Dear Dr. Smith,

Please find our revised manuscript "A model framework on atmosphere-snow water vapor exchange and the associated isotope effects at Dome Argus, Antarctica: part I the diurnal changes " by Ma et al. We have explicitly addressed all the comments and suggestions from the two reviewers. Below we briefly described the main comments and our responses.

One of the main comments/suggestions from the first reviewer was that the conclusions would require significant modifications if the influence with free atmospheric layer were incorporated into the model. When involving the effects of the free atmosphere in the revised manuscript, in summer, the diurnal variations in snow isotopes become larger, so as the enrichments after 24 hours and/or longer duration. In winter, the modeled diurnal variation and changes (i.e., depletion) after 24 hours and/or longer duration also become larger, though the absolute values are still much smaller compared to that in summer conditions. As a result, the key conclusion that vapor exchange at the atmosphere-snow interface leads to a larger seasonality of snow isotopes holds the same. This is why in the response we state “the conclusions are unchanged”.

This reviewer also questioned why the modeled the snow δD amplitude is so small. We compared our modeled of snow δD amplitude of (1.6±2.71) ‰ with the observed amplitude of ~ 3 ‰ at the Konhen Station. They are in fact comparable, and the lower value at Dome A is due to the lower wind speed at Dome A. This reviewer also has pointed out some errors or questions, which we have explicitly addressed as stated in the response file.

The comments from the second reviewer were focused on the continuous simulations at Dome A and sensitivity tests. We have included two additional simulations that utilize continuous meteorological inputs for summer and winter days at Dome A. The details of these simulations can be found in Section 2.2 and Section 3.2.4 of the revised manuscript. We also reformulated the description of sensitivity test results in Section 3.4, discussing how the factors tested influenced the simulations of diurnal variations in water vapor isotopes and snow isotopes. In addition to these technical improvements, we conducted a comprehensive language revision of the manuscript and the addition of some new references in the manuscript.

We have also conducted a comprehensive language revision of the manuscript with the assistance of Nature AI language tool. We hope this would improve the writing.

We confirm that this manuscript has not been published elsewhere and is not under consideration by another journal. All authors have approved the manuscript and agree with its submission to The Cryosphere. Please address all correspondence to genglei@ustc.edu.cn. We look forward to hearing from you at your earliest convenience.

Sincerely,

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Response to Reviewer #1's comments

General comments:

1) While the authors have been updated the models that they used in the manuscript, the conclusions are unchanged, as stated by the authors in the response. This is surprising, because the added influence with the exchange with the free atmosphere should create extremely significant change to the vapour boundary layer. It's difficult to evaluate how this has actually been computed by the authors without in depth evaluation of what was done, which is beyond my duty as a reviewer.

Response: We thank the reviewer for this question. To clarify the point, that the key conclusion of the original manuscript without involving the effects of the free atmosphere is vapor exchange at the air-snow interface would tend to enlarge the magnitude of seasonal snow isotope variation, as it causes enrichments in surface snow isotopes in summer, while more or less depletions in winter.

When involving the effects of the free atmosphere in the revised manuscript, in summer, the diurnal variations in snow isotopes become larger, so as the enrichments after 24 hours and/or longer duration. In winter, the modeled diurnal variation and changes (i.e., depletion) after 24 hours and/or longer duration also become larger, though the absolute values are still much smaller compared to that in summer conditions.

As a result, **the key conclusion that vapor exchange at the atmosphere -snow interface leads to a larger seasonality of snow isotopes holds the same**. This is why in the response we state "the conclusions are unchanged".

Other than the above mentioned points, we did revised a bit of the conclusion: since based on the simulated results with or without the effects of free troposphere, the modeled changes in winter is not comparable to (i.e., lower than) those in summer, due to much stable boundary layer condition in winter. This makes the effects in summer can't be offset by summer, leading to overall enrichments in snow isotopes.

2) It's a bit surprising that the snow isotopic composition is shown as anomaly in Figure 3d, which makes me suspect that the simulation estimates were not matching with the observations in order to obtain the observed values for the vapour.

Response: Thanks for this question. Yes, in Figure 3d, we chose to show the modeled difference instead of the absolute values. It is indeed that the modeled absolute snow $\delta^{18}O$ doesn't make the observed surface $\delta^{18}O$ at Dome C. However, the reason is that in the model we used the isotopic composition of fresh summer snow (~-47‰ reported from Touzeau et al., (2016)) as initial snow isotope composition, which is higher than $\delta^{18}O$ observed in surface snow (i.e., -51.16‰). if we replaced the model initial value of -47‰ with -51.16‰, then the modeled absolute values are consistent with the observations (Figure 1 in this response).

