

Responses to comments of Referee #2 - 2nd iteration

We appreciate the efforts of Referee #2 in reviewing the manuscript. These detailed comments have significantly improved the draft.

After reading the revised manuscript and the author reply, I have the impression that the authors have responded appropriately to the reviewer comments. This revised version of the manuscript reads much better, and is more logically organized in terms of structure and content. After these revision, a few additional items stand out more clearly now that should be further improved. Most of the improvements concern the writing, and none of these points is major, so I consider this iteration a minor revision.

Minor comments:

1. The writing in the introduction can be improved. There are a couple of statements that would benefit from a reference, and where the connection and logic are not clear or not consistent.

Response: We appreciate this comment. We modified the introduction based on the detailed comments below.

2. The comparison of source properties between different components in Sec. 2.2.1 would need a form of variability quantification to be interpretable. The differences are similarly small for wind, temperature, and humidity, and it is not fully clear why wind is discussed specifically. Is it really surprising that wind speed is the main modulator in terms of evaporation strength, given the absence of large land regions that can supply dry air (the other component of evaporation)?

Response: To save computational resources, we only run the simulation for Fig. 1b for two years, with one-year spin-up period. So we cannot present interannual variability here. However, a lack of variability quantification or statistical tests, which might require ~30 year simulations, does not afflict our conclusion here that two methods provide close results. We mainly discussed wind in Fig. 8 because similar analysis on moisture source latitude, SST, rh2m does not show consistent patterns over Antarctica. Yes, it is surprising to have this finding, which is also why we stressed this point in the text. We will also investigate the impact of RH relative to SST on evaporation strength.

3. Sec. 3.3, regarding source temperatures, the results does not exactly match the literature, as is being argued. Also, how large is the variability (1 sig std)? Can some of the discrepancy be explained by a differing regional focus of previous studies? A similar quantification would be useful to assess the significance of the apparently very small differences of rh2m and wind10 in different regions that are discussed in the same paragraph.

Response: True, the literature reports a large range, while using different methods. Our results lie in the middle of the estimates. The variability of source properties is shown in Fig. B5, generally less than 1 °C for precipitation source SST over Antarctica.

The two modelling studies by Koster et al. (1992) and Delaygue et al. (2000) report numbers for the whole of Antarctica, whereas Petit et al. (1991) reports numbers for surface snow collected across Antarctica. So the differences might not result from different study regions nor time periods.

4. Limitation of model in simulating storms, precipitation are never addressed. How much do we learn about the actual precipitation in Antarctica, and how much do we learn about how the model simulates precipitation in Antarctica?

Response: As this study focused on preindustrial periods, evaluation of the model simulation against recent observations of storms or precipitation can be problematic. While we consider the coarse model resolution to be insufficient to simulate coastal precipitation, there is limited observations over Antarctic Plateau to evaluate model performance. Though, our current research collected multiple datasets of precipitation and vapour isotope observations over Antarctica to evaluate model performance using ERA5-nudged simulations.

5. The method description mentions some modifications compared to Fiorella et al., 2021, and one has to go to the appendix to learn about what these modifications are and what uncertainties that imposes. I would strongly favour mentioning specifically what is modified in comparison to Fiorella when referring to the Appendix in the main text, so that readers know what they will find there. Also, the information about a <2% mass balance error from the imposed flux limits should be stated in the main text. Clearly, such an error margin will be relevant when discussing the precipitation for different amount categories (Sec. 3.4, Fig. 10).

Response: Thank you. We added the following: “For example, we only trace moisture evaporated from the open ocean, and we use three water tracers for each source property to ensure the conservation of water masses and limit the propagation of numerical errors.”

Detailed comments:

L. 7 onwards: wind10 is uncommon and not specific in terms of wind speed or wind direction. I suggest changing to the more commonly used and specific abbreviation vel10 (for wind velocity).

Response: Thank you. wind10 is replaced with vel10 throughout the manuscript.

L. 17: "first time has been quantified" the highly emphasised significance of this finding remains unclear. Is it not enough to state the finding without the "first time" claim?

Considering also that this is a statement about a model, not about nature (i.e. wind measurements).

Response: Thank you. We removed the statement 'first time'.

L. 23: add reference to 2nd sentence

Response: Thank you. We added reference to Purich and Doddridge, 2023 and Gorodetskaya et al., 2023.

L. 25: not clear what "this" refers to, rewrite

Response: Changed 'On top of this' to 'Furthermore'.

