Responses to comments of Referee #1

Thank you for the time and thoughts on this manuscript. We appreciate these comments.

This study quantifies the evaporative sources of Antarctic precipitation for a preindustrial simulation with the ECHAM6 model. The spatial and seasonal variability of the evaporative moisture source contributions to Antarctic precipitation at different elevations and for precipitation events of different intensities are discussed along with the typical moisture transport pathways to the ice sheet. Different moisture source conditions relevant for setting the evaporation flux and impacting water isotope records as well as the anomalies of these source conditions during uptakes contributing to Antarctic precipitation compared to climatology are investigated. Finally, shifts in moisture source regions associated with variations in the Southern Annular mode are analysed. The chosen methodological approach is based on an innovative scaled-flux water tracer approach implemented in ECHAM6 similarly as initially proposed by Fiorella et al. 2021 using the iCAM6 global circulation model. I much enjoyed reading this interesting and innovative paper, which is well-written and has nice and captivating figures. For me personally the scientific highlights of this paper are i) the anomalously strong storminess at the moisture source of humidity feeding Antarctic precipitation and ii) the shifts in source locations and conditions observed with the SAM. On a methodological side, I find the scaled flux water tracer implementation very attractive and the documentation of their implementation in ECHAM6 in Appendix A useful. I particularly like the comparison done with the traditional numerical tracer setup using pre-specified evaporative regions (Appendix A).

I have only one “easy” general comment, which is related to the fact that this study is based on preindustrial simulations. This is all fine per se, but the authors should make this much clearer in their introduction, in which anticipated future changes with global warming are mainly discussed. Currently, for me as a reader there is a mismatch between the use of a preindustrial simulation and the knowledge gap uncovered in the introduction with sentences like “It is not yet clear how SAM variations and associated changes in moisture flux tracks will impact precipitation across Antarctica”. One cannot address this question with a pre-industrial simulation, but of course the mechanisms linking variations in SAM with changes in moisture sources and transport pathways can be studied very well with a preindustrial simulation and the authors do it elegantly. Even more importantly, the fact that the authors use a simulation with preindustrial climate conditions matters, when they compare their moisture source decomposition with other studies such as the Lagrangian study by Sodemann and Stohl, 2009. I would therefore suggest to smooth out this mismatch by pointing more at the need for a better process understanding in the introduction, and remind the reader of the different time period covered in their simulations when comparing their results to previous studies.

We fully agree with the referee that model simulations across different climate periods are required while applying water tracers in the context of global warming. As the referee pointed out, we focus here on the mechanisms that imprint on moisture sources. These will be further tested and evaluated in subsequent simulations. In our revised manuscript, we stressed in all relevant sections that the results are based on preindustrial simulations.

Though, we believe that insights obtained from water tracers in this study through a preindustrial climate simulation is valuable for the knowledge gap mentioned in the introduction.

Minor comments:

1) "novel" in the title is a bit unspecific, could be more precise

Response: Thank you. It is changed to 'innovative'.

2) L. 1: "for gaining insights into **past and future** polar, and global changes"

Response: Changed.

3) L. 1: "changes" in what exactly? Environmental changes?

Response: Yes, changed to "environmental changes".

4) L. 7-10 and results section about Fig. 4: "The tendency of poleward vapour transport to follow moist isentropes means that central Antarctic precipitation is sourced from more equatorward (distant) sources via elevated transport pathways than coastal Antarctic precipitation. We find however this tendency breaks down in the lower troposphere, likely due to diabatic cooling." I find this analysis based on Fig. 4 interesting but also very puzzling. Fig. 4 shows a zonally averaged mass-weighted vertical cross section of the source latitude of water vapour. This Fig. is discussed in the result section in a moist isentropic framework, from which we would expect moisture from a given latitude to follow the moist isentrope corresponding to the surface equivalent potential temperature from that latitude. I am a not so sure about what Fig. 4 tells us exactly:

To me it seems like the moist isentropic framework is a very crude approximation to the typical transport pathways and doesn't provide more than a justification for the fact that precipitation falling on the plateau tends to come from further equatorward than coastal precipitation. Similarly, it most likely explains why precipitation in the warm season comes from further equatorward than in the cold season. But other than that, when looking at Fig. 4, I see mainly deviations from the moist isentropic framework. On the Antarctic plateau the highest elevations don't pool their moisture from the most equatorward/warmest sources. And even more generally, in the upper troposphere, I see substantial deviations of the steepness of the source latitude contours from the moist isentropes. Of course, we expect that because in these dry upper tropospheric regions water vapour can be substantially older and make the distribution of source latitudes much wider. Could the authors maybe provide a weighted mean standard deviation or interquartile range of the source latitude in addition to the mean? This would provide a way of characterising the widths of the source distributions. I would assume that it is widest in the center of the storm track but maybe I am wrong.