In the previous versions, we chose $\delta^{18}O$ of fresh snow as the initial value to better constrain the changes due to air-snow exchange. To avoid confusion, in the revised manuscript, we replotted Figure 3 with $\delta^{18}O$ observed in surface snow (i.e., -51.16‰) as the model initial values, to make the results more consistent with the observations reported by Casado et al., (2018).

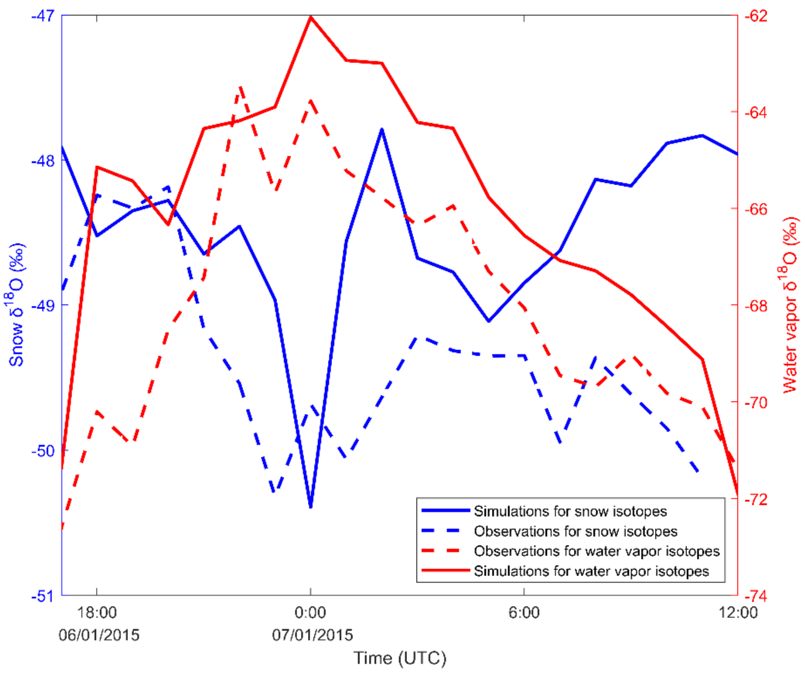

Figure 1: The simulations and observations of snow and water isotopic composition in the near surface atmospheric layer during the Jan 6-7th, 2015 at Dome C (the initial snow isotopic composition is -51.16‰ for running simulations).

3)The snow isotopic composition is missing in Figure 4d, so it's difficult to know what was to be seen there, but it seems like the amplitude of the snow dD variations are extremely small, which is not very realistic.

Response: Sorry, yes we forgot the put the snow $\delta^{18}O$ data there, and in the revised manuscript, we have added it. Regarding the amplitude of snow δD, the model gave an estimate of 1.6±2.71‰ for Dome A clear-sky conditions (Figure 4e of the main text). This value is a little smaller than the observed peak-to-peak amplitude of ~ 3‰ at Konhen Station in January. Such a difference can be attributed to the lower wind speed at Dome A (Text S4 in the Supplementary).

At Dome C, there is no available data of snow δD on the diurnal scale. Only one paper reported an observed value of ~2‰ for a peak-to-peak amplitude of diurnal variations in snow $\delta^{18}O$ during a frost event (Casado et al., 2018). This reported value is significantly higher than the average of simulated snow $\delta^{18}O$ variations at Dome A (0.8±0.35‰). The averaged meteorological input (including days in December, January, and February) used in Dome A simulations is the reason of the smaller diurnal amplitude of snow $\delta^{18}O$. For example, the averaged wind speed at Dome A (2.8 m/s) is lower than that in Dome C (3.3 m/s), leading to a less effective exchange between snow and water vapor. We consider individual day simulation, the diurnal amplitude in snow $\delta^{18}O$ diurnal variations can exceed 1.5‰ at Dome A, as evidenced by continuous simulations in January (Figure 7 of main text). This is also comparable to the observations at Dome C.

4)The winter conditions shown in Fig. 6 are clearly started with non matching vapour and snow isotopic composition since the vapour isotopic composition is converging toward a different value.

Response: We have re-examined Figure 6 and the winter simulation results using the updated model. It has come to our attention that there is a discrepancy between the simulation results in snow d-excess and the curve depicted in the submitted figure. This error has arisen from the extensive modifications made in this study, leading to our confusion between the calculation results with the updated model and those with the previous model. In the revised manuscript, we have corrected this mistake. The corrected result does not have the issue of converging toward a different value (Figure 2 of this response or Figure 6 of main text).

