General comments:

This manuscript presents a valuable dataset of isotopic compositions of daily precipitation at Concordia station for ten years from 2008 to 2017. The authors did detailed analysis of meteorological conditions, the water isotope data, and model-data comparisons. The dataset is useful to evaluate model performance, investigate climate controls on water isotopes in Antarctic precipitation, and quantify impacts of post-depositional processes on ice core records. However, the structure of the content can be more concise, and the added scientific values of this manuscript, especially compared to Stenni et al. (2016) that presents the first three-year data, are not very clear. Therefore, major revision is suggested before publication.

R: We are grateful to reviewer #2 for his/her comments and for providing input that is useful for improving our manuscript. We believe we addressed the reviewer's comments and highlighted the novelty and the added value of this new study compared to previous published literature. Please see our point-by-point answers for each comment hereafter. The author's comments are in normal text, the referee's comments are in italic.

Major comments:

1, *Titles of many subsections are too short to be informative. E.g. "2.2 Analytical ", "3.5 Correlations".*

R: We have reformulated some subsection titles as follows:

2.2 Analytical → Water stable isotopes analysis of precipitation samples

2.3 Weather data → 2.3 Weather observations and reanalysis data

2.4 iGCMs → Isotope-enabled general circulation models (ECHAM5- and ECHAM6-wiso)

3.2 Water stable isotope data → Water stable isotope data and its correlation with temperature

3.5 Correlations → Correlations between water stable isotope data and meteorological parameters

2, *The authors stressed in many place that the dataset is 'unprecedented', "unique", or "of extreme importance". However, it is not clear what additional values do this manuscript bring to the research community compared to Stenni et al. (2016). For example, when this dataset is applied to evaluate model performance in Section 4, does it add more confidence in identifying model bias? Does it help to identify a direction to improve the model simulations or for further studies?*

R: According to our answer to reviewer #1, the dataset presented in this manuscript has several strengths. Indeed, 10 years of precipitation data led to more robust results and statistical interpretation: e.g. the inter-annual variability of the isotopic signal in precipitation is currently better framed in this study than in the previous paper (Stenni et al. 2016). About the model performances, we describe now why this dataset can be useful, in general, for benchmarking iGCMs (introduction section). More specifically, the comparison between observations and ECHAMx-wiso is possibly biased because of the adopted microphysics scheme. The data provided in the present study may help to improve cloud parameterization through model-data coupling in d-excess (microphysics scheme, ice nucleation rates…).

The above considerations are now better discussed in the modified "Introduction" section (see also the answer to the referee #1 regarding the scientific novelty).

3, The authors presented analysis on both weighted and unweighted monthly or annual values. Can the authors elaborate which one is more suitable in which conditions?

R: The weighted and unweighted data and the temporal averaging time strongly depend on the lifetime of the atmospheric processes considered. Generally, weighted data are preferable when they are compared or related to other variables or when comparing different periods. For instance, when comparing intervals in ice core records it becomes clear that each layer archived in the ice is representative of the amount of snow accumulated over a period. This is also true when considering that the precipitation can be distributed unevenly over the year. This impacts the delta-T relationship as well. For example, if all the precipitation in a year occurs in summer, the resulting delta values will be biased towards summer temperatures and will be different from those measured in another year when the precipitation is concentrated in wintertime, although the annual mean temperature could be identical.

Since the dataset presented here may be useful to several scientists for different purposes, we prefer presenting all possible combinations of results to extend as much as possible the usefulness of our data.

We added a sentence in the new paragraph of the "Introduction" section (Lines 137-141) that is already modified for answering to referee #1: "To this end, the data used in this study are presented as both weighted and unweighted for the precipitation amount."

We also added a short sentence in Section 3.2, when we presented some weighted delta values for the first time (Line 320-325):

"The weighted and unweighted data and the temporal averaging time strongly depend on the lifetime of the atmospheric processes considered, a fact particularly important when dealing with precipitation in continental Antarctica, which is unevenly distributed throughout the year (Fujita and Abe, 2006; Turner et al., 2019). Indeed, the weighted $\delta^{18}O$ and $\delta^{2}H$ values are thought to be better correlated with snowfall temperature (Masson-Delmotte et al., 2008; Servettaz et al., 2023)."