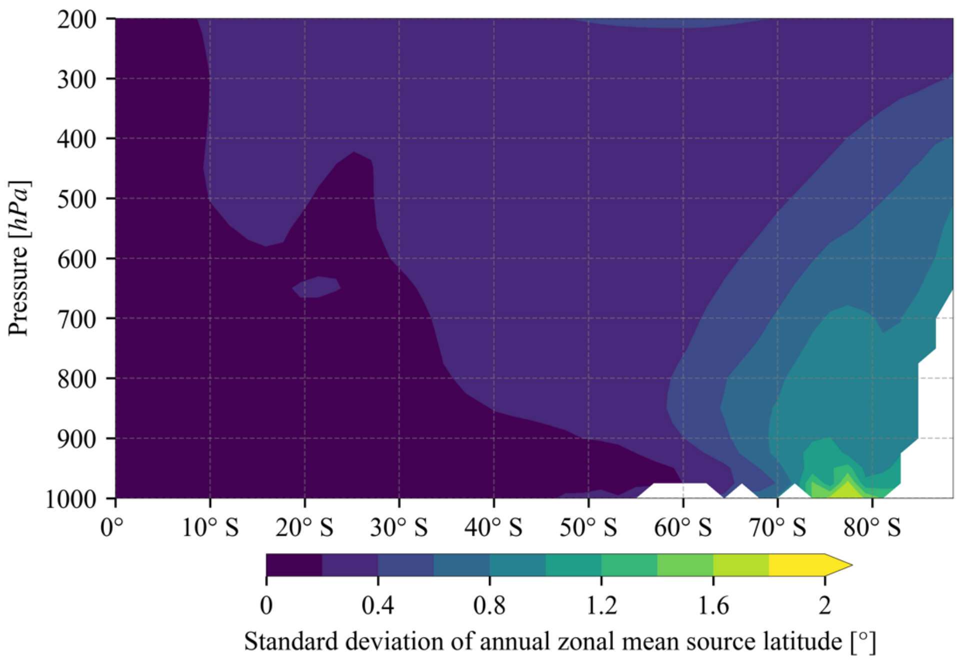
→ Thus, in short, from this analysis, I see mainly deviations from the moist isentropic poleward moisture transport framework, rather than agreement with it. I think this aspect ought to be discussed more in depth along with Fig. 4 (both panels). And, in particular, if diabatic processes are invoked for explaining these deviations, then why not name, which one the authors think could play a role?

Response: We appreciate this comment, and we fully agree with the referee. We rephrased the following sentences to stress the deviations.

The part in the abstract is changed to "Central Antarctic precipitation is sourced from more equatorward (distant) sources via elevated transport pathways than coastal Antarctic precipitation. This has been attributed to a moist isentropic framework, i.e. poleward vapour transport tends to follow constant equivalent potential temperature. However, we find notable deviations from this tendency especially in the lower troposphere, likely due to radiative cooling."

The part In the result section is changed to "This pattern has been attributed to a moist isentropic framework (Pauluis et al., 2010; Bailey et al., 2019; Wang et al., 2020), which suggests that poleward moisture transport follows moist isentropes, i.e. contours of equivalent potential temperature. However, we find notable deviations from this framework especially in the lower troposphere (Fig. 4), as moisture transport pathways intersect moist isentropes. This might be expected due to the radiative cooling effects of water vapour in the troposphere (Manabe and Strickler, 1964)."

We checked the annual standard deviation of zonal mean moisture source latitude of atmospheric humidity as in the following figure. The magnitude is generally small and is larger in polar regions.



5) L. 14: “wind10”: this variable is not yet defined in the abstract, please be specific at this stage. I also think that it is not such an elegant variable name for the main text.

Response: wind10 is defined in L. 7. Yes, but we might not easily find a better name for it.