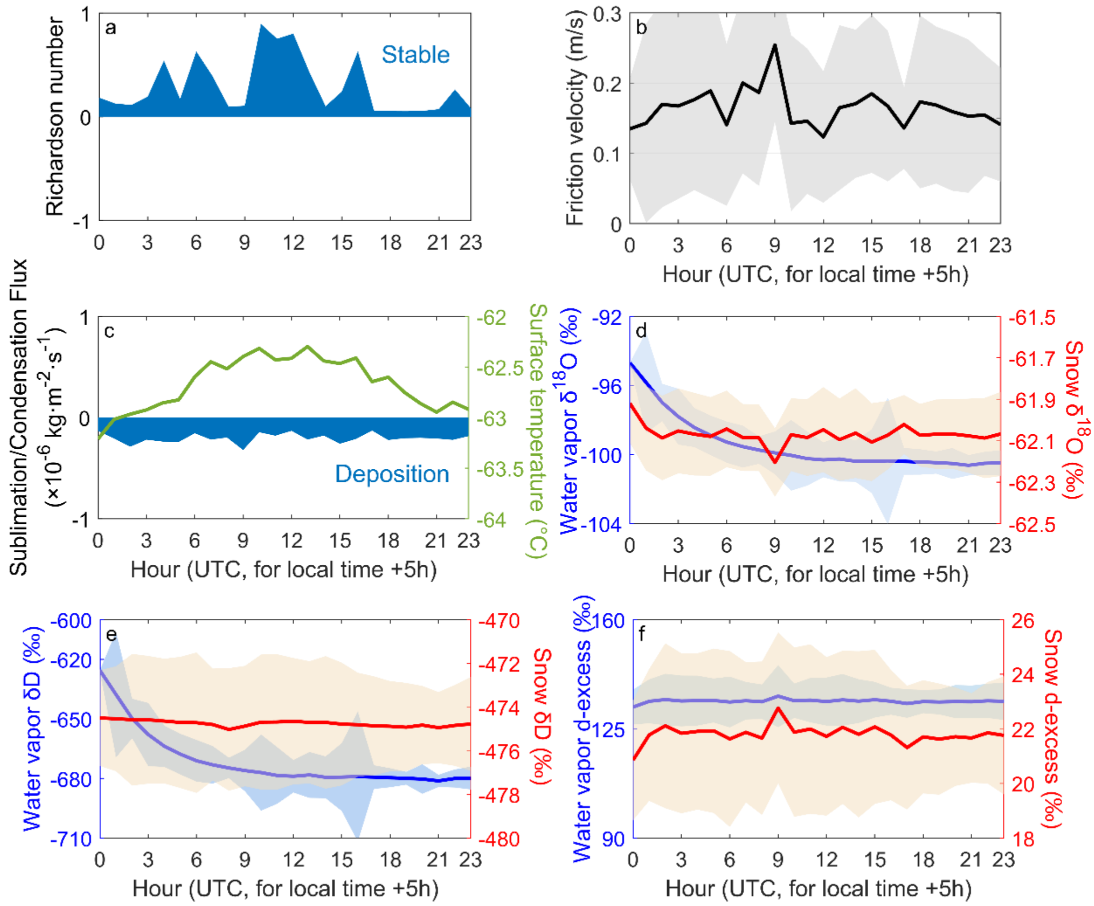

Figure 2: The simulated hourly mean vapor exchange flux and variations in atmospheric water vapor and snow isotopes under winter conditions at Dome A: (a) Richardson number, (b) friction velocity, (c) vapor exchange flux, (d) snow and water vapor $\delta^{18}O$, (e) snow and water vapor δD, (f) snow and water vapor d-excess. The uncertainties for each variable are displayed by shaded area in each subpanel.

5) Overall, I'm sure that the authors undertook a tremendous amount of work, and that this could potentially be an interesting manuscript, but I feel like the rigour of model shown here, and the application to Dome A conditions, is not sufficient.

Response: We appreciate the reviewer's time and efforts to evaluate this manuscript, we agree that there could be still rooms to improve the manuscript even after we have revised significantly according to the reviewer's constructive suggestions. But nevertheless, we think the results of this manuscript are new, since the results indicate this special kind of post-depositional processing, i.e., vapor exchange at the air snow interface is tending to enlarge the seasonal variations in snow isotopes, not as other processing tending to smooth the variability. We think this is a good enough point to elucidate and it shall inspire new observations or experiments to confirm this in the future.

End of the responses to Reviewer #1

Reference

Casado, M., Landais, A., Picard, G., Münch, T., Laepple, T., Stenni, B., et al.: Archival processes of the water stable isotope signal in East Antarctic ice cores, The Cryosphere, 12(5), 1745-1766, doi: 10.5194/tc-12-1745-2018, 2018.

