REVIEWER #1

The revised manuscript has improved considerably. Although the focus remains on fundamental statistical analysis of the data set, I think it is well worth publishing.

Since the central value of this paper is to provide the precious observational data that will be used in future studies, then providing the data in an easy-to-use format is crucial. However, at the moment, the isotope datasets cited in "Data Availability" are not available for review; reading the Data Availability text alone, it reads as if each data set is uploaded separately. Secondary use would be laborious if this speculation is correct (the authors provide the isotopic data separately). Thus, it is recommended that the data used for the figures in this paper, at least the temperature, the precipitation weighted, and the respective averages used (e.g., daily, monthly, annually), are published in one file in an easily accessible form. It will surely improve the value of this paper.

R: We have prepared an Excel file with all the required data. We have uploaded this file as a new dataset in Zenodo. The dataset includes 3 main datasheets:

1. DAILY data with date, daily dataset for isotopes ($\delta^{18}O$, $\delta^{2}H$, d-excess), starting and ending date and hour, temperature and RH from the AWS (aws.temp and aws.RH, respectively) and the ERA5 temperature and total precipitation (era5.t2m and era5.tp, respectively);
2. MONTHLY data with months, monthly averages for for isotopes ($\delta^{18}O$, $\delta^{2}H$, d-excess), monthly averages for isotopes weighted for AWS temperature ($\delta^{18}O$.aws.temp, $\delta^{2}H$.aws.temp, d-excess.aws.temp), monthly avergages for for isotopes weighted for ERA5 total precipitation ($\delta^{18}O$.era5.tp, $\delta^{2}H$.era5.tp, d-excess.era5.tp), monthly averages for air temperature and RH from the AWS (aws.temp and aws.RH, respectively) and the ERA5 temperature and total precipitation (era5.t2m and era5.tp, respectively);
3. ANNUAL data with years, annual averages for isotopes ($\delta^{18}O$, $\delta^{2}H$, d-excess), annual averages for isotopes weighted for AWS temperature ($\delta^{18}O$.aws.temp, $\delta^{2}H$.aws.temp, d-excess.aws.temp), annual averages for isotopes weighted for ERA5 total precipitation ($\delta^{18}O$.era5.tp, $\delta^{2}H$.era5.tp, d-excess.era5.tp), annual averages for air temperature and RH from the AWS (aws.temp and aws.RH, respectively) and the ERA5 temperature and total precipitation (era5.t2m and era5.tp, respectively).

The link and DOI of the datasets are provided in the main text (data availability section).

REVIEWER #3

General comments:

Thanks for the prompt responses on previous comments. I still have some concerns about the scientific values of this manuscript and the structure.

Major comments:

Based on the abstract, the main scientific findings are the estimated temporal slope between delta and temperature and the evaluation of ECHAM simulations. However, it is unclear how these findings relate to existing literature and what are the added values. For example, previous studies also reported spatial slopes, is the value derived here similar or different, and what does it mean for ice core interpretation? Previous studies also compared ECHAM5 and 6, what are the added insights here?

R: We thank the referee for the comments. We already replied to his/her concerns in the previous answers regarding the added values of our dataset. The spatial slope is not discussed here but we only pointed out the difference of our temporal slopes from the previous paper (Stenni et al., 2016), which just accounted for 3 years. Moreover, we also discussed the difference between our data and the spatial slope reported by Masson-Delmotte et al. (2008), see Lines 112-115. We also deeply discussed this question further in the introduction.

Previous studies did not compare ECHAM5-wiso or ECHAM6-wiso with precipitation data collected at Concordia station during a 10 year period, which are provided here. Goursaud et al. (2018 CP, https://doi.org/10.5194/cp-14-923-2018) used ECHAM5-wiso data only on 3 years precipitation data in Concordia. Thus, the comparison provided in the present paper is more robust because spanning over a longer period (10 years).

The introduction may not be informative and concise enough for a non-expert in water isotopes.

R: We modified the introduction following the suggestions of the previous referees, lengthening it for explaining the scientific novelty.

It is suggested to omit not very relevant stuff to highlight the importance of this study. For example, Line 70: Why is it necessary to list the age of ice cores?

R: We added this information for readers that might be non-expert in ice core science and to highlight the importance of water isotope records obtained so far in East Antarctica.

Line 78: It is true that low accumulation/low temporal resolution and blowing-winds might undermine water isotope interpretation, but why is it important to this study? Line 82: It is true that post-depositional processes affect ice core records. But since this manuscript does not investigate post-depositional processes, is it necessary to list all the processes?

R: The isotopic composition of precipitation is indeed the input signal for postdepositional processes that need to be taken into account for paleoclimatological studies. Furthermore, the precipitation collected in the present study may also be subject to secondary processes and as such these potential processes must be highlighted. We are confident that this is clearly explained in the main text.

Line 96, it is suggested to add a topic sentence at the beginning to inform the readers what to expect in this paragraph.

R: Done. We have edited the sentence to make it clearer.

The conclusion seems to be longer than necessary. For instance, Line 582: Is it necessary to introduce wind-drifting in the conclusion?

R: The drifing snow represents one of the limitations of the precipitation sampling and in our opinion is important to highlight it in the conclusions.

Line 587: This sentence may be more suitable for Data and methods section rather than Conclusion.

R: The same as before.

Minor comments:

Line 46: Does local temperature drive precipitation isotopes? Or is it just an empirical relationship resulting from their common correlations with condensation temperature?

R: We agree with the referee, but the condensation temperature is not easy to estimate. In addition, the site temperature is usually derived for paleoclimatological reconstructions, also considering that a relationship between condensation and surface temperature is observed in Antarctica.

Line 589-599: These are simply summaries of main statistics. No implications are discussed for ice core community.

R: We agree that the Conclusion section is long and that some data provided here were already presented in the main text. However, we prefer to include some main results in the Conclusion section. Therefore, the paragraph has been shortened by only keeping key findings.

Regarding the implications for the ice core community, we have already discussed this topic in the Introduction section, also considering the long-lasting and still not resolved question on the sensitivity of isotopes to temperature over different time scales.

*"*The precipitation isotopic composition and the surface temperature showed a marked seasonal variation over the investigated period with a moderately high linear relationship at the daily scale. The relationship becomes stronger when using monthly averages. The $\delta^{18}O$ (and $\delta^{2}H$) to T_{AWS} slope of 0.52‰/°C (and 3.52‰/°C) computed on the daily values slightly increases to 0.59‰/°C (and 3.9‰/°C) when computed over annually averaged data, although no statistically significant ($p<0.05$) long-term linear trends were identified during the 2008-2017 period."

Line 600-618: The texts on LMWL and model evaluation should be evaluated. Implications of the LMWL results should be mentioned.

R: This was already done in the results and discussion and is reinforced here in the conclusions. We are confident that the discussion over this topic does not need improvements.

Line 608: Why is there a positive bias? Is it common among other GCMs?

R: This positive bias in inland Antarctica was already found in Goursaud et al (2018) for ECHAM5-wiso, as also reported in the manuscript, and is consistent with the warm bias observed for temperatures. We are not evaluating other GCMs in this paper, thus we cannot provide a complete answer to the point raised by the referee. We are already planning to compare our data to other isotope-enabled GCMs in future publications.