6) L. 63: “issues with the **Lagrangian** identification of precipitation events solely using thresholds in specific humidity changes”. Yes, true, but reproducing precipitation events at the right place and at the right time is tricky as well for ECHAM6. Actually, as long as the precipitation statistics are faithfully reproduced over the ice sheet over the time period considered, this aspect of precipitation event representation is irrelevant in the context of a free running simulation. So, I am not sure this is a fair point to make at this stage. I would simply say that the Eulerian method presented in this study is complementary to the Lagrangian one and offers an elegant online diagnostic of the moisture sources of Antarctic precipitation.

Response: Thank you. We deleted that statement and added in the next paragraph: “This Eulerian method is complementary to the Lagrangian one and offers an elegant online diagnostic of moisture sources.”

7) L. 81: “revised definitions of Heavy Precipitation”: revised compared to what? I would remove “revised”. Maybe also think about not using any abbreviation (and not necessarily capital letters) for heavy and light precipitation because there are already many abbreviations used in this paper.

Response: We removed the word “revised” and the abbreviations for heavy and light precipitation.

8) L. 92: “the EAIS may be slightly too low in elevations” Why may? Some “valleys” may be missing too in the topography at this coarse resolution. Do you mean that the EAIS is on average too low?

Response: We wanted to express that the peak elevation over EAIS is lower in the simulation than in nature. We removed this phrase to avoid ambiguity.

9) L. 102: is q^{near_sfc} the q at the lowest model level?

Response: Yes. We modified the variable description accordingly.

10) L. 120: to characterise the near-surface humidity gradient $q_{near\_surface}$-q_{SST} or RH_{SST} would be more effective, see Aemisegger and Papritz et al. 2018
Aemisegger, F., and L. Papritz, 2018: A Climatology of Strong Large-Scale Ocean Evaporation Events. Part I: Identification, Global Distribution, and Associated Climate Conditions. J. Climate, 31, 7287–7312, https://doi.org/10.1175/JCLI-D-17-0591.1.

Response: Thanks for this comment. This is indeed one of our current research questions: whether it has added value to trace RHsst in the study of water isotopes.

11) L. 125: “moisture parcels” -> moisture in air parcels?

Response: removed ‘parcels’.

12) L. 141: “The difference...” in what? In the chosen definition of LP and HP?

Response: modified as “These definitions”.

13) Section 2.3: Do I understand it correctly that the LP and HP definitions are chosen such that these categories contribute to a significant share of the mass balance? Just to be symmetric in the information given about the two categories: how much does the HP category contribute to total precipitation? (i.e. what is the top-10%-precipitation-days’ share of total precipitation?).

Response: Yes, while light precipitation contributes to 10% of total precipitation by definition, heavy precipitation contributes to 30 to 70% of total precipitation as shown in Fig. B4.

14) L. 154: “on the low side” -> lower than the reconstruction, “high across some coastal areas” -> larger than in the reconstruction

Response: changed.

15) L. 157: I would say that ECHAM6 clearly shows a larger interannual variability (2x larger than the reconstruction): is this expected given the temporal resolution of the ice core data?

Response: Yes, there is larger interannual variability in the simulation. We are not sure whether it is because the model simulates too large variability or the accumulation product based on ice core data did not capture enough variability. The ice core data is annually resolved, but diffusion in the ice might smooth out the variability. We modified the sentence to: “Interannual variability, measured as the percentage of annual standard deviation to the annual mean, is slightly higher in the ECHAM6 simulation (~20%) than in the Medley dataset (~10%).” The investigation of this variability difference is out of the scope of this study.

16)L. 160: mention the ERA5 period with which you compared your preindustrial simulation. The warm season precipitation (NDJFMA) is quite substantially higher in ERA5, is this an effect of the slightly warmer Southern Hemisphere atmosphere in the period 1979-2021?

Response: Thanks, this can be a very valid explanation. We did find that there is a significant increasing trend in Antarctic precipitation in a CMIP6 historical simulation using AWI-ESM, which uses ECHAM6 as the atmospheric component. So we are not surprised that our preindustrial simulation using ECHAM6 shows less precipitation than ERA5.

17) L. 169-170: make clear that this is in the annual mean.

Response: added.

