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 are grateful to you and the reviewers for the constructive comments and suggestions which significantly improve the manuscript. In the revised manuscript, we have made substantial revisions according to the comments/suggestions. Below we briefly described the main comments and our responses. Detailed responses can be found in the point-to-point response file

One of the main comments/suggestions was on the components and the physical mechanisms of the model. The reviewers suggested the exchanges between the atmospheric boundary layer and the free troposphere should also be considered in the model. In response, we added a third box into the model structure and then modified the calculations of mass and isotopic balance during atmosphere-snow vapor exchange accordingly. The calculations of the latent heat flux and humidity in the model were also modified according to the reviewer's comments. After these modifications, the model performance was improved as now it was able to reproduce the observed isotope changes in surface snow at Dome C, in addition to the good agreements between the modeled and observed isotope changes in vapor water. However, although the absolute values of the modelled results are changed, we note the patterns of the results stay the same, so as the conclusion.

The other main comment was on the designs of the simulations, i.e., how long the simulations should be performed, and what types of input data (i.e., stacked means or daily data) should be used. In the revised manuscript, for Dome C simulations we used daily meteorological data during the studied period as input. However, for Dome A simulations, in order to obtain representative results for summer clear-sky, cloudy and winter conditions, we still used the stacked means as input and focused on the diurnal variations and changes.

We confirm that all authors have approved the revised manuscript and 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

This manuscript considers the exchange between water molecules between the firn and the atmosphere, and the impact it can induce on the change of isotopic composition in extremely low accumulation regions of Antarctica. Using the results from Dome C as an analogue for Dome A is a clever strategy that can yield promising results to how to explain the impact of surface processes on the future Dome A ice core. The study takes into account the variations of stability of the atmosphere with systematic calculations of the Richardson number and developed three case studies associated with two sets of summer conditions (clear sky and cloud), and one set of winter conditions.

While the authors used a rather classical set of equations to evaluate the isotopic exchanges during sublimation and condensation, it seems not pertinent here, as it ignores major contributors to the boundary layer processes and only consider the system as a closed box without exchange with the free atmosphere. As a result, the results do not match the observations that were made for the surface snow isotopic composition at Dome C, even though, it is supposed to be the case study used to parametrise the model.

I suggest profound modifications to the model, which take into account exchanges between the atmospheric boundary layer and the free atmosphere on top of the surface processes, and which would match the surface snow changes, at least in order of magnitude, before considering the manuscript for publication.

Response: We greatly appreciate the reviewer's insightful comment on the physical mechanism of our model. We agree that realistically exchanges between the atmospheric boundary layer and the free troposphere on top of the surface processes should be considered. It was our originally plan that we wanted to explicitly focused on how much changes on snow isotopes can be induced by processes at the air-snow interface alone. This may not reflect the real changes but can reveal the most potential effects associated with the processes at the air-snow interface. Thanks to the reviewer's suggestion, that we realized that it might be better to include the free troposphere which will make the result more comparable with the observations. Therefore, in the revised manuscript, we included the mass exchange between the boundary layer and the free troposphere by adding a third box as illustrated in the revised Figure 1. The calculations and equations were also changed to reflect the modifications to the physical mechanisms in the model. But we wanted to note that, with including the effects of exchanges between the boundary layer and free troposphere, the main conclusion of the manuscript doesn't change (the magnitude of modeled changes are affected but still in the same direction).

