General comment

Stable water isotopes ($\delta^{18}O$) measure in polar regions like Antarctica are traditionally used to reconstruct past surface air temperature (SAT). However, this relationship is influenced by many parameters like surface elevation, air mass sources or sea surface conditions. Moreover, this relationship varies spatially and over time, and has not been investigates using historical simulations (1850 onward), yet. To tackle this issue, Goursaud Oger et al. investigated the SAT – $\delta^{18}O$ relationship during the historical period in Antarctica using an ensemble of historical climate simulations (1851-2004) performed with the isotope-enabled HadCM3 general circulation model. The found strong SAT and precipitation temporal trends during this period, but only weak trends for $\delta^{18}O$, meaning no significant relationship between SAT and $\delta^{18}O$ over one third of Antarctica. They conclude that the decoupling between $\delta^{18}O$ and SAT occurs primarily because of the impact of autumnal sea ice loss during the simulated warming.

The analyses and idea are simple (in a good way) and well written, making the article easy to read and follow. To better quantify the influence of parameters other than temperature on stable water isotopic composition of ice in Antarctica is an important topic for paleoclimate community, and this article represents an additional valuable contribution to that discussion. This article is worthy for publication in CP after addressing the minor points detailed below, including more in-depth analyses of the circulation of air and moisture masses induced by changes in sea ice.

We thank the reviewer for the time and relevant comments he made. These contributed to improve our manuscript. We also hope that the changes we brought will answer your expectations.

Major comments

In terms of analyses, the warm – cold anomalies and the seasonal effects are very interesting and well investigated. On the other hand, in the abstract and the conclusion, the authors talk about the involved variations in moisture transport and air mass intrusions due to sea ice transport. I would expect deeper analyses on this aspect, and not just some general statements in the conclusion section. For example, the winds patterns are shown in Figure 4 but not cited in the text.

We are aware that our study is not exhaustive. More analyses could be made to deepen the explanation of processes behind our results. Especially, a complete investigation on the atmospheric circulation change could be lead, adressing the effect of the different teleconnections, through different modes impacting Antarctica (e.g. as done by Marshall and Thompson (2016) for SAT).

Meanwhile, we looked at the impact of the SAM. Our results were given in Appendix F. We show that the HadCM3 reproduces the impacts of SAM on SAT and P reported in previous studies (Clem et al., 2016; Fogt et al., 2020), i.e. colder and drier conditions in a positive SAM. For $\delta^{18}O$, the HadCM3 simulates a depletion in most areas of the Antarctic continent while the SAM is in a positive phase, but these results are associated with relatively low correlation coefficients with means of -0.26±0.11 over the Historical period and -0.27±0.12 for the period 1950 – 2004. We thus conclude that our simulations cannot establish a robust link between the SAM and the Antarctic precipitation weighted $\delta^{18}O$. This result is supported by the diversity of $\delta^{18}O$ measurements from precipitation and firn/ice cores on different Antarctic locations (e.g. Vega et al., 2016; Kino et al., 2021; Servettaz, 2022; Dreossi et al., 2023). Moreover, it was shown that SAM impacts are different with the ENSO phases (Wilson et al., 2016), and that other modes affect Antarctic climate (e.g. Shields et al., 2022).

Finally, we suggest that the impact of the atmospheric circulation on Antarctic precipitation weighted $\delta^{18}O$ for the Historical period will be strickly tackled in another future study. We thus completed Section 5 ("Drivers"):
"The dynamic processes behind the sea ice extent induced δ18O changes are complex and multiple. Although the Southern Annular Mode, leading mode of the atmospheric variability in the Southern Hemisphere, might explain part of these δ18O simulated changes (Appendix F), a more comprehensive study might investigate the impact of the atmospheric circulation changes."

In the conclusion, l.291, we replaced:
"We indentify three processes [...]" by *"We suggest [...]"*, meaning that an extended study is necessary to check the atmospheric processes at the origin of our simulated results.

In section 2.1, the model and the setup of simulations should described a little bit more. Six historical simulations have been performed. How the authors make these simulations a little different from each other? With different initial conditions? By changing the value of a parameter? For the HadCM3 model, it should be stated from this section that this is a atmosphere-ocean coupled model.