And why do the authors weight the variables using total precipitation from ERA5 (Line 296), rather than observed precipitation amount?

R: Unfortunately, we do not have those data for the whole period 2008-2017. The quantification of the precipitation amount collected on the benches was made in 2008-2010 and then started again in 2017.

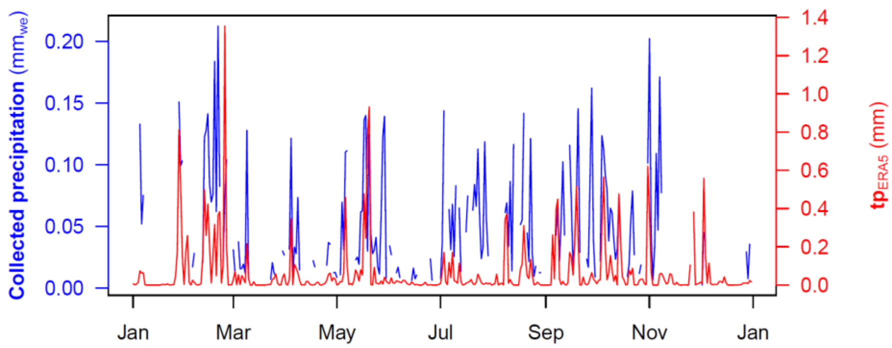
Thus, for the purpose of the present study, we chose to use ERA5' "tp" data. As we discuss in the next point, there is quite a good agreement between the observed and modeled precipitation amount over the year 2017.

As reported in section 2.1, sample collection occurred daily. Thus, the observed precipitation amount analyzed in this study is only a qualitative value that might be related to both fresh snowfall as well as wind-drifted snow and possibly also affected by wind erosion. We better elucidated this concept in section 2.3 (Line 211-213):

"It is worth noting that given the qualitative nature of the observed accumulation, the tp_{ERA5} parameter has been used in this study as representative of the precipitation amount of the observed daily snow samples.".

If there is no corresponding precipitation in ERA5, how do the authors do the weighting?

As reported in section 2.1, monthly and annual-averaged weather data were computed only over days with available samples. Hence, the observed precipitation-weighted relationship between isotopes and weather data is only available at monthly and annual timescales. As also reported in the answers to reviewer #1, in the figure below we present a comparison between the collected precipitation and the tp_{ERA5} for 2017. Although this time series is only available for one year, it is possible to see a qualitative good agreement between the two datasets.



Since there is a general good agreement between ERA5 and experimentally-collected amount of precipitation, we are confident that the use of ERA5 data is suitable for the aims of this study.

4, The authors did extensive correlation analysis between different variables. However, different variables might be correlated because of their common correlation to another variable. For example, the correlation between deuterium excess and temperature discussed from line 388 might be related to their correlation with d18O. It a partial correlation analysis can confirm this point, what is the point of regression analysis between deuterium excess and temperature?

R: We understand this point and agree with the referee. However, since the goal of this manuscript is to provide as much information as possible for other studies, we chose to also add the correlation between d-excess and temperature. In addition, even though a fraction of the correlation between d-excess and temperature can be explained by the correlation between $\delta^{18}O$ (and $\delta^{2}H$) it is also important to highlight that d-excess is more sensible to other variables.

5, The structure and content of the conclusion section can be more concise. For example, the sentences starting from line 527 and line 533 can be shortened, and the sentence starting from line 538 can be removed.

R: Done. The sentences were shortened and removed, respectively.

Minor comments:

Line 22: "AWS", introduce the full form.

R: Done.

Line 24: 3.5 ‰/°C for δD/TAWS.

R: Done.

Line 46: "although occurring progressively over successive condensation events between the initial evaporation and the final deposition areas. " This formulation is very strange. What is occurring?

R: Done. We deleted part of the sentence.