Touzeau, A., Landais, A., Stenni, B., Uemura, R., Fukui, K., Fujita, S. et al.: Acquisition of isotopic composition for surface snow in East Antarctica and the links to climatic parameters, The Cryosphere, 10(2), 837-852, doi: 10.5194/tc-10-837-2016, 2016.

Response to Reviewer #2's comments

General comments:

1) The authors have mostly (see Comment L.147) addressed the flaws in the latent heat flux calculation and the model theory. Sufficient uncertainty and sensitivity analyses are performed, yet the outcomes of these analyses or their implications for the results' robustness are not addressed in the discussion. I suggest adding a discussion of the results' robustness based on the sensitivity analyses in 3.4, Text S4, and Text S5 before publication.

Response: Thank you for the feedbacks, and we also appreciate the valuable suggestions. In the revised manuscript, we have revised the description of the sensitivity analysis results in Section 3.3. Based on the sensitivity test results, we have added two paragraphs in the discussion section of main text to discuss the results from sensitivity tests.

The revised Section 3.3 are as follows:

The results of the sensitivity tests for the three groups are shown in Fig. 8. In the first group of tests, the magnitude of the diurnal variations in water vapor $\delta^{18}O$ ($\delta^{18}O_v$) is highly influenced by H_0 but not by h_0 (Fig. 8a). This finding aligns with previous calculations at Kohnen Station, which demonstrated a decrease in the magnitude of $\delta^{18}O_v$ with increasing mixing layer height (Ritter et al., 2016). On the other hand, the magnitude of diurnal variations in snow $\delta^{18}O$ ($\delta^{18}O_s$) exhibits a greater sensitivity to h_0 (Fig. 8b). This finding is consistent with field experiments showing that isotopic enrichment induced by atmosphere-snow water vapor exchange tends to decrease with increasing snow thickness (Hughes et al., 2021). Similar to the magnitude of $\delta^{18}O_s$, the changes in $\delta^{18}O_s$ after a diurnal cycle are more sensitive to h_0 (Fig. 8c).

In the second group, within the realistic $\delta^{18}O_{s0}$ and $\delta^{18}O_{v0}$ ranges, it is evident that the magnitude of $\delta^{18}O_v$ diurnal changes is more sensitive to $\delta^{18}O_{v0}$ than $\delta^{18}O_{s0}$ (Fig. 8d). As $\delta^{18}O_{s0}$ decreases, the magnitude of $\delta^{18}O_s$ diurnal changes decreases, emphasizing the influence of $\delta^{18}O_{s0}$ on snow isotopic variations (< 0.05‰ in Fig. 8e). In addition, the value of $\delta^{18}O_s$ after a diurnal cycle shows a greater sensitivity to $\delta^{18}O_s$, while such a change remains small (<0.01‰ in Fig. 8f).

Changes in $\delta^{18}O_{f0}$ significantly influence the magnitude of diurnal variations in $\delta^{18}O_v$, as shown in Fig. 8g. In contrast, these changes have a lesser effect on the magnitude of diurnal $\delta^{18}O_s$ variations and $\delta^{18}O_s$ changes after a diurnal cycle (Figs. 8h and 8i). The snow density has a considerable effect on $\delta^{18}O_s$, while it induces only a small change in the magnitude of diurnal $\delta^{18}O_v$ fluctuations.

The two added paragraphs in Discussion section are as follows:

The diurnal variations of water vapor isotopic composition, resulting from the exchange between the atmosphere and snow surface, are subject to influences beyond mere meteorological conditions. Specifically, fluctuations in the boundary layer height (H_0) can result in either an attenuation or an amplification of the magnitude of variations in water vapor isotopic composition (Ritter et al., 2016), as evidenced by Fig. 8a. Furthermore, the interaction between the free atmosphere and the boundary layer can significantly impact the diurnal variations in the water vapor isotopic composition

(Casado et al., 2018). Specifically, during periods of intense mixing, the variations in water vapor isotopic composition become more pronounced (Fig. 8g and Text S3). However, in the model employed for this study, these two input parameters are maintained as constants to simplify the calculations, whereas they vary daily in reality. This simplification for model calculations may lead to a reduction in the interday variability of simulated water vapor isotopic compositions (Fig. 3e).

