the reported surface processes and post-depositional processes (e.g. snow erosion and re-deposition)?

- Sublimation is often discussed as a reason to alter the surface snow isotopic composition significantly throughout the 5-year period. The authors discuss the difference between surface and subsurface snow isotopic composition, but do not use or apply the mentioned model by Wahl et al. (2022) to simply estimate the depth of snow affected by sublimation. Moreover, they mention (l. 695ff.) that the location at Dome C on the East Antarctic Plateau has a very intermittent snow accumulation in time and space (which is well-known). Besides presenting the new datasets, this study does not provide more insights into the mechanism of post-depositional processes affecting the stored climatic information in stable water isotopes in such environments. Extending their simulations with e.g. diffusion and/or sublimation (models for both do already exist, (e.g. Münch et al., 2016; Wahl et al., 2022)) or simply estimating the effect of these processes, would help to quantify the role of these processes and to further disentangle ambient noise, post-depositional processes, and the climatic imprint in stable water isotopes in precipitation as well as in surface and subsurface snow.
- ECHAM6-wiso is compared to the observations and the SISG simulations. What is the benefit of including a GCM here? Can you provide any suggestions for the modelling community how to improve the representation of isotopes in isotope-enabled GCMs? Why not using a smaller model, such as Wahl et al. (2022) or Dietrich et al. (2023) developed when they proved that they can reproduce observations?

<u>Specific comments (minor)</u>

- L. 59: low*er* atmosphere
- L. 81: water stable isotopes -> stable water isotopes
- L. 81f.: It is written “snow surface and subsurface” and later “surface snow and subsurface snow”. Please consistently use one term throughout the manuscript.
- L. 118: Is the uncertainty determined with an independent quality control sample?
- L. 118: two standard deviation*s*
- L. 118 and l. 142: First, the uncertainty is given by two standard deviations and later, in l. 142, by one standard deviation of a quality standard. Why are you using different measure for uncertainty? It seems that the first uncertainty is lower when using one standard deviation, especially for dD (0.7 ‰/ 2 = 0.35 ‰). Does this influence the d-excess presented in this study?
- L. 151: The time step is one hour. How is a 1-hour average calculated from a 1-hourly spaced timeseries?
- L. 160: Why are you not averaging the data to 1-hour time steps as well (as the AWSIT data)?
- L. 167 and 169: Which test was used to test for significance?
- L. 191: flux~~es~~
- Table 1: The sampling rate of the CALVA measurements is given as 30s. Here, you are referring to the used averages. “Measurement time step” is in my opinion not the correct term here. Maybe you can rephrase it.

- L. 287: Which relationship is used here to calculated $d^{18}O$ and dD from atmospheric temperature?
- L. 460: How do you explain the fact that the precip-weighted means are higher than then arithmetic means?
- L. 469f.: You write that “the summertime” difference between observations and ECHAM6-wiso “decreases when the precipitation d18O is weighted by the precipitation amounts”. To me, this only accounts for December and January while the overall difference increases. Can you explain why? What is your conclusion here?
- Figure 7:
    - Why are you only reporting the RMSE and not the correlation as well?
    - I assume that the data shown in the scatter plot are the same as in Figs. 6a and 6d. It seems that ECHAM6-wiso suggests more days with precipitation than the observations do. How are you handling this? Are you accounting for these or are you ignoring these days?
- L. 528f: Are these the equations used in the ECHAM6-wiso model itself or are these empirically calculated?
- L. 811ff.: My understanding of a proxy system model might be different than yours. Following Evans et al. (2013), for instance, a *proxy system model may be defined as the complete set of forward and mechanistic processes by which the response of a sensor to environmental forcing is recorded and subsequently observed in a material archive*. Wahl et al. (2022) and Dietrich et al. (2023) did a great job in coming up with statistical approaches to e.g. calculate expected isotopic variations in surface and subsurface snow from sublimation. However, I see their models rather as an input to more sophisticated (proxy system) models.

References:

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Picard, G., Arnaud, L., Caneill, R., Lefebvre, E., and Lamare, M.: Observation of the process of snow accumulation on the Antarctic Plateau by time lapse laser scanning, The Cryosphere, 13, 1983–1999, https://doi.org/10.5194/tc-13-1983-2019, 2019.

Wahl, S., Steen-Larsen, H. C., Hughes, A. G., Dietrich, L. J., Zuhr, A., Behrens, M., Faber, A. -K., and Hörhold, M.: Atmosphere-Snow Exchange Explains Surface Snow Isotope Variability, Geophysical Research Letters, 49, https://doi.org/10.1029/2022GL099529, 2022.