L. 37: "surprisingly little": I would disagree that surprisingly little is known about Antarctic precipitation in general. But there are for sure important knowledge gaps when it comes to precipitation at high latitudes. Consider rephrasing by stating specifically what (relevant) knowledge gaps exist. In its present form, the statement sounds a bit like the knowledge gap is with the authors - clearly a misinterpretation to try and avoid.

Response: We appreciate this comment. We changed this sentence to "our understanding of thermodynamic and dynamic factors driving Antarctic precipitation remains limited."

L. 39 onward: The sentences in this paragraph lack logical connection and stringency. Statements such as "tend to occur", "is conducive for" and "occur alongside" create a strange disconnect (as if coincidental) between dynamically deeply connected and interrelated aspects of the atmospheric flow. Please rephrase.

Response: Thank you for this comment. We changed it to: "Marine air intrusions are efficient at transporting moist and warm air from subtropics to Antarctica (Schlosser et al., 2010; Dittmann et al., 2016). The intrusions generally occur alongside strong meridional flow during planetary wave amplification (Adusumilli et al., 2021; Noone et al., 1999; Massom et al., 2004), sometimes in the form of atmospheric rivers (Gorodetskaya et al., 2014; Wille et al., 2021).

L. 46: "are mainly known" - I don't think this is a correct statement, both aspects of these precipitation events are well documented in literature

Response: Yes, we agree. This sentence is changed to "The marine air intrusions play a major role in heavy precipitation events at both coastal locations and Antarctic interior (Genthon et al., 1998; Gorodetskaya et al., 2014; Stohl and Sodemann, 2010)".

L. 48: "is useful for predicting" this could potentially be the case, but I miss the supporting evidence for this statement in the way that is written here. It is also unclear why you raise this

point here, would this not be more something for the implication of the study?

Response: Thank you. We removed this statement here.

L. 59: "it is not yet clear how SAM variations...": this statement contradicts the available knowledge about how SAM influences precipitation in the previous sentence. Maybe instead highlight specific aspects where additional knowledge gaps exist?

Response: Thanks, we changed it to "To project Antarctic precipitation changes, it is important to understand how SAM impacts moisture transport paths."

L. 125: consider simplifying this equation and writing by using simpler but equally accepted/established symbols such as q_s , q_{2m}

Response: Thank you, we simplified it to q_s and q .

L. 138 onwards: "quite close", "very similar": these expressions express a subjective judgement. Can this be phrased more objectively. Adding a small table for all assessed variables would be useful.

Response: We appreciate this comment. We removed these subjective statements. We only wrote the differences in source latitude from two methods in the text, and we consider it to be enough to show the confidence in the accuracy of two methods. Therefore, we do not include a small table to disturb the flow.

L. 140: clarify how this bias is computed, what is the reference?

Response: Here "bias" refers to the major deviations in results of prescribed-region water tracers from those of scaled-flux water tracers. We do not have an "absolutely accurate" reference, and the statement "bias" is indeed our judgement. We consider it as bias for two reasons: 1) The bias is inherent in the assumption that moisture source latitude from each latitude bin is the middle value of the latitude range. 2) The bias can be reduced by using finer latitude bins (e.g. 5°).

L. 150: "Furthermore" does not connect well to the previous sentences, since this is a disadvantage of the scaled-flux water tracing method.

Response: Yes, we removed this word.

L. 153: "Finally" does not appear logical here, since there is no connection between this higher-level comparison of both Eulerian approaches to Lagrangian methods. Maybe you could expand a sentence that explains that this is a more general comparison?

Response: Thank you. We changed 'Finally' to 'In addition'.

L. 155, Section 2.3: These thresholds are still extremely small in comparison to numbers that

can be obtained in the real world situations on a daily basis. With typical snow density, these thresholds would translate to a 0.2 mm and 0.02 mm thick snow layer. I understand that it is possible to use such threshold values in a model context, but some statement mentioning the limited transferability of such threshold values to the real world are advised. The need to use differing definitions of heavy and light precipitation days is evidence of this fact. Some models can produce extremely small precipitation rates for extended periods of time, which affects the lower 10th percentile.

Response: We fully agree the limited transferability to observations. We added: “We note that this definition has limited transferability to station observations.”

L. 173: "Though it is lower" - rephrase

Response: Changed to “The simulated accumulation is”.

L. 175: "This could partly ..." - those are vastly different explanations. What are the concrete consequences of smoothed topography in the model, and how likely is it that the model is more correct than the accumulation reconstruction?

Response: We split it into two sentences. “The relatively coarse (T63) spatial resolution of the simulation might not be adequate to simulate coastal precipitation. Furthermore, the reconstruction of Antarctic accumulation might be affected by limited ice core records and local processes such as melt events in coastal regions (Monaghan et al., 2006).” The smoothed topography in ECHAM6 could have a major impact on coastal precipitation, which need to be quantified with high-resolution modelling. The accumulation reconstruction is obtained with spatial patterns from model output of MERRA2. Coastal melt events, which are not represented in the model, might lead to model-data mismatch as well.