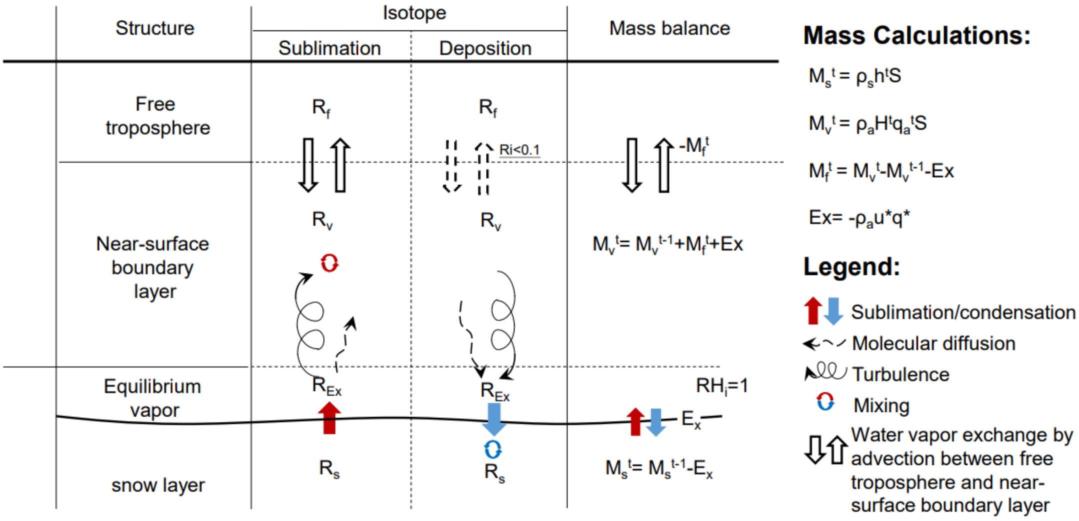

Figure 1: Schematic diagram of the box model used in this study (Revised version).

Major Comments:

1) *The box model developed by the authors was parametrised against vapour measurements obtained at Dome C, in order to compensate for lack of measurements at Dome A. The outputs of the model predict changes of vapour isotopic composition that seem realistic, but it is not the case for the changes of snow isotopic composition for which the variations are extremely small (less than 0.02‰) while the observed changes are around 2‰ during a typical night (Casado et al., 2018). The relative changes of snow and vapour isotopic compositions during a typical clear sky night were modelled in this manuscript, and suggested that a closed box model (which is de facto what the authors have implemented since no exchanges between the free atmosphere and the boundary layer are taken into account) is not realistic for this type of event.*

Response: Thanks for pointing out this. Indeed in the original model framework, the modeled results on snow isotopic changes cannot match the observations. In the revised manuscript, this is addressed by including the effect of exchanges between boundary and free troposphere. In particular, the new results indicate that simulated changes in snow isotopic composition are significantly larger than the original model (i.e., ~ 0.02‰ for $\delta^{18}O$) at Dome C. Especially, for the case the reviewer mentioned, i.e., a typical night with a frost event on Jan 6-7th, 2015, the diurnal changes of newly simulated results between the maximum and minimum can reach 2‰ for snow $\delta^{18}O$ (as demonstrated in Figure 2). This magnitude is in line with the observations for snow isotopes from Casado et al. (2018) which is ~ 2‰.

In the revised manuscript, we have re-run all simulations under Dome C conditions and three different cases at Dome A using the adjusted model. The simulated results within a 24-hour period were displayed in Figures 3-6 of the revised manuscript.