We only altered the initial conditions, starting each simulation a year apart. We clarified these features and detailed the applied protocol, as asked by the second reviewer l.79 to 90 :

"*Here, we use the Hadley Center Atmosphere-Ocean general circulation model (HadCM3; AOGCM), to run six transient Historical simulations. HadCM3 is a version of the coupled Atmosphere-Ocean UK Met Office climate model (Pope et al., 2000; Gordon et al., 2000). The model is equipped with stable water isotopes (Tindall et al., 2009). Its horizontal resolution is 3.75° × 2.5°, and there are 19 vertical levels (Pope et al., 2000; Gordon et al., 2000; Tindall et al., 2009). The setup of the Historical simulations is described in (Schurer et al., 2014), and follows the recommendations of the third Paleoclimate Modelling Intercomparison Project (PMIP3; Schmidt et al., 2011)(PMIP3; Schmidt et al. 2012). Each simulation is forced with time-varying orbital, solar, volcanic, land-use and well-mixed greenhouse gas forcing. Changes in orbital parameters were calculated following (Berger, 1978). Volcanic forcing is that described in (Crowley et al., 2008). The solar forcing follows (Shapiro et al., 2011). Changes in CO2, N2O and CH4 were set following the PMIP3 standard (Schmidt et al., 2011). Changes in the abundances of 6 Halocarbons were prescribed following (Tett et al., 2007). Changes in land-cover were prescribed by reclassifying the Global land cover reconstruction developed by (Pongratz et al., 2008). Each of our simulations were only altered by starting each simulation a year apart.*"

Also, parameterization relative to isotopes with sea ice should be described? How sea-ice - atmosphere exchanges are taken into account in the model for the isotopes? How ocean free vs. sea ice covered areas considered for the calculation of isotope concentration in surface water vapor? Is there any sublimation of snow on sea ice? With a fractionation effect? Or is there nothing specific coded for the isotope sides, meaning that isotopes are influenced by mainly by changes in air mass and moisture transport, only.

Sea ice is represented in the model and is calculated by the oceanic component. Ice sheets and sea ice in the model are initialised with a δ18O value of -30 and -2 ‰ respectively. The isotope component of HadCM3 ignores the small fractionation associated with sea ice processes and thus makes the approximation that sea ice melting/formation is non-fractionating (Holloway et al. 2016; Tindall et al., 2009; Pfirman et al., 2004). Because of the slow diffusivity of heavy isotope species within ice, sublimation from sea ice is also assumed to be non-fractionating (Jouzel, 1986).

HadCM3 requires a small water and isotope flux to represent iceberg calving and close the hydrological and isotope budgets. Since the isotope flux was not calculated directly from the model, a very small drift in ocean isotopes remains, similar to the case for salinity (Pardaens et al. 2003). The drift is not large enough to affect the results of the century-scale simulations shown here.

The figures need to be improved. The font size of the titles, letters, equations, and labels are really too small in all the figures. The figure 2 needs big improvement. See for example the figures 2 and 5 in the recent paper from Servettaz et al. (https://doi.org/10.5194/tc-17-5373-2023). Moreover, it would help to note somewhere the name of the different regions of Antarctica, and to which colors they do correspond. In the current state of the paper, it is hard to follow which region is where for people not familiar with Antarctica geography.

We increased the fontsize of written text (label, etc) in the figures. We also relocated the subplots of Figure 2 and added the names of the regions. We hope that these improvements will help the reader to follow which region is where.