Based on the results of the sensitivity tests, diurnal variations in isotopic composition of snow due to water vapor exchange processes can also be influenced by several parameters, such as snow thickness, snowfall isotopic composition, snowfall density, and surface roughness (refer to Fig. 8 and Texts S4). Among these factors, changes in snowpack thickness exhibit the most pronounced impact on the isotopic effects of water vapor exchange processes. Specifically, when the snow thickness exceeds 3 cm, the water vapor exchange effect struggles to induce interday variations in snow isotopes. On the other hand, the effects of snowfall isotopic composition, snowfall density, and surface roughness on the isotopic composition of surface snow may be limited during the Dome A summer season (Texts S4 and Fig.S3), given the realistic range of potential variations in snowpack parameters.

2) The authors have now simulated the impact of vapor fluxes for a continuous time series at Dome C. However, for Dome A, their simulation is still based on average diurnal cycles of the input data. To assess how averaging the input data might affect the results (see Comment L.416-417), I suggest one additional simulation for Dome A with continuous input data, regardless of the cloudiness.

Response: Thanks for your nice suggestion. We have added two additional simulation cases running with continuous meteorological inputs at Dome A site. One case is realized on summer days disregarding the influence of clouds, the other one is on winter days. The running duration for two cases are 11 days, consistent with the Dome C simulations. The selected period for summer simulation is from 5th to 16th of January for each year during 2006-2011(data were not observed in 2005). The winter period for simulations is 5th-16th, July. Thus, 6 groups of simulated results for each season can be obtained to calculate the average of continuous changes in water vapor and snow $\delta^{18}O$, as shown in Figure 1.

The continuous simulations at Dome A show opposite trends for changes in snow isotopes between summer and winter simulation conditions (Figure 1b and 1d). This supports that the seasonal snow isotope variations can be enlarged due to the snow-atmosphere water vapor exchange process. Also, the annual net effects can lead to an increase in the annual mean value of snow isotopic composition, in consideration of a more significant isotopic effects in summer. These important simulations and conclusions have been added into the main text (Method (Section 2.2), Results (Section 3.2), and Discussion).

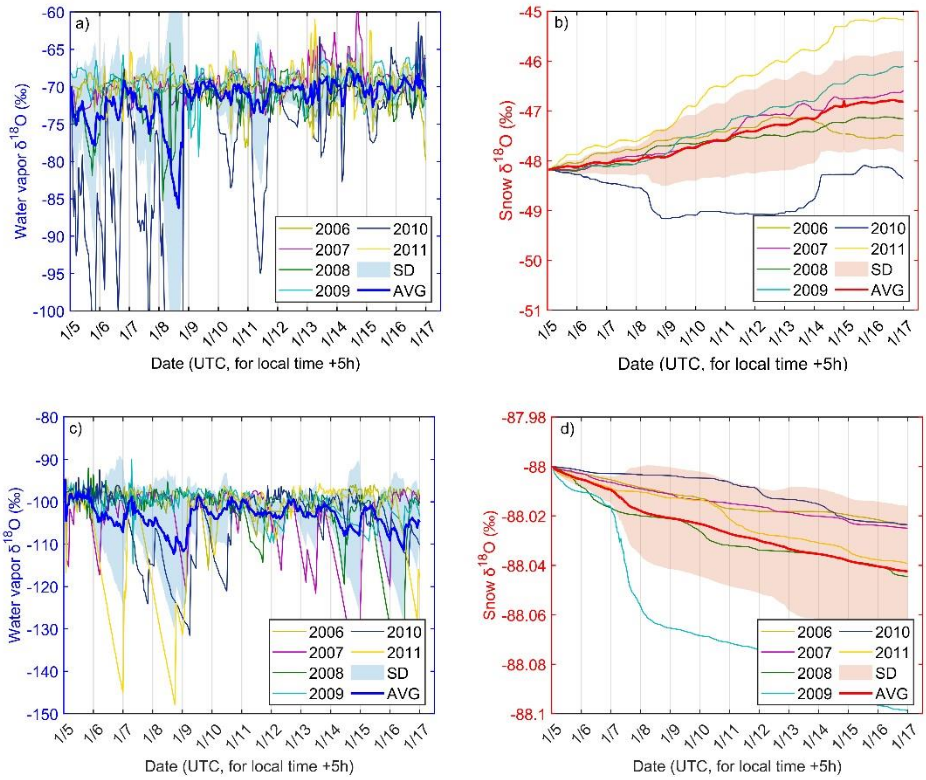

Figure 1: The continuous simulations in snow and water vapor isotopes at Dome A. Panel a) and b) respectively represents summer simulations in a 11-day period (Jan 5-16th, 2006-2011), Panel c) and d) are same to Panel a) and b), but for wintertime (Jul 5-16th, 2006-2011). In all panels, the light lines represent the simulated results of water vapor $\delta^{18}O$ for each year during the simulation period. The bold solid line and the light blue shadow are the averages (AVG) and standard deviations (SD) of $\delta^{18}O$ simulations in each year, respectively.