18) L. 170: The share of moisture sourced from sublimation over Antarctica probably depends on the parametrisation of the surface sublimation flux (e.g. Gerber et al. 2023). Could it be that the regions affected by very high sublimation fluxes with e.g. strong katabatic winds and blowing snow sublimation tend to feed cold air outbreaks and contribute more to precipitation over the ocean? Or are the sublimation fluxes over Antarctica just so small compared to the available moisture in the atmosphere? Gerber, F., Sharma, V., & Lehning, M. (2023). CRYOWRF—Model evaluation and the effect of blowing snow on the Antarctic surface mass balance. Journal of Geophysical Research: Atmospheres, 128, e2022JD037744, https://doi.org/10.1029/ 2022JD037744.

Response: We fully agree that the contribution to total precipitation from the Antarctic ice sheet depends on the parameterisation of surface sublimation fluxes. And we find notable differences in this contribution from simulations of ECHAM6 and the Unified Model (UM) using both the same water tracing diagnostics. This will be discussed in a future paper from our group. We added one sentence: "The magnitude of continental recycling depends on the parameterisation of surface sublimation fluxes (Gerber et al., 2023) and thus requires further investigation, e.g. inter-model comparisons or sensitivity tests of surface schemes."

We only find notable contributions from Antarctica to oceanic precipitation over the Ross ice shelf. Again, it depends on the parameterisation.

19) L. 179: does this finding about the precipitation coming from the open ocean south of 50°S also relate to the fact that the ocean surface of this oceanic region is larger around the AP and WAIS than for the EAIS? Does it have to do with the sea ice extent in the different basins? Does the steeper topography of the EAIS play a role (forcing the rain out of the humidity sourced from the South Atlantic south of 50°S along the EAIS slopes). Also, in terms of dynamical drivers of this share of precipitation from south of 50°S: it is really interesting to note that the Dronning Maud land receives the most equatorward moisture of whole Antarctica (even though the highest elevation of the Plateau lies much further to the southeast). This might be linked to the spiral shaped form of the Southern Ocean storm track. In the South Atlantic many extratropical cyclone genesis points are climatologically located relatively far North compared to the South Pacific (see Wernli and Schwierz, 2006). Cyclones likely play a key role in poleward moisture transport in this region.

Wernli, H., and C. Schwierz, 2006: Surface Cyclones in the ERA-40 Dataset (1958– 2001). Part I: Novel Identification Method and Global Climatology. J. Atmos. Sci., 63, 2486–2507, https://doi.org/10.1175/JAS3766.1.

Response: We very much appreciate these points. Those are valid hypotheses that different contributions to EAIS/WAIS/AP precipitation from the ocean south of 50°S can be related to the geographic size of the ocean regions, sea ice extent, and topography (higher in EAIS and thus less moisture from nearby oceans).

We do think extratropical cyclones play an important role in poleward moisture transport. It could be a very promising study to combine climatology of cyclones and moisture source diagnostics, which is unfortunately not in the plan of our current study. There are a few other findings in this study that could potentially be better understood with a better knowledge of cyclone climatology: Pacific and Indian oceans contribute more than twice to Antarctic precipitation than the Atlantic ocean, while their geographic sizes are not twice larger; Antarctic precipitation is sourced from windier conditions than usual (~2m/s), which might be linked to cyclone activities.

20) L. 186 & L. 194: “tends to take an elevated path” -> “has to rise to higher altitudes”? An elevated pathway nearly sounds as if it would travel in the upper troposphere for a very long time before raining out over the Antarctic ice sheet.

Response: Agreed and changed.

21) L. 189: “so” -> “therefore”

Response: changed.

22) L. 191: “approximating a moist adiabatic poleward ascent”, this tendency is discussed already in Stohl and Sodemann, 2009, albeit with isentropes (not moist isentropes) Stohl,A.,and Sodemann,H.(2010), Characteristicsofatmospherictransportintothe Antarctic troposphere, J. Geophys. Res., 115, D02305, doi:10.1029/2009JD012536.

Response: Thanks, changed.

23) L. 191: “this tendency to follow contours of equivalent potential temperature breaks down in the lower troposphere” -> tendency to follow moist isentropes?

Response: This sentence is removed as in the response to the first comment.