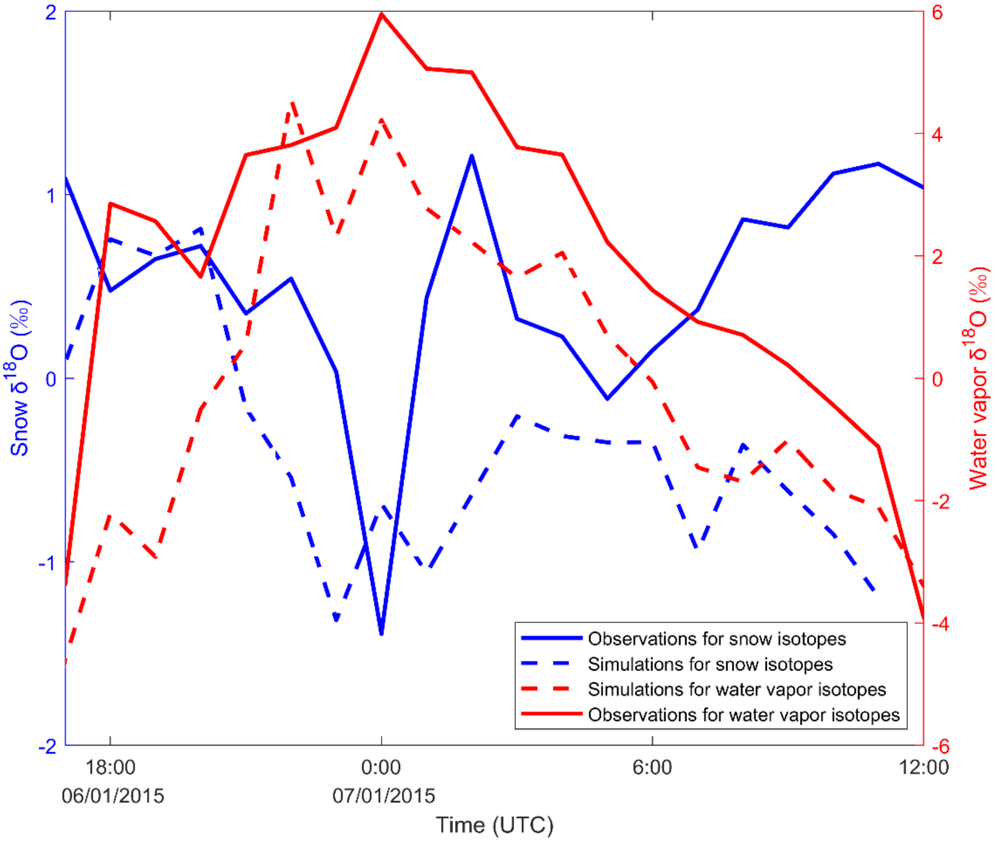

Figure 2: The changes of snow isotopes and water vapor isotopic composition (relative to the average value within a 24-h simulation) in the boundary layer in Jan 6-7th, 2015 at Dome C.

2) *Another aspect that suggests that exchanges between the boundary layer and the free atmosphere must happen is the Richardson number. Indeed, for negative Richardson numbers, the atmosphere must be quite convective, which suggest that the boundary layer exchanges with both the surface snow and the free atmosphere.*

The atmosphere is qualified as stable for any positive Richardson number, yet, it seems that some studies suggest that some amount of mixing remains quite strong for 0 < Ri < 0.1 (Zilitinkevich et al., 2007) . This could be discussed.

Response: Thanks for this valuable suggestion. In our original simulations, we assumed that unstable conditions for atmosphere stability only existed under negative Richardson numbers. Based on this assumption, we considered how mixing between the boundary layer, surface snow, and the free troposphere can affect the water vapor isotopic composition in the near-surface atmospheric layer and snow isotopes during the warming phase with negative Richardson numbers. However, as pointed by the reviewer that Zilitinkevich et al. (2008) suggested mixing can occur under positive Richardson numbers as well. If this is true, our original simulations for the water vapor isotopic composition in the near-surface atmospheric layer may be underestimated in the cooling phase.

To test the relationship between mixing occurrence conditions and Richardson numbers, we ran simulations for Dome C taking into account mixing when Ri<0 and Ri<0.1. As shown in Figure 3, the case with Ri<0 (Case II) indeed underestimates the water vapor isotopic composition in the near-surface atmospheric layer during the cooling time. Based on this comparison, in the revised manuscript, we incorporated mixing into the modeling once Ri<0.1 (Case I) in addition to the original consideration with Ri<0. Discussion on taking into account Ri<0.1 was added in supplementary information (Texts S4) of the revised manuscript, and in the main text all results were updated with consideration of the mixing when Ri<0.1.