Note that while recomputing our regional $\delta^{18}O$ trends, we realised that we had made a mistake not specifying those that were not signficant (p-value>0.05). As a result, over the last 50 years simulated by HadCM3, only three regions, the Indian, the Weddell and the Dronning Maud Land coastal regions display significant linear relionships.
We thus adapted Figure 2 by shading in grey non significant trends and completed the caption:
"Grey shaded rows correspond to non significant relationships (p-value>0.05)."
In the text, we removed l. 185 to 187:
"Over the last fifty years, a part from the Victoria Land where a very weak trend appear, other regions present weaker trends with correlation coefficients now ranging from 0.11 to 0.38 while gradients increase with values ranging from 0.03 ‰ per decade for the WAIS and the plateau, to 0.14 ‰ per decade for the Weddell coast."
And instead we have written l.185 to 187:
"Over the last fifty years, only three regions, the Indian, the Weddell and Dronning Maud Land coastal regions keep on displaying significant δ18O trends, that double or more compared to the Historical period, with gradients of 0.08, 0.08 and 0.14 ‰ per decade respectively."
Also, we adapted the comparison with the results from Casado et al. (2023),
"They found gradients with the same range of values, from 0.09 ‰ for the Indian coast, to 0.19 ‰ for the Weddell coast, while they found significant relationships where we do not, for time windows varying from 40 to 65 years. Note that for most of the regions, the significance of our simulated relationships disappear when taking time windows lower than 75 years (Appendix D). This could be explained either by a two low simulated anthropogenic variability, as suggested by (Casado et al., 2023), or a change of the drivers on δ18O."

Minor comments

Lines 5, 30, 58, 68, and others: I would replace "water stable isotopes" by "stable water isotopes"
Done.

3rd paragraph of the introduction: Cite also Servettaz et al., TC, 2023 (https://doi.org/10.5194/tc-17-5373-2023)
We added the citation l.51 *"These other controls include: changes related to atmospheric dynamics, such as changes in the synoptic and seasonal nature of precipitation (van Ommen and Morgan, 1997; Krinner and Werner, 2003; Jouzel et al., 2003; Sime et al., 2008; Servettaz et al., 2023)"*.

Lines 46-47 and 92: “Antarctica2k” without space like in Stenni et al. (2017). Define A2k for Antarctica2k here, too.
Done.

Lines 50-53: please add the study from Buizert et al., Science, 2021 (https://doi.org/10.1126/science.abd2897) et Cauquoin et al. CP, 2023 (https://doi.org/10.5194/cp-19-1275-2023), except if you want to focus on warm period only (in that case, some references like Werner et al. (2018) should be removed). If you choose the latter option, please precise at the beginning of the paragraph that you talk here only about warm periods. :
Done, l.48 to 53:
“These other controls include: changes related to atmospheric dynamics, such as changes in the synoptic and seasonal nature of precipitation (van Ommen and Morgan, 1997; Krinner and Werner, 2003; Jouzel et al., 2003; Sime et al., 2008; Servettaz et al., 2023) and air mass sources (Landais et al., 2021), various impacts from changes in Antarctic ice sheet morphology (Holloway et al., 2016; Werner et al., 2018; Buizert et al., 2021; Goursaud et al., 2021), and sea ice variability (Holloway et al., 2018; Cauquoin et al., 2023).”

Line 101: please describe briefly the ECHAM5-wiso simulation (AGCM at T106 resolution nudged to ERA40/ERA-Interim). And cite Werner et al. (2011, https://doi.org/10.1029/2011JD015681).
Following a comment of the second reviewer, we replaced the analyses that were extracted from the ECHAM5-wiso simulation published in Stenni et al. (2017), by new analyses processed using an ECHAM6-wiso simulation communicated by Alexandre Cauquoin.

Compared to ECHAM5-wiso, the performance of the water isotopes in ECHAM6-wiso (Stevens et al., 2013, Cauquoin et al., 2019) is clearly improved. This is attributed to: (i) a modification of the supersaturation parameters ; (ii) that the kinetic fractionation at the evaporation over oceans is now assumed to be independant of the wind speed in order to better represent the d-excess versus deuterium relationship from the Antarctic Snow reported by Masson-Delmotte et al. (2008) ; and finally (iii) that the sublimation processes now accounts for the isotopic content of snow over sea ice. Based on the evaluation of global simulations against ERA-interim and ERA5 reanalyses, Cauquoin and Werner (2021) report that the nudging does not significantly change the simulated isotope values, while increasing the resolution generally improves the performance of the simulations. However, the evaluation of the simulated water stable isotopes in precipitation over Antarctica remains rather qualitative (Figure 1, Cauquoin and Werner, 2021).

