General comments

This paper reports the last ten years of precipitation isotope observations at Concordia Station, Antarctica. The data set is extremely valuable and should be published. The results are mainly based on correlation analysis and numerous comparisons with reanalysis data and isotopic models.

R: The manuscript was revised taking into consideration your suggestion. We would like to thank the referee #1 for his/her valuable contribution that improved the overall quality of our manuscript. The author's comments are in normal text, the referee's comments are in italic.

On the other hand, I felt that the scientific novelty is unclear.

R: As also pointed out by referee #2, the dataset presented in this manuscript has several strengths. First, the dataset covers 10 years. This is an unprecedented series of data for daily precipitation in Antarctica and therefore the dataset used here led to more robust results and statistical interpretation. For instance, the inter-annual variability is currently better framed than in the previous paper (Stenni et al., 2016). Second, the current dataset is also more robust for evaluating the isotope-enabled GCMs performance. Under this view, we compared our experimental data with ECHAMs-wiso, but also different models can be evaluated against this dataset. Moreover, our observations are based on precipitation rather than surface snow. Third, the interannual variability is probably better captured in this extended dataset because it is more likely to include atmospheric processes acting on scales larger than 3 years. Fourth, these data can be used as input for isotopic models investigating post-depositional processes of surface snow, firn, and ice core records. Fifth, the basic statistical results, e.g., meteoric water lines, seasonal patterns, weighted values, etc., presented in this manuscript are scaled over different periods, such as daily, monthly, and inter-annual scales. These data may therefore be useful to researchers working on different scientific areas, such as atmosphere, climate and weather.

The aforementioned key points are added to the main text as separate items in order to better elucidate the novelty and added values of this manuscript and dataset (Lines 124-141):

"This dataset represents an unprecedentedly long record of precipitation experimentally measured in East Antarctica with several potential advantages for glaciological and palaeoclimatological studies:

- a better framing of the inter-annual variability of the isotopic composition of precipitation with respect to previous works. Indeed, 10 years of observations more likely include atmospheric processes acting on scales larger than 3 years (Stenni et al., 2016);
- a more robust evaluation of the up-to-date isotope-enabled general circulation models (iGCMs) performances, comparing 10 years of experimental data with outputs from different models. For instance, the data provided in the present study may help to improve cloud parameterization through model-data coupling (e.g., microphysics scheme, ice nucleation rates);
- the experimentally collected precipitation data can be used as input for isotopic models investigating post-depositional processes of surface snow, firn, and ice core records, since the precipitation isotopic composition is the input signal of the atmosphere-snow surface and subsurface systems;
- the basic statistical results, e.g., meteoric water lines, seasonal patterns, weighted values, etc., presented in this study are scaled over different periods, such as daily, monthly, and inter-annual scales. These data may therefore be useful to researchers working on different scientific areas, such as atmosphere, climate and weather. For instance, the data provided in this study may be useful to better constrain the δ-T

thermometer. To this end, the data used in this study are presented as both weighted and unweighted for the precipitation amount.”.

Although the text is clearly written, the results of the correlation analysis are described in detail, and what the results mean is not discussed well. It should be clarified: "What do we learn from this observation?"

R: Part of the answer to this comment is reported in the previous point. We agree that a better discussion of the dataset may be included. This way, we improved the data description and the discussion of the results.

We amended some sections, improving the data discussion: in particular, we included new discussion in Section 3.2 (delta vs. temperature), Lines 344-349, regarding the delta-T relationship, Section 3.3 (LMWL) Line 389 and Section 3.4 (d-excess) Lines 444-456 regarding the relationship between d-excess and delta values.

Lines 344-349: “These regressions parameters show a small variability when separately computed on different years (range 0.32-0.63‰/°C). Thus, the $\delta^{18}O$-temperature slope was almost constant during the 2008-2017, except in 2011 (0.32‰/°C); when excluding 2011, the range was 0.4-0.63‰/°C. This slope range is even smaller than the confidence interval of the interannual slope [0.39;0.83] (Table 1). On the other hand, the slope range variation over 10 years at Concordia station seems to be smaller than the spatial variation (0.6-0.91‰/°C), as reported in Masson-Delmotte (2008).”.