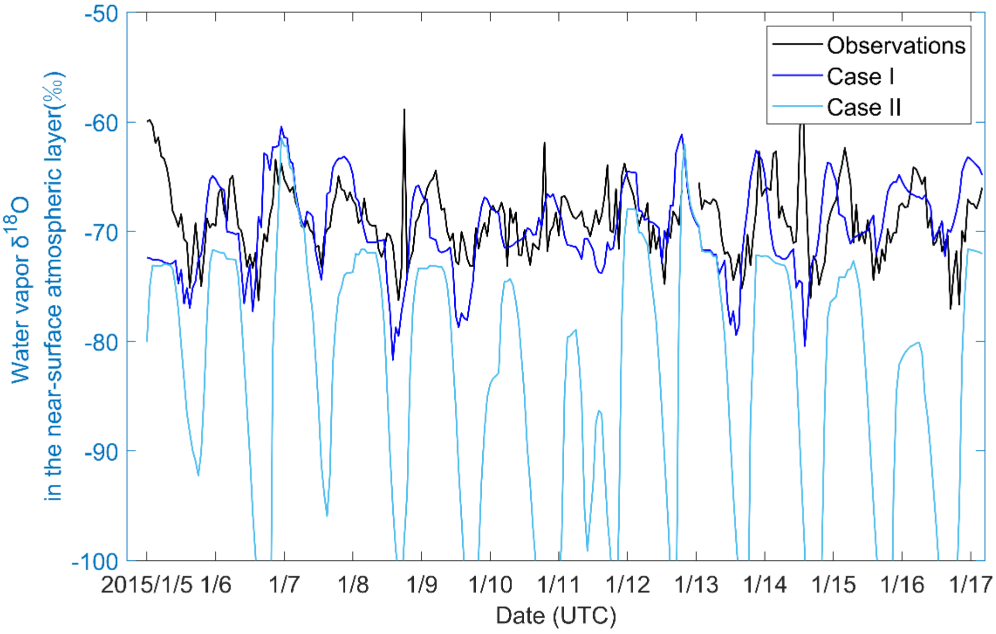

Figure 3: The comparison of water vapor isotopic composition between the simulated and observed changes at Dome C. Two simulated cases are presented here to discuss the occurrence condition of mixing. In case I, the mixing is assumed to only happen when Ri<0 in the cooling phase, while case II also considers the occurrence of mixing when Ri<0.1 in the cooling phase.

3) *Some limited vapour data exist at Dome A (Liu et al., 2022). While these data might be difficult to compare to your results, in particular consider how high the d-excess is, which could be associated with calibration issues, it should be discussed.*

Response: Thanks for this suggestions. Actually before finalizing the manuscript, we have discussed with the leading author of the Liu et al. (2022) study, but we noted that due to the harsh environment, direct observations of water vapor as the Liu et al did is difficult and the calibration can induce large issues. In addition, their measured sites are actually not exact the same at Dome A (~100 km away). In the end we didn’t choose to compare this dataset. But since the reviewer asked, in the revised manuscript, we compared our simulations at Dome A with the data of water vapor δ^{18}O, δD, and d-excess from Liu et al. (2022). We found that both our simulations and observations exhibit diurnal patterns, with high values occurring during the warming phase (daytime) and low values during the cooling phase (nighttime). However, we note that the magnitude of the observed diurnal changes in water vapor δ^{18}O and d-excess at sites near Dome A are very large, over 40‰ and 200‰, respectively. This could be due to calibration drift caused by the extremely cold and dry conditions during the measurements at the nearest Dome A site.

Therefore, in Section 4 of the revised manuscript, we only qualitative compare and discuss the similarities and/or differences between simulations and observations, without delving into quantitative details.

4) *Considering how fundamental these changes are, an updated version of the manuscript could have completely different conclusions.*

Response: We really appreciate the reviewer's comments. By including the effect of exchanges between boundary and free troposphere, the modeled results indeed differ a lot compared to the original model. However, the modeled changes in snow and vapor isotopes are still in the same direction (the magnitude or absolute values differ), and the main conclusion stays the same as that the air-snow exchange would lead to diurnal variations in atmospheric water vapor δ^{18}O and δD by 4.75±2.15 ‰ and 28.79±19.06 ‰ under summer clear-sky conditions at Dome A, with corresponding diurnal variations in surface snow δ^{18}O and δD by 0.81±0.24 ‰ and 1.64±2.71 ‰, respectively. These values become smaller compared to those in the previous simulations. After 24-hour simulation, snow water isotopes were enriched under clear-sky conditions. However, there is no or very little enrichment for snow water isotopes under cloudy conditions, which is different with the previous simulations. Under winter conditions at Dome A, the model still indicates the diurnal change in atmospheric and surface snow water isotopes are not significant, but the model predicts more or less depletions in snow δ^{18}O and δD in the period of 24-hour simulation, opposite to the results under summer clear-sky conditions. This suggests that the air-snow vapor exchange tends to enlarge snow water isotope seasonality.

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.

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.

Zilitinkevich, S.S., Esau, I.N.: Similarity theory and calculation of turbulent fluxes at the surface for the stably stratified atmospheric boundary layer, Boundary-Layer Meteorology, 125, 193–205, doi: 10.1007/s10546-007-9187-4, 2007.