Having obtained this new model output data from the newer version of ECHAM, we performed the same analysis as previously applied to ECHAM5 and HadCM3. This does indeed resolve the discrepancy between the models – ECHAM6-wiso and HadCM3 (in the newer ECHAM6 version) have equivalent SAT-δ^{18}O surface air temperature relationships.

We thus made the following changes in the text:
- In section 2 (“the data and methods”), l.102 to 110:
“Our Historical SAT–δ18O linear relationship at the regional scale are compared with the regional slopes and correlation coefficients that we computed from the AGCM ECHAM6-wiso equipped with water stable isotopes (Cauquoin et al., 2019). The water stable module of this last generation of the model ECHAM was updated compared to its predecessor, especially (i) the supersaturation parameters, (ii) the kinetic fractionation at the evaporation over oceans, now assumed to be independent of the wind speed in order to better represent the d-excess versus deuterium relationship from the Antarctic Snow reported by (Masson-Delmotte et al., 2008), and finally (iii) the sublimation processes now accounting for the isotopic content of snow over sea ice. Here, we use a simulation run at a T127L95 resolution (0.9° x 0.9° horizontal resolution and 95 vertical

levels) and nudged towards the ERA5 reanalyses (Hersbach et al., 2020) over the period 1979 – 2022 Cauquoin and Werner (2021)."

- In section 4 ("Temperature versus δ18O relationships"), l.202:
"To enable a consideration of model dependency, we also compare our Historical ensemble against a nudged ECHAM6-wiso simulation (Table 1)."

- In section 4.2 ("Stability over the Historical period and model dependancy"), l.230 to l.237:
"Interestingly, the ECHAM6-wiso simulation and the last 50 years of our HadCM3 simulation display similar SAT-δ18O relationships. ECHAM6-wiso simulates slightly stronger relationships with a mean correlation coefficient difference of 0.04, while gradients tend to be slightly higher in HadCM3 with a gradient difference of 0.13 ‰/°C. The only notable differences are for Dronning Maud Land and the Indian coast with stronger relationships and higher gradients simulated by HadCM3 (Table 1).
Thus, whilst it is unclear whether the nudging of ECHAM6 towards ERA5 reanalysis, the model resolutions or differences in sea ice behaviours, are the main reason for these discrepancies, it is clear that simulated temperature versus δ18O relationships have low but significant uncertainties. These need to be considered, both regionally and for the most relevant climate state, before being undertaking any inferences of past temperatures using isotopes measured in ice cores."

In section 6 ("conclusions"), l.284 to 286:
"Interestingly, we find similar but slightly weaker SAT-δ18O correlations and slightly higher gradients compared to ERA5 –nudged ECHAM6-wiso simulations at the regional scale."

We also updated Table 1.

Line 115: which reanalysis data?
We precised : « We analyse our simulations against available observations and reanalysis data (e.g. the Climate Forecast System Reanalysis and the the National Centers for Environmental Prediction reanalyses 2) »

Line 158: Medley and Thomas (2019)
Done.

Lines 170-171: sounds strange (strongest for one place and highest for another place)
The correlation coefficients (strength of a linear relationship) and the slope of the relationship (low or high relationship) have no link. One linear relationship can be high (high slope) but weak (low correlation coefficient) which make it not consistent, or it can be low (low slope) but strong (strong correlation coefficient) so the two variables are strongly linear linked through a slow which is low. I hope it makes it sense.

Line 179: Rephrase the beginning of the sentence to avoid the double use of "values".
Done : « They found gradients with the same range of values »

Line 191: ECHAM5-wiso
Done.

Lines 226 and 227: remove the brackets for the two references.
Done

Lines 228-231: maybe the atmosphere-ocean coupling, including sublimation of snow on sea ice?
You are true that differences between model physics could also explain the differences so we

completed l.242 to 245:

"Thus, whilst it is unclear whether the nudging of ECHAM6 towards ERA5 reanalysis, the model resolutions, the model physics or differences in sea ice behaviours, are the main reason for these discrepancies, it is clear that simulated temperature versus δ18O relationships have low but significant uncertainties."