3) The authors make statements at several points in the manuscript (e.g., Comments L23., L.397, L.463-464, L.475-477) without providing sufficient evidence. I kindly ask the authors to revise such statements and reformulate them appropriately. I further suggest conducting a comprehensive language revision of the manuscript, as occasional imprecise formulations may lead to misinterpretation. Lastly, in its current form, the manuscript is missing references to figures and supplemental material wherever relevant. This makes it difficult to follow the authors' explanations, and I strongly advise providing all references before the publication of this manuscript.

Response: We are really grateful to the reviewer for the rigorous considerations. We have made the necessary revisions following the detailed comments provided by reviewer #2. A comprehensive check to the manuscript has been conducted, ensuring that erroneous sections have been rectified to the best of our ability. Additionally, efforts have been made to add the references as much as possible. Given the substantial modifications made to the revised manuscript, the individual sentence or section revisions are not listed one by one here. The reviewers can refer to the tracked changes version for a detailed overview of specific modifications. It is our sincere belief that these revisions will enhance clarity and comprehension of the article for all readers.

Detailed comments:

L.23: Please add: under "average" summer clear-sky conditions. It is important to distinguish the isotopic impact of an average clear-sky from the average impact during all clear-sky days, as both cases could differ significantly.

Response: Thanks, added.

L.28: Please clarify what is meant by "more or less".

Response: Thanks, we meant to express that the changes are small or negligible, but removed *"more or less"* to make this sentence more clear.

L.54: Estimates of the long-term effect of atmosphere-snow water vapor exchange on the snow isotopic composition in Greenland have been done by Dietrich et al. (2023), but not yet in Antarctica.

Response: Thanks for this suggestion. We reformulated this sentence as following: <u>*Isotopic effects associated with atmosphere-snow water vapor exchange at longer time scales have been done at Greenland Ice Sheet (Dietrich et al., 2023), but not yet in Antarctica.*</u>

L.144 (and others): I suppose the used Formula is either the "August–Roche–Magnus Formula" or the "Magnus Formula" to calculate the saturation-specific humidity since qs cannot be directly calculated from the Clausius-Clapeyron Equation? Please add the name of the used formula.

Response: Thanks for pointing out this inappropriate statement. The q_s calculations was based on August–Roche–Magnus Formula in this study. We have added the name of the used formula in the L.144 of revised manuscript.

L.144, L.146, Table S1: Please correct to "Clausius-Clapeyron".

Response: Thanks, correct.

L.147: If I understood correctly, you set the correction term ΨM to zero. Firstly, this needs to be stated in the manuscript. Secondly, under the mostly stable conditions in polar regions, the stability correction terms ΨM and Ψq cannot be neglected. Please include ΨM and Ψq in your latent heat flux calculation. E.g. following Holtslag and De Bruin (1988) for stable conditions (assuming ΨM=Ψq), and Paulson (1970) unstable conditions.

Response: Thanks for your rigorous consideration. In fact, we have used the correction term ΨM and *Ψq* when calculating latent heat flux calculations in the revised version. The ΨM and *Ψq* was set following Louis et al., (1979) for stable and unstable conditions. This chosen parameterization scheme is characterized by high calculating efficiency, compared to the iteration method like Paulson (1970) for unstable conditions. However, the correction terms were not added into the equations listed in the manuscript due to our carelessness. Also, there is no sufficient statement on the setting of ΨM and *Ψq* and the chosen of parameterization scheme in the Section 2.2.2. In the revised manuscript, we have corrected the equations and added some sentences to bring convenience for readers. The details are as follows: *The ΨM is calculated for stable, unstable and neutral boundary layer using the functions taken from Louis (1979).*

L.261: Typo: negative -31.01 ℃

Response: Thanks, correct.

L.269: Why is it relevant to mention stellar images here? Please remove or clarify.
Response: Thanks for this comment. Previous studies have used two different methods, i.e., sonic radar and seeing—the angular size of stellar images to determine the boundary layer height at Dome A. The measurements from sonic radar were only conducted from 2009 February to 2009 August, whereas the seeing—the angular size of stellar images were mainly performed during 2019. All of them confirm a median thickness of approximately 14 metres for the boundary layer at Dome A. Thus, we mentioned the stellar images here to ensure the credibility of estimation for the boundary layer height at Dome A.