Response to Reviewer #2's comments

General comments

1) *My main concern is that the way the authors word their conclusions and their title suggests they provided model estimates of the diurnal variations in the snow and vapor isotopes. In fact, presented simulations are driven by average diurnal cycles of the meteorological parameters. Thus, instead, the authors provide the impact of an average day on initialized snow and vapor isotopes. The presented current results show how a given initial surface snow and vapor isotopic composition could develop within the first 24 hours when applying water vapor exchange.*

It is unclear to me why the authors didn't run the simulation based on the meteorological input of individual days instead of stacking and averaging the input data. This limits the simulation time to only 24 hours. Such a short time does not allow for the development of the snow surface over several days. I would consider a minimum of a week spinup time to perform a model simulation in a more equilibrated state as could be expected in nature.

The intuitive approach to obtain an estimate of the average diurnal impact on the isotopes would be to run a longer simulation over several days and give the average daily impact. It seems to me that the authors have the needed data and tools to provide a model simulation over several days, as suggested above. This will improve the manuscript's relevance and provide better applicability of their results to explain observed changes in the snow isotopic composition.

Response: We appreciate the reviewer's insightful comments. In the original manuscript, we chose to use the mean stacked conditions to conduct simulation since we wanted to highlight the effects of air-snow exchange in a general case. But in order to avoid confusion, in the revised manuscript, the simulations were conducted using continuous meteorological input for each individual day during the studied period at Dome C, where the model was run during the entire studied period (Jan 5th to Jan 16th, 2015), and the simulated results were stacked and averaged to evaluate the changes in snow and water vapor isotopes within a 24-hour period, as shown in Figure 3 of the revised manuscript. The model performance in water vapor isotopic variations is better than the simulations in the original manuscript. For snow isotopic composition, the diurnal evolution of simulated results can basically match with observations in the order of magnitude during a typical frost event (Figure 2 in this response).

In the Dome A simulations, however, the selected days for clear-sky, cloudy, and winter conditions were not continuous, making it difficult to conduct simulations as was done for Dome C. Instead, we were only able to use the model for one day to simulate the diurnal changes in snow and water vapor isotopes, after a week of spin-up time. This allows to evaluate the effects of air-snow vapor exchange under representative meteorological conditions. It is important to note that the input meteorological conditions and latent heat flux during both the spin-up time and the simulated period at Dome A were obtained from stacking observations or calculations on selected days, due to the non-continuous clear-sky and cloudy days in the studied period. Furthermore, the choice of the modeling running day and duration can significantly influence the final results of snow and water vapor isotopic composition, as meteorological conditions and latent heat flux vary significantly between two different days within a season. To mitigate this effect, it is recommended to use the averaged meteorological conditions to run simulations at Dome A. These approaches at least provide some, on average, quantitative information on the isotopic effects of atmospheric-snow water vapor exchanges at Dome A.

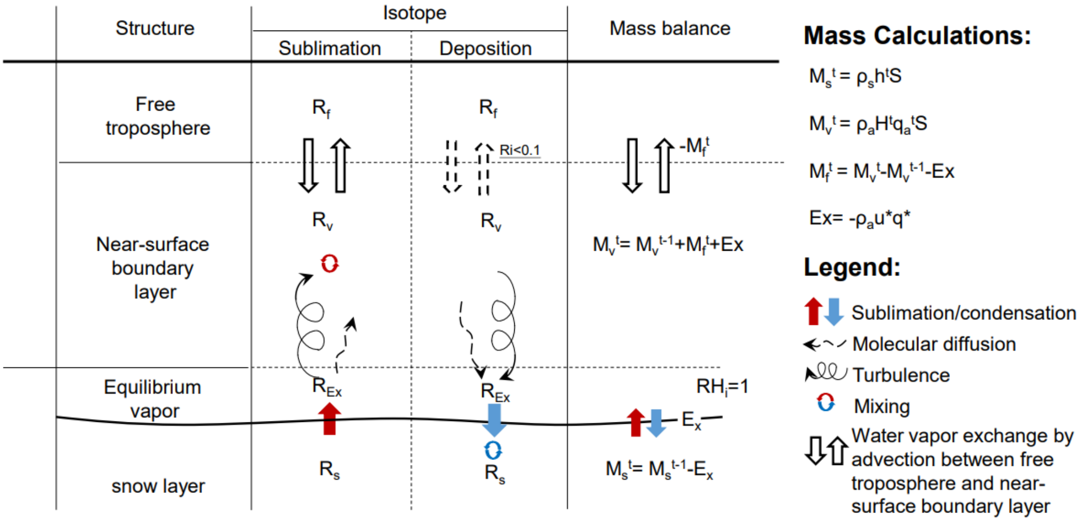

Figure 1. Schematic diagram of the box model used in this study (Revised version).