Line 389: “This decrease directly impacts the d-excess values (see next section).”.

Lines 444-456: “Indeed, as previously reported in 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. ”.

I think seven figures in the main paper, 34 supplementary figures, and five tables are too much.

R: We strongly believe that the figures and tables reported in the main text are proportional to the information provided and are needed to support the main findings of the research. On the other hand, we agree that the number of figures and tables in the supplement could be reduced. This way, we have removed 9 figures in the SI, namely former figures S16-S19, S21-S23, S30, S31.

The manuscript should be revised to reflect the following comments before publication.

R: Please see our point-to-point replies below.

Major comments

(1) L.110-115: I think the issue of the discrepancy between the three years of data (Stenni et al., 2016) and the Antarctic spatial slope is not fully discussed in the main text. The temporal d18O/T slope in this study is smaller than the spatial slope. The cause of this difference and its implications (especially its impact and implications on the recent controversy about temperature reconstruction of the Antarctic ice cores, e.g., Buizert et al., Science, 2021) need to be discussed.

R: We agree with the referee's comments. This comment refers to a long-lasting, controversial, and still unsolved question in paleoclimate reconstructions from Antarctic ice cores. For example, the slopes between the delta values and temperature have been shown to be highly variable considering different time intervals (Casado et al., 2017). The paper by Buizert et al (2021) reconstructed the magnitude of the last glacial maximum cooling using borehole thermometry. A deeper analysis of this issue could be impossible, since we are not considering the post-depositional processes that can act on snow, thus impacting on the isotope-temperature relationship.

We added the reference suggested by the reviewer with a short discussion in the introduction section in order to give a wider view of this issue (Lines 113-119):

"Hence, the slopes between the delta values and temperature have been shown to be highly variable considering different time intervals and locations (Casado et al., 2017). On the contrary, by reconstructing the magnitude of the last glacial maximum cooling using borehole thermometry, Buizert et al. (2021) showed a large variability of the δ-temperature slope considering different ice core locations. Generally, this latter study reported quite higher δ-temperature slopes (range 0.82-1.45 ‰/°C) than studies using water isotope composition. This represents a long-lasting, controversial, and still unsolved question in paleoclimate reconstructions from Antarctic ice cores.".

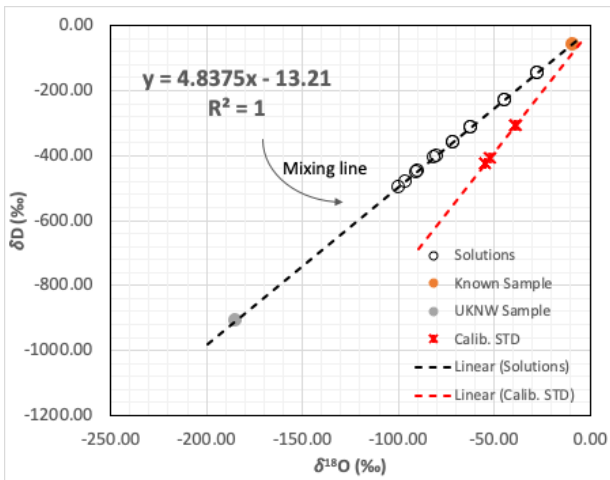
However, we also need to stress that this manuscript considers precipitation rather than surface snow, firn or ice.

Reference

Casado, M., Orsi, A., and Landais, A.: On the limits of climate reconstruction from water stable isotopes in polar ice cores, Past Glob. Chang. Mag., 25, 146–147, https://doi.org/10.22498/pages.25.3.146, 2017.

(2) L155-160: Most precipitation isotopic ratios in this study are lower than that of SLAP. In other words, it has been extrapolated. Maybe future studies will be conducted to evaluate the effects of extrapolation, so please describe how many WSs were used (2 or more) and the isotope ratio of each WS.