24) L. 191: about the “break-down” of the moist isentropic approximation of poleward transport: Which diabatic cooling mechanism do the authors think plays a role? Could diabatic heating especially in the upper troposphere also explain part of the observed deviations from the moist isentropic framework?

Response: The diabatic cooling mainly results from radiative cooling effects of water vapour (Manabe and Strickler, 1964). Yes, diabatic heating is most obvious in tropical mid-to-upper troposphere. Although equivalent potential temperature is conserved during vapour condensation for a given air parcel, latent heat release might still heat remaining moisture to cross moist isentropes.

25) L. 197: as mentioned in my general comment: just make sure that it stays in the mind of the reader that this study covers another time period.

Response: added: “though their study was for present-day climate rather than preindustrial climate as in our study.”

26) L. 201: most equatorward sources for DJF: that is surprising because of the minimal sea ice extent and the slightly weaker jet and storm track in summer. Is this simply due to the warmer atmosphere on average, leading to higher humidity contents and longer transport distances? And as you write flatter isentropes "giving access" to more equatorward sources?

Response: Thanks for this comment. We were also surprised by this result. As sea ice is at minimum during austral summer DJF, we would expect the opposite to be true. We thought of two mechanisms as written in the text: 1) flatter moist isentropes in DJF (not convincing enough as there are significant deviations from the moist isentropic framework, so removed now); 2) weaker westerlies in DJF. The second hypothesis is partly supported by our results in Section 3.5.

In DJF, the atmosphere is warmer, humidity is higher, and oceanic evaporation is lower, which means longer residence time of moisture. However, we are not sure whether it means longer transport distance.

27) L. 203: milder->flatter

Response: changed.

28) L. 209: The fact that source longitude shows the largest interannual variability is interesting and shows that the strength of the westerlies and the storm track dynamics is likely important for modulating the moisture source properties.

Response: Thank you for this point. We fully agree and added the following: "It suggests that the strength of southern westerlies and the storm track dynamics are likely important for modulating the moisture source properties."

29) L. 211: "source latitude and longitude"

Response: Changed.

30) L. 214: "lies within the range of estimates from the literature"

Response: Changed.

31) L. 220-227: Interesting! This corresponds to the range of wind speeds associated with events of strong ocean evaporation in the Southern Ocean as discussed by Aemisegger and Papritz, 2018. Extratropical cyclones and trailing fronts were identified as key weather systems with which enhanced ocean evaporation is associated.

Response: It is really nice to know this relevant study. Based on Eq. 1 it can be expected that if wind speed increases while other variables staying the same, evaporation will increase linearly with wind speed. It might be relevant to project how evaporation will change under climate change and how it impacts Antarctic precipitation.

32) L. 226: “other forms of storms” what is meant here: tropical cyclones or polar lows? I think if you write “extratropical cyclones propagating along the Southern Ocean storm track” you also include subgroups such as mesocyclones or polar lows.

Response: Thank you for pointing it out. We changed it as suggested: “extratropical cyclones propagating along the Southern Ocean storm track”.

33) L. 230: what does “evaporation processes will cause some decoupling of moisture source properties” mean? Do you mean that synoptic-scale variability at the source causes variability in the evaporative conditions?

Response: No. We meant moisture source properties can be slightly decoupled from moisture source locations because of their impacts on evaporation.

34) L. 234: what do you mean by “wind10 at source” -> climatological wind 10 at the source?

Response: Yes, changed.

35) L. 239: in the previous two paragraphs you just discussed **dynamic** not thermodynamic controls on moisture availability for Antarctic precipitation.

Response: Yes, we changed it to “dynamic control”.

36) L. 256: A case study discussing this suppression of ocean evaporation during polarward moisture transport in warm sectors of extratropical cyclones is Thurnherr and Aemisegger, 2022.

Thurnherr, I. and Aemisegger, F.: Disentangling the impact of air–sea interaction and boundary layer cloud formation on stable water isotope signals in the warm sector of a Southern Ocean cyclone, Atmos. Chem. Phys., 22, 10353–10373, https://doi.org/10.5194/acp-22-10353-2022, 2022.

Response: Thank you for pointing to this nice study as further supporting evidence. It is cited now.

37) L. 282-285: The impact of the SAM on the storm track dynamics and shifts in strong ocean evaporation patterns as well as near surface conditions is discussed in Aemisegger and Papritz, 2018.

Response: Thank you for letting us know. It is cited.