2) *Secondly, there are errors in the calculation of the latent heat flux as well as the calculation of the isotopic flux. Please see the details below. In addition, to my understanding, the latent heat flux is calculated based on already stacked and averaged meteorological data. Since the latent heat flux is non-linearly dependent on these meteorological parameters, the resulting flux based on the averages can diverge severely from a diurnal average of the latent heat flux resulting from hourly calculations. The presented simulations need to be re-run using the corrected latent heat flux calculation.*

Response: We would like to express our gratitude to the reviewer for bringing to our attention the errors in the calculations of latent heat flux and isotope flux. We have taken into account the detailed comments provided in this response and have made the necessary corrections to the equations for these parameters in the revised manuscript.

As part of our revisions, we have also changed the calculation method for the latent heat flux and isotope flux for Dome C. Instead of using stacked and averaged meteorological data within 24 hours, we now use continuous meteorological input for individual days over the studied period. For the Dome A simulations, the latent heat flux calculations remain the same as the Dome C simulation cases. However, the isotope flux was obtained by stacked and averaged latent heat flux data due to the selection of cloud conditions (Comment #1). These changes in the calculation method can provide more accurate changes in the flux parameters on a diurnal scale. Furthermore, the uncertainties of these parameters can be easily estimated by calculating the standard deviation of the simulated results on the given days. More details on this can be found in Comment #52 of this response.

3) *Another concern is that even when the above-mentioned errors in the latent heat flux calculation are corrected, the conditions for the Monin-Obukov similarity theory (MOST) are often violated under polar conditions. The present study does not discuss the quality of the calculated latent heat flux. If the authors pursue the goal of providing as realistic estimates of the water vapor exchange on the isotopes as possible, they have to make sure that the quality of the driving parameter, the latent heat flux, is well evaluated for similar conditions.*

Response: Thanks the reviewer for this comments. Indeed, the eddy covariance (EC) technique is a more robust method for quantifying latent heat fluxes and calculating isotopic fluxes at the atmosphere-snow interface, as demonstrated by Whal et al. (2021). However, this technique heavily relies on specialized measurement instruments, making it difficult to determine the latent heat flux in the absence of such instruments. As a result, high-quality latent heat flux data is not available at most polar sites.

Alternatively, the Monin-Obukhov similarity theory (MOST) are widely applied in polar regions because it calculates the latent heat flux based solely on meteorological parameters. While it seems not to be very suitable under polar conditions especially in winter, some previous studies have used the bulk method and MOST to calculate surface fluxes and the results were reasonable. For example, the King and Anderson (1994) study indicated that MOST can well describe the winter heat and water vapor fluxes at the Halley station of the Brunt Ice Shelf. Van den Broeke et al. (2005) calculated the year-round turbulent fluxes with MOST along a traverse line from coastal to inland region in Dronning Maud Land, Antarctica. Based on these, we think it is acceptable to use MOST and the bulk method if we intend to predict the potential mass and isotope changes that can be caused by atmosphere-snow vapor exchange.

When it comes to the quality of model calculations, the key factor is whether the model has been built using appropriate physical processes and meteorological parameters. If such a model can accurately reproduce observations at Dome C, it is highly likely that it will also be able to make predictions for Dome A within some degree of uncertainty. We hope we can have more observational data from Dome A to constrain the model, which is on progress but not available currently.

Detailed comments

1) *L20-22: This is misleading because the given values refer to the simulated changes when applying one average summer day. The way it is currently written suggests that the given values correspond to the average daily impact on the isotopes when simulating many different summer days.*

Response: Thank you for bringing the misleading information to our attention. We have revised the manuscript by re-simulating the continuous variations for snow isotopes and water vapor isotopes at the atmosphere-snow interface. Using the new simulated results obtained from Dome C and Dome A, we have calculated the daily impact of atmosphere-snow water vapor exchange on water isotopes. This was done by averaging the hourly values during summer clear-sky, cloudy and winter days. Based on these new results, we have rewritten the Abstract to reflect our findings accurately.