L.291: I presume data from the model "ECHAM5-wiso" is used (Werner et al. 2011).
Response: No, when deriving the δ-T slope, we didn't use ECHAM5-wiso data. The compiled data of precipitation isotopic composition in Pang et al. (2019) were collected from previously published papers, including Landais et al., (2012), Touzeau et al., (2016), Stenni et al., (2016), Touzeau et al., (2016), Casado et al., (2016) and Ritter et al., (2016). These observations have been used to obtain the δ-T slope and then calculate the $\delta^{18}O_{s0}$ in winter season at Dome A. The ECHAM5-wiso data was then used to compare with the calculated $\delta^{18}O_{s0}$ in winter season at Dome A, to verify the calculations. These explanations have been stated in the previous response and added to the main text.

L.360: Diurnal changes, not cycles.
Response: Thanks, correct.

L.397: Neither of the three figures supports this statement since none shows isotopic values.
Response: Thanks for this valuable suggestion. We changed other 3 images (Fig.2, Fig.4c and Fig.4d) to support our statement in the revised manuscript.

L.407-408: Please reference Text S4 here.
Response: Thanks, added.

L.411: Liu et al. (2022) are not in the reference list.
Response: Thank you so much for your careful check. We have added this reference in the list.

L.413-416: "We noticed": Where are these diurnal changes shown? Or is it Liu et al. (2022) who show these changes? I furthermore assume that it is a 200‰ change in δD, not in d-excess.

Response: We very appreciate the reviewer's comment. The observed diurnal changes in water vapor isotopic composition at the nearest Dome A site are shown in Liu et al. (2022). For the d-excess, it has large diurnal variations with an amplitude of ~200‰ (Fig.2). The exact reason is still unclear, but could be the calibration drift caused by the extremely cold and dry conditions during the measurements. In the revised manuscript, we added the reference at L.413-416 and a sentence to make it more clear for readers.

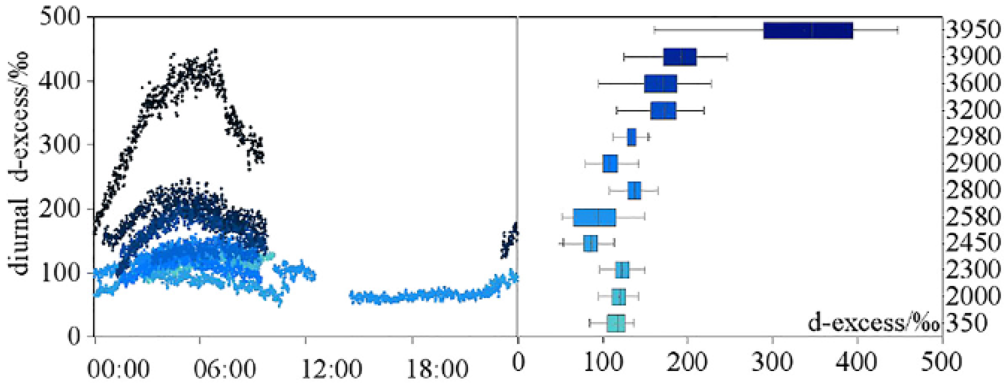

Figure 2: Diurnal cycles of water vapor d-excess during the measuring method from Zhongshan to Dome A (cited from Liu et al., 2022). The color successive change represents gradual distance variation from near coastal to interior inland Antarctica. All the signals are dominated by the presence of diurnal cycles with the isotope variation amplitude increased to interior.

L.416-417: The given numbers correspond to the diurnal variations, not the absolute values of δD. Please correct. In addition, where do the values for Kohnen and Dome C come from? Please update the text with the references.

Response: We would like to express our gratitude to the reviewer for pointing out these errors. The descriptions of δD observations have been reformulated according to the reviewer's suggestions. The related references have also been added to the end of each value. The revised sentences are as follows:

Our modeled δD variations at Dome A (28.78±19.06‰) are lower than the observed diurnal variations in water vapor δD at Kohnen station (36±6‰ from Ritter et al., (2016)) and at Dome C (38±2‰ from Casado et al., (2016)).

L.416-417: In the supplements is shown that an increased wind speed variability leads to a larger diurnal magnitude of the vapor δ 18O. I suspect the lower diurnal magnitude to be a consequence of the averaged meteorological input. This could be tested by running an additional simulation for Dome A with continuous meteorological input without distinguishing between cloudy and clear-sky days. Please add a sentence that references and discusses the results from Figure S3.