38) L. 291: “These results quantify the degree to which poleward moisture fluxes are associated with the SAM” -> are modulated by the SAM? And maybe instead of fluxes rather use poleward moisture transport?

Response: Thanks, changed.

39) Fig. 10: the stippling is very difficult to see

Response: Thank you for pointing this out. We enlarged the stippling in all related figures.

40) L. 300: “SAM states exert controls” -> “the SAM impacts”

Response: Changed.

41) L. 310: “limits on” -> “of”

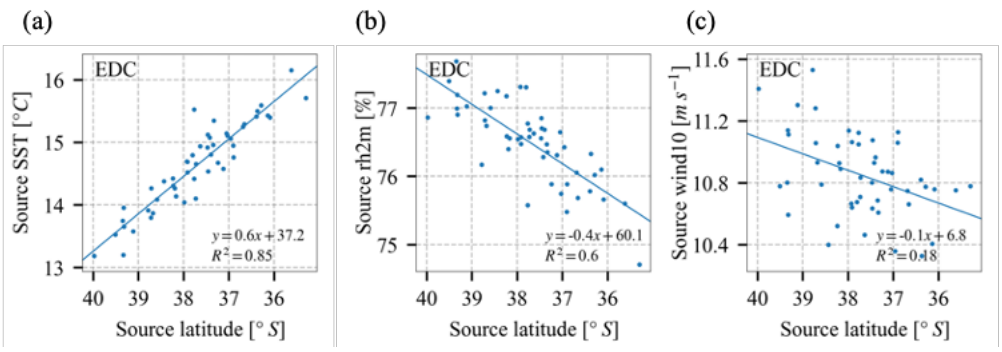
Response: Changed.

42) L. 328: “yields a more precise value compared to previous method” -> explain why explicitly

Response: This sentence is removed here and the pros and cons of our method compared to previous ones are discussed in detail in subsection 2.2.1.

43) L. 330-332: this is a bit obscure to me. True in close approximation for SST but not necessarily for rh2m and the wind speed.

Response: Yes, this is more valid for SST, but also valid for rh2m and wind10. Correlation analyses indicate high correlation between source latitude and source properties (highest for SST and lowest for wind10 as shown below for EDC as an example). Predominant meridional gradients (Fig. B2) of these variables could partly explain these correlations, though we notice a meridional maximum of wind10 at around 50 degree south. The decoupling between source wind10 and source latitude is then explained in the next paragraph.



44) L. 355: “other types of tracers in numerical systems” -> what do you mean by this?

Response: We modified it to be clearer: “Finally, we note the new scaled-flux tracing approach is applicable not only to water tracers in atmospheric GCMs but also to other types of tracers, e.g. aerosol tracers, in numerical systems”.

45) L. 356: “the full potential of our water tracing diagnostics is yet to be identified” -> This last sentence is a bit unspecific for the end of such a nice paper.

Response: Thank you. This sentence is removed.

46) Appendix A: very nice and interesting documentation. I just get lost at line 385. What does the index *i* represent and what do you mean when you write "by summing up all vapour contributions in a grid box, we obtain:"? Do you sum up over a number of grid points in a given region? In equation A4 some variables have an index *i* and others not. I didn't get why. And in A1 *t*, λ, ϕ have an index *i*, but in the next lines not. Sorry to be picky, but I really would like to understand this Appendix.

Response: Thank you, this is a nice point to improve the manuscript. We added the following to explain "i": "For any infinitesimal evaporative flux i from the open ocean". Indeed, it is an abstraction of infinitesimal moisture parcels in the model.

By "By summing up all vapour contributions in a grid box, we obtain", we meant summing up all infinitesimal moisture parcels in a grid box, which represents all moisture in a grid box.

In A4, if t, λ, ϕ have an index i, they represent the time, latitude and longitude associated with the evaporation flux i; if t, λ, ϕ do not have an index i, they represent any subsequent time, latitude and longitude. The same for A1.

47) Appendix A: I like your comparison with the predefined-region water tracing method described at lines 410ff a lot. To me this is a highly effective way to show the advantages of the scaled-flux approach and I would find it very helpful to have a short version of the results from this comparison in the main text.

Response: We appreciate this comment and we added this figure and associated text to a new subsection 2.2.1, also as a response to comments of the second referee.