2) *L26: I disagree with this statement. Although, in contrast to summer, the meteorological variables don't seem to have a diurnal cycle in winter, the simulation of the isotopic changes shows similar magnitudes to the simulated changes in summer. How do you come to the conclusion that there are no relevant isotopic changes simulated on a diurnal scale in winter? Please clarify what this statement refers to. In that context, please reconsider the use of the term "diurnal cycle" or "diurnal pattern" in the manuscript. For me, a diurnal cycle is a repetitive pattern, i.e., similar values are found at the same time of the day. However, the authors use that term when describing the simulated isotopic change within 24 hours (e.g., L26, L295, L296, L310, L314, L319, L328, L330, L335, L339, L353-355, L361, L403,. . .). But since the simulated isotopic values are different at 00:00 and 24:00 of the simulated day, the isotopes do not show a diurnal cycle but a change during one day.*

Response: Thanks for pointing out this. Our simulations at Dome A indicate that the water vapor isotopic composition during winter exhibits similar magnitudes of change to those observed during summer. However, the variations in snow isotopic composition during winter are significantly smaller than those observed during summer. This difference can be attributed to the more pronounced changes in meteorological conditions and latent heat flux that occur within a 24-hour period during summer days. As a result, we have revised the Abstract to emphasize the significance of meteorological conditions on the impact of atmosphere-snow water vapor exchange. Additionally, we have rephrased the sentences in L26 to provide a more explicit statement in the revised manuscript.

"Under winter conditions at Dome A, the model predicts that more or less depletions in snow δ^{18}O and δD can be caused by atmosphere-snow water vapor exchange in the period of 24-hour simulation, opposite to the results under summer conditions.."

We also appreciate the feedback regarding the misnomer and have thus replaced the term "diurnal cycle" or "diurnal pattern" with the more accurate term "diurnal changes" or "diurnal variations" in the revised manuscript.

3) *L114-116: This sentence lacks clarity, please reformulate it. The calculation of sublimation and deposition is based on the same formula in the model, so why are two different formulations used here? And please change "followed by a mixing procedure and then uptake of surface snow", e.g., to "and the deposit is mixed into the snow surface layer".*

Response: Thank you for your comment. Previous studies have shown that there are differences in isotopic fractionation between sublimation and deposition (Ritter et al., 2016; Hughes et al., 2021). It is important to note that during deposition, the dominant process is equilibrium fractionation, whereas sublimation is significantly influenced by kinetic fractionation, except for equilibrium fractionation. Therefore, it is necessary to use two different formulations to describe the isotopic balance between snow and water vapor in Section 2.2. In case of mass changes in sublimation and deposition, the same formula as shown in Eq: (1) can be used.

However, we agree that the statement mentioned in the comment was confusing, and we have rewritten it in the revised manuscript as follows:

"During sublimation, water vapor is released from snow, transported into the atmospheric layer via turbulent mixing and molecular diffusion, and immediately mixed with the water vapor already in the boundary layer. During deposition, water vapor is influenced by aerodynamic resistance from turbulence and molecular diffusion, and the deposit is mixed with the surface snow layer."

4) *L124: What does "mainly" and "etc" refer to? Are further input parameters required to run the model? If so, please provide a complete list of all input parameters. If not, please remove the "etc".*

Response #4: Remove, Thanks.

5) *L129-130: Please provide a sufficient discussion of the uncertainty of the calculated latent heat fluxes beyond what is presented in S2 in the supplements. Is there a way to evaluate the quality of the latent heat flux calculations using another dataset (e.g., measured with an eddy covariance system)?*

Response: Thanks for your comment. We have made significant updates to the revised manuscript, particularly regarding the estimation method for the uncertainty of the latent heat flux calculations. The original Monte Carlo method has been replaced with a more straightforward approach that involves stacked and averaged simulations over multiple days. This new method relies on continuous calculations for the latent heat flux using meteorological input data from individual days. We have provided a detailed explanation of this new method in the Texts S2 of the supplements (details can be seen in Comment #52), where we also analyze the impact of the uncertainty of the calculated latent heat fluxes.

It is crucial to assess the accuracy of the latent heat flux calculations. However, there were no available measurements from the eddy covariance system to validate the calculations at Dome A. Therefore, we had to rely on comparing our calculations with those in previous publications. Ma Y. et al. (2011) had previously estimated the latent heat flux at this site. According to their findings, the latent heat flux calculations exhibited significant cycles on the diurnal scale and its diurnal ranges are 2.7 W/M^2 during summertime. These features and the order of magnitude for latent heat flux are consistent with the calculations in our study. Moreover, both the previous studies and our study found that the diurnal changes in latent heat flux are not significant during winter days. Based on these similarities, we are confident that the latent heat flux calculations in our study are reliable.