Response: Thanks for the reviewer's nice suggestion. The simulations at Dome A site have been done with continuous meteorological input from 5 to 16 January in each year (2006-2011). As shown in Figure 1 of this response, we find the water vapor $\delta^{18}O$ has a clear diurnal variation with higher values in sublimation period and lower values in deposition period. This pattern is consistent with the simulations using the averaged meteorological input. For the diurnal magnitude, the continuous simulations are indeed higher than those from the averaged meteorological input on several individual days. However, the diurnal variations in most of days are close or lower than 4.75‰, which was calculated with the averaged meteorological input in summer clear-sky days. This comparison suggests that the data processing method for model input will not cause a lower diurnal magnitude showing in water vapor $\delta^{18}O$.

Additionally, the discussion on the results from Figure S2 (Figure S3 in the original edition) and related reference have been added into the manuscript. The details are as follows:

Wind speed also plays a key role in driving isotopic variations at Dome A, because its increase can amplify the variations in latent heat, leading to more pronounced diurnal changes in water vapor and snow isotopic composition (Supplementary Text S4, Bréant et al., 2019).

L.420: Vertical convection or horizontal advection?

Response: It is vertical convection. This error has been corrected in the revised manuscript.

L.439-440: Figures 4 and 5 show no general vapor depletion. I suggest removing the second part of this sentence.

Response: Thanks for this suggestion. The second part of L.439-440 has been removed from the manuscript.

L.463-464: The presented results and figures do not provide sufficient evidence that allows a statement regarding the long-term isotopic impact of vapor exchange. Please provide evidence or remove this statement.

Response: From the diurnal simulations, it is apparent that the surface snow $\delta^{18}O$ and δD would become enriched compared to fresh snow in summer, while in winter surface snow isotopes would be depleted compared to fresh snow. If the diurnal changes could be accumulated and other isotopic modifications were not taken into account, an amplification of the snow isotope seasonality would be caused by atmospheric vapor-snow exchange. This assumption indeed needs to be supported by more calculations. We originally planned to do the seasonal simulations at Dome A, but these work are out of the scope of this paper. Thus, the statement on long-term isotopic impact of vapor change has been removed from this manuscript right now. We expected to provide more evidence to support this statement in the future work.

L.475-477: The evidence for this statement is missing.

Response: Thanks for this comment. Based on the simulations in the Figure 4 and

Figure 6, we found that the snow isotopes become more enriched after a 24-h period during summer. In contrast, the winter snow layer has an opposite change in $\delta^{18}O/\delta D$ on the diurnal scale. If other post-depositional processes and precipitation intermittency are not considered, the diurnal changes in snow isotopes induced by atmosphere-snow vapor exchange will be accumulated under the ideal condition. Considering the opposite effect between summer and winter, the annual net effect from atmosphere-snow vapor exchange will be small on the snow isotopes. While this inference holds true from a qualitative perspective, it remains to be more explored in the future work. These explanations have been added into the discussion.

Figure 4d: The line for the snow isotopic composition is missing in the figure.
Response: Sorry for this mistake. We have added the line representing snow isotopic composition in the Figure 4d of revised manuscript.

Figure 4 and Figure 5: Please choose the same y-axis ranges for Fig. 4 and 5.
Response: Thanks for this valuable comment. After adjustment, the y-axis ranges for Figure 4 are in accord with those of Figure 5.

Text S4, L.123-126: Figure S2 shows that the vapor $\delta^{18}O$ is strongly underestimated in Case II (turbulent mixing for Ri¡0.1). The written text suggests the opposite.
Response: We apologize for our carelessness in the Text S4, L.123-126, and Figure S2. The Case I represents the simulations under the turbulent mixing for Ri<0.1, whereas the Case II shows the modeled results when Ri<0. In the revised edition, we have corrected this error in the Figure S1 (Figure S2 in the original edition).

Figure S3: Typo in legend of ”Water vapor δ 18O-Case II”
Response: Thanks, corrected.

End of the responses to Reviewer #2

Reference

Dietrich, L. J., Steen-Larsen, H. C., Wahl, S., Jones, T. R., Town, M. S., & Werner, M.: Snow-Atmosphere Humidity Exchange at the Ice Sheet Surface Alters Annual Mean Climate Signals in Ice Core Records. Geophysical Research Letters, 50(20), e2023GL104249, doi:10.1029/2023GL104249, 2023.

Liu J., Du Z., Zhang D., Wang S.: Diagnoses of Antarctic inland water cycle regime: Perspectives from atmospheric water vapor isotope observations along the transect from Zhongshan Station to Dome A, Frontiers in Earth Science, 10, doi: 10.3389/feart.2022.823515, 2022.

Louis, J.: A parametric model of vertical eddy fluxes in the atmosphere, Boundary-Layer Meteorology, 17, 187–202, doi: 10.1007/BF00117978, 1979.