R: We agree that the isotopic composition of precipitation for some specific events is more depleted than the SLAP composition and hence also more depleted than our most-negative laboratory standard (~ -424‰ for δ^2H). However, we tested the CRDS analyzer linearity by diluting, with precise weighting, an extremely depleted water (δ^2H ~ -900‰) with tap water (δ^2H ~ -56‰). The following figure shows that the instrument response is extremely linear, capturing all the dilution stages with a coefficient of determination that is almost one. Moreover, the true δ^2H value of the tap water ("known sample") differs only by ~1.4‰ from the calibrated value of tap water using "Calib. STD" line (Calib. STD line was build using standards with δ^2H ranging from -424‰ to -306‰, shown as red crosses in the plot).



Hence, we are confident that the calibration line for precipitation analysis, as defined by the internal WSs for each analysis run, is valid also outside the span of the WSs and the error on the extrapolated depleted precipitation events is negligible.

Continuing the question related to WSs, we used two standards (one about -300‰ and the other -400‰) for each analysis run to build the calibration line. A third standard (values around -400‰) is used for QC. Note that all the internal standards are calibrated regularly against VSMOW-SLAP.

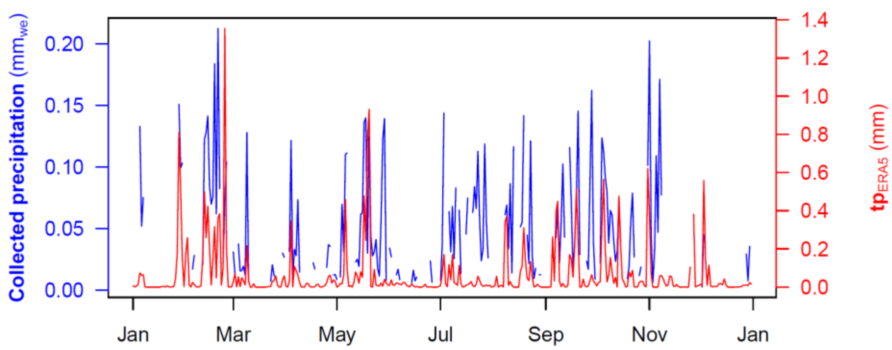
We edited the text accordingly as follows (Lines 171-175):

"Two working standards were used during each run to build the calibration line and a third working standard was used for quality control. All the working standards are in the range of very negative values as found in Antarctic snow and were regularly calibrated against VSMOW-SLAP. Internal laboratory tests have shown the linearity of the instrumental response outside of the calibration interval."

(3) Fig. 1: The authors explained that the isotope ratios were measured only when there was enough snowfall to collect data. Specifically, what is the lowest accumulation (mm/day or more)? Based on Fig. 1, the number of isotope data varied considerably from year to year. In particular, 2010 and 2017 are large, and 2015 and 2017 are also somewhat large. Does this mean that the number of days of snowfall events varies significantly from year to year? Is this consistent with the number of days that snow was collected and the amount of snowfall in the reanalysis data?

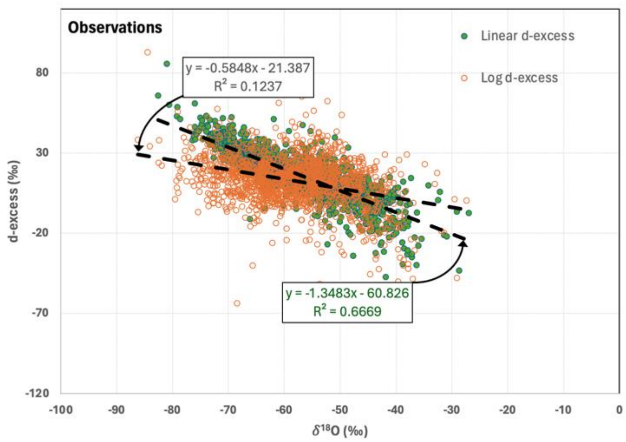
R: In this dataset, the lowest accumulation values that we observed during collected precipitation (in 2017, which is the only year with these data) ranged between 0.0024 and 0.2126 mm water equivalent (average 0.053 mm water equivalent). However, this value slightly changed from year to year depending on the operator in the field. This is particularly true during the winters when the harshest conditions occur. As observed by the referee, this uncertainty is also shown by the different number of samples collected year by year.