Line 47: "Consequently, the different sensitivity of the empirical δ-T relationship in East Antarctic ice is generally poorly constrained with respect to other regions". It is not clear what this sentence wants to express.

R: Sentence modified. We deleted "with respect to other regions", which was not clear.

Line 143: "samples in the sealed bags"?

R: Done. Sentence modified.

Line 268: "daily pattern" not "diel pattern"

R: Done.

Line 284: "during the days with collected samples" and "during the sampling days" are duplicated.

R: Done.

Line 485: Are the scatter plots in Fig.6 based on daily values? If the precipitation occurs on different days in simulations and observations, how are they matched together?

R: The scatterplots report only the days with both experimental and modeled precipitation data. This is now also reported in the figure caption.

Line 537: "this could explain the occurrence of negative d-excess values in this season." Do you have any supporting evidence for this statement?

R: We have no evidence from our study, but this was already observed by other authors in Antarctica, e.g., Casado et al. (2021) and Ritter et al. (2016). We added these two references supporting this evidence.

Line 545: "mean monthly averages".

R: Done.

Line 550: Is this based on annual data? Are ten annual data points enough to evaluate long-term trends?

R: No, the linear trend is calculated over the monthly averaged data, thus over 120 points. This was reported in subsection 2.5 "Data processing".

Line 554: How can sublimation leads to lower slope in LMWL?

R: Ritter et al. (2016) have shown that sublimation processes can cause fractionation in the presence of very porous snow (as in this study, we are not dealing with ice) at the snow–air interface. Moreover, this latter study also reports that "it is possible that snow would behave more like a liquid than like a solid in this respect and would fractionate". Moreover, Sokratov and Golubev (2009), as well as Stichler et al. (2001), previously showed that sublimated snow samples lie on a line with a slower slope than the GMWL.

Stichler, W., Schotterer, U., Fröhlich, K., Ginot, P., Kull, C., Gäggeler, H. and Pouyaud, B., 2001. Influence of sublimation on stable isotope records recovered from high-altitude glaciers in the tropical Andes. Journal of Geophysical Research: Atmospheres, 106(D19), 22613-22620.

Sokratov, S.A. and Golubev, V.N., 2009. Snow isotopic content change by sublimation. Journal of Glaciology, 55(193), 823-828.

Line 559: "The high d-excess values found in winter, as well as its seasonal amplitude, are mostly due to the extremely low condensation temperature rather than to changes in moisture origin." Do you have supporting evidence for this conclusion statement?

R: This was already shown by Touzeau et al. (2016) and other papers (Craig, 2961, Uemura et al., 2012, etc.), discussing the effects of the decrease of the slope of the meteoric water line at very low condensation temperature. We improved the discussion in the main text, also following the comments of referee #1 (see section 3.4, Lines 444-456):

"Indeed, as previously reported by Craig (1961) and Uemura et al. (2012), any process which deviates from the average δ^2H-$\delta^{18}O$ slope 8 (GMWL) can affect the d-excess parameter. To this end, we calculated the logarithmic version of d-excess to assess whether the observed δ^2H-$\delta^{18}O$ of precipitation better fit a curve rather than a straight line (Uemura et al., 2012), as in the canonical definition of d-excess following the GMWL. The logarithmic transformation effectively reduces the sensitivity of the observed d-excess to observed $\delta^{18}O$ (slope from -1.35 to -0.58) and almost flattened the sensitivity of the observed d-excess to observed δ^2H (slope from -0.18 to -0.03). Such a smaller sensitivity between δ values and d-excess for the logarithmic transformation highlights first that special attention should be paid when dealing with extremely depleted precipitation since the linear approximation introduced by the GMWL does not hold anymore. This is especially true when attempting to extrapolate any relationship between precipitation d-excess in extremely cold regions and the evaporative conditions of warmer moisture sources. Second, different processes might be involved in the precipitation sample before the collection, such as mixing with wind-drifted snow and sublimation (Ritter et al., 2016), which could translate into a smaller $\delta^{18}O$ vs δ^2H slope for precipitation samples.".