L. 238: "It suggests that..." this sentence does not add new insight, and is in addition vague. Can something more firmly be extracted from this analysis?

Response: Thank you. We removed this statement. Unfortunately, we do not infer more firm insights from this result now.

L. 254: "This narrow range" - can you expand how the narrow range connects to the role of cyclones? Is there a specific reason why you mention both cyclones and storm tracks?

Response: This is the question that we propose to merit further studies. So we changed these sentences to “Further studies are merited to investigate the relationship between this narrow band of source vel10 of annual mean precipitation over Antarctica and extratropical cyclones propagating along the Southern Ocean storm tracks”. I mention only extratropical cyclones now.

L. 258 onwards: Firstly - Secondly are quite far apart and thus hard to relate, rephrase

Response: Thanks. We removed these words.

L. 259: unclear, where can this decoupling be seen?

Response: Thank you. We removed this statement as it does not provide valuable information and can be confusing.

L.281: "the modelling results" rephrase such that it becomes clear that you refer to your own modelling results here

Response: Changed to "our water tracing results"

L. 338: It would be good to differentiate here between a model perspective and nature. For example, add to this sentence "in a climate model framework".

Response: Added.

L. 335: The conclusions should also list some limitations of this study and the overall approach. For example, the limitations of model-simulated precipitation in Antarctic, and the rather coarse resolution of the simulation, which affects the realism of cyclones and fronts, maritime air intrusions, and so on. If you think critically, how much can ultimately be learned about the nature of Antarctic precipitation from this new tool?

Response: We stressed these limitations in the final paragraph and pointed to further research directions: "We note that the results presented here are based solely on a single model simulation. To explore the model dependence of the results, we are developing similar water tracing diagnostics in another atmospheric GCM, the UK Met Office Unified Model. As the coarse spatial resolution of the ECHAM6 T63 simulation might be insufficient to resolve coastal atmospheric flows and marine air intrusions, high-resolution simulations will be conducted in our future studies."

Regarding how much we can ultimately learn from this new tool for Antarctic precipitation, we firstly need to discuss what we want to know about Antarctic precipitation ultimately. The main scientific questions around Antarctic precipitation are its spatial-temporal distribution, temporal variability across multiple scales, what are the driving factors, and how it changes under climate change. These questions are hard to investigate and fully understand with limited observations under harsh environmental conditions. Climate models are necessary, but they might not give the right results because of poor representation of key processes, e.g. Southern Ocean clouds, Antarctic ice sheet surface moisture fluxes. Limited observations might also not be enough to constrain the models physics and parameterizations. Therefore, such a diagnostic tool could be helpful to understand model behaviour, improve the models, and maybe also improve the interpretation of observations, e.g. water isotopes. We have no

doubt that the water tracers are a valuable diagnostic modelling tool, and we consider the full potential of the tools need to be explored with inter-model comparisons, high-resolution modelling, and model-data comparisons.

L. 361: The wind may be linked to cyclones, but also to other features, and simply pressure gradients. Does the wind only play a emphasized role in the evaporation flux variability because of the absence of land regions which could lead to variations in RH?

Response: Thank you for these useful points. It is true that the wind speed can be simply related to pressure gradients. Our results only indicate that given other conditions, higher wind speed favors moisture availability for Antarctic precipitation at individual oceanic grid cells in ECHAM6. Regarding RH or moisture gradient near the ocean surface, we will do such analysis since we can also trace RH related to SST now. However, it remains a question how much it helps to understand the real-world water cycle.

L. 369: I suggest the last paragraph be rewritten to improve the flow and connection between these sentences. Right now they appear merely as an unsorted list.

Response: Thanks. We rewrite it as below:

“We have identified several directions for future research. We note that the results presented here are based solely on a single model simulation. To explore the model dependence of the results, we are developing similar water tracing diagnostics in another atmospheric GCM, the UK Met Office Unified Model. As the coarse spatial resolution of the ECHAM6 T63 simulation might be insufficient to resolve coastal atmospheric flows and marine air intrusions, high-resolution simulations will be conducted in our future studies. While this study focuses on preindustrial conditions, moisture source changes in historical periods, paleoclimate, and future scenarios could also be investigated. Finally, we note that the scaled-flux water tracing approach is applicable not only to Antarctic problems, but also to a range of questions associated with water cycle changes in the rapidly changing environment.”