Line 236: add a reference to Figure 5.

Done

Line 244: the largest (no capital T).

Done.

Lines 244-246: say explicitly the months.

For clarity, we added in the above line (243) :

"displaying seasonal anomalies (for the winter season, e.g. from June to August, and for the summer season, e.g. from December to February)"

Lines 253: there is no figure 5h.

We corrected to Figures cf.

Line 278: It's not ERA4 but ERA40 and ERA-Interim.

Following the changes related to the ECHAM simulations, this sentence was removed.

Lines 279-282: see first major comment. I think more analyses of wind patterns (for example) to demonstrate these conclusions would improve the paper.

Please refer to our response to your first major comment.

Line 294: See second major comment. These specificities of HadCM3 should be said before in the paper.

It was added as asked in your second major comment.

Line 295: higher (no capital H).

Done.

Figure 1: increase the font size (for all figures), including for the equations, make the average and linear regression curves thicker, use something like a_all, a_recent, r_all, r_recent to differentiate the two equations in each plot.

We increased the fontsizes throughout the manuscript. However, we could not add something like " a_all, a_recent, r_all, r_recent", as there is not enough space on one line and that it would hide part of the plot on multiple lines.

Figure 4: state in the legend that these anomalies relative to annual mean. The wind fields are visible. Put more space between the arrows and draw them thicker and bigger. There is a typo “>” in the last line of the legend.

Done.

References.

Cauquoin, A. and Werner, M.: High-Resolution Nudged Isotope Modeling With ECHAM6-Wiso: Impacts of Updated Model Physics and ERA5 Reanalysis Data, J. Adv. Model. Earth Syst., 13(11), e2021MS002532, doi:https://doi.org/10.1029/2021MS002532, 2021.

Clem, K. R., Fogt, R. L., Turner, J., Lintner, B. R., Marshall, G. J., Miller, J. R. and Renwick, J. A.: Record warming at the South Pole during the past three decades, Nat. Clim. Chang., 10(8), 762–770, 2020.

Holloway, M.D., Sime, L.C., Singarayer, J.S., Tindall, J.C., & Valdes, P.J. (2016). Reconstructing paleosalinity from δ18O: Coupled model simulations of the Last Glacial Maximum, Last Interglacial and Late Holocene. *Quaternary Science Reviews*, *131*, 350-364.

Jouzel, J. (1986), Multiphase and multistage condensation processes Hand-book of Environmental Isotope Geochemisty, vol. 2,The Terrestrial Environment, pp. 61–112, Elsevier, New York

Marshall, G. J., & Thompson, D. W. (2016). The signatures of large-scale patterns of atmospheric variability in Antarctic surface temperatures. *Journal of Geophysical Research: Atmospheres*, *121*(7), 3276-3289.

Pardaens, A., H. Banks, J. Gregory, and P. Rowntree (2003), Freshwater transports in HadCM3, Clim. Dyn., 21(2), 177–195.

Pfirman, S., W. Haxby, H. Eicken, M. Jeffries, and D. Bauch (2004),Drifting Arctic sea ice archives changes in ocean surface conditions,Geophys. Res. Lett.,31, L19401, doi:10.1029/2004GL020666.

Tindall, J. C., P. J. Valdes, and L. C. Sime (2009), Stable water isotopes in HadCM3: Isotopic signature of El Nin õ–Southern Oscillation and the tropical amount effect,J. Geophys. Res.,114, D04111, doi:10.1029/2008JD010825.

Turner, J., Connolley, W., Cresswell, D., & Harangozo, S. (2001). The simulation of Antarctic sea ice in the Hadley Centre climate model (HadCM3). *Annals of Glaciology*, *33*, 585-591.

Vega, C. P., Schlosser, E., Divine, D. V., Kohler, J., Martma, T., Eichler, A., ... & Isaksson, E. (2016). Surface mass balance and water stable isotopes derived from firn cores on three ice rises, Fimbul Ice Shelf, Antarctica. *The Cryosphere*, *10*(6), 2763-2777.