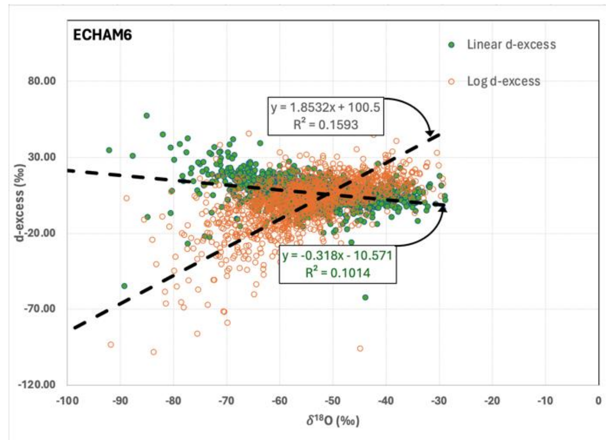
The following figure shows the comparison between the collected precipitation and the ERA5' total precipitation (tp_{ERA5}) for 2017. Although the time series for the collected precipitation is only available for one year (2017), there is a good qualitative agreement between the two time series.



(4) Regarding the discussion on the relationship between d-excess and d18O: Since the slope of D-18O is 6.6 (fig. 3), it is evident by simple mathematics that calculating d-excess with a slope of 8 would be inversely correlated with d18O (Figs. 1 and 5 and others). Relatedly, in Fig. 6, there is a large discrepancy between the model and observed d-excess, but this can also be understood because the slope of the model's D-18O is close to 8 (7.66). However, the authors briefly mention the possible decrease in MWL slope (L. 412-413). I think it is necessary to discuss this point more, as the authors repeatedly showed d-excess vs d18O relationship. Specifically, the logarithmic definition of d-excess (Uemura et al., Climate of the Past, 2012; Markle and Steig, Climate of the Past, 2022) has been proposed to alleviate this problem. At least the impact of the logarithmic definition on these results could be added to the discussion.

R: The logarithmic definition of d-excess effectively reduces the sensitivity of the observed d-excess to observed $\delta^{18}O$, yielding a relationship which is like the one predicted by ECHAM6, as shown in the figures below. Similarly to Uemura et al. (2012), the sensitivity of the observed d-excess vs $\delta^{2}H$ (slope = -0.18, R^2=0.53) is almost flattened after the logarithmic transformation (slope = -0.03, R^2=0.05) (data not shown).





However, it is also worth noting that the logarithmic definition of d-excess on ECHAM6 data produces a spurious sensitivity between $\delta^{18}O$ and d-excess (same for $\delta^{2}H$). Hence, the logarithmic transformation of d-excess better fits the delta for very depleted precipitation but it cannot be used to perform a direct comparison with the model. We argue that some of the reasons for the better fit of the log d-excess to the observations is due to the occurrence of sublimation of precipitation, mixing with wind-drifted snow and/or other post-depositional processes, which are translated into a smaller $\delta^{18}O$ vs $\delta^{2}H$ slope of precipitation samples.

We believe that we better highlighted this aspect in the revised version of the manuscript (see also previous answer; 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 $\delta^{2}H$-$\delta^{18}O$ 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 $\delta^{2}H$-$\delta^{18}O$ 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 $\delta^{2}H$ (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 $\delta^{2}H$ slope for precipitation samples."

(5) If some of the Supplementary figures are to be deleted, the scatterplots of correlations (Fig. S16-23) would be a candidate.

R: Done. See previous points.

Technical comments

L63 "the emprical d-T relathionship valid" -> "…is valid…"

R: Done.

L75: I think it is better to use some publication (white paper or perspective) instead of a URL as a citation.

R: We add the paper by Parrenin et al., 2017 (The Cryosphere)

L246 "Figure SI2" -> « Figure S2 »»

R: Done.

L260 "may had led"-> "may have led"

R: Done.

L411 "explained with" -> "explained by"

R: Done.

L569 "worth to mention" -> "worth mentioning"

R: Done.

References:

Buizert et al., Science, 2021, https://doi.org/10.1126/science.abd2897

Markle and Steig, Climate of the Past, 2022, https://doi.org/10.5194/cp-18-1321-2022

Uemura et al., Climate of the Past, 2012, https://doi.org/10.5194/cp-8-1109-2012

R: We added two references in the main text (Buizert et al., 2021 and Uemura et al., 2012).