6) *L134, Eq 1.: The formula that the authors use to calculate the latent heat flux is not correct. Following Berkowicz and Prahm (1982) (B&P82) from solving Eq. 22 for LE, then using H from Eq. 11d with u and $\Theta *$ from Eqs. 11a and 11b, $\Delta u = u_{air} - u_{surface}$ with $u_{surface} = 0$, and $\gamma = cp/Ls$ you obtain:*

$$LE = \rho L_s \kappa^2 \cdot \frac{u_{air}}{log\left(\frac{z_{u,a}}{z_{u,0}}\right) - \Psi_m\left(\frac{z_{u,2}}{L}\right) + \Psi_m\left(\frac{z_{u,1}}{L}\right)} \cdot \frac{q_a - q_s}{R \cdot log\left(\frac{z_{t,a}}{z_{t,0}}\right) - \Psi_h\left(\frac{z_{t,2}}{L}\right) + \Psi_h\left(\frac{z_{t,1}}{L}\right)} \tag{1}$$

Additionally, Ls should not show up on the right side of the formula when giving the expression for LE/Ls. Please correct the theory of the box model calculation and re-run all simulations of the study. Furthermore, in Eq. 1, in L134 and L138: There is no time derivative given in B&P82, they use Δ to indicate the vertical gradient. When using the MOST, the latent heat flux depends on the wind speed as well as the vertical humidity gradient (qa-qs).

Response: We are grateful to the reviewer for this valuable suggestion. Based on this feedback, we have made necessary corrections to Eq: (1) in the revised manuscript. However, for simplification of calculation, we ignored the corrected parameters in Eq: (1) during modeling. Using the revised model, we generated new simulations and the updated results are presented in Figures 2-6 of the main text (at the end of this response).

7) *L135: Please change "ρ_V " to "ρ_a".*

Response: Thanks, correct.

8) *L145: Where does the chosen value of 0.244 mm for the roughness length come from? The latent heat flux is highly sensitive to the choice of the roughness length. Please provide a sensitivity analysis of the simulated results to the choice of a range of roughness lengths, e.g., 0.1 mm to 2 mm.*

Response: The roughness length (z_0) at Dome A was calculated in this study using the least square method and wind observations at three levels (1 m, 2 m, and 4 m) under neutral conditions, which typically vary between 10^{-5} to 10^{-3} m. To simplify the calculations, a constant value of $z_0 = 2.44 \times 10^{-4}$ m was used in the modeling. This estimate was determined using all wind speed data (397 groups) under neutral conditions. It is worth noting that z_0 in this study is close to the previous calculation of 1.45×10^{-4} m from Ma et al., (2011).

We acknowledge the importance of z_0 value in obtaining accurate results. In response to the reviewer's suggestion, we have added a sensitivity test for z_0 in the supplementary section (Texts S5). Additionally, we have provided detailed explanations and cautions for z_0 calculations in the supplementary.

The added texts S5 are shown as follows:

"Besides the initial parameters, changes in z_0 might influence the isotopic effects of atmosphere-snow water vapor exchange. Thus, we also conducted the sensitivity test for z_0 and run for a 24-h period under summer clear-sky conditions at Dome A. The test was focused on the sensitivity of surface snow and water vapor $\delta^{18}O$ to varying z_0 between 0.01 to 10 mm. All other simulation settings were the same as in Section 2.2.4 of the main text.

The results of sensitivity tests for z_0 are shown in Fig. S4. As shown in the figure, the magnitude of the diurnal variations in water vapor $\delta^{18}O$ ($\delta^{18}O_v$) is very sensitive to z_0 (Fig. S4a) because z_0 determines the latent heat flux. This is consistent with Ritter et al. (2016) who pointed out that diurnal variations in water vapor isotopic composition decrease with the increase of boundary layer height. The magnitude of diurnal variations in snow $\delta^{18}O$ ($\delta^{18}O_s$) is also sensitive to z_0 (Fig. S4b and S4c). However, the changes in $\delta^{18}O_s$ is smaller than $\delta^{18}O_v$."

9) *L172: Above (in L138), RH_i is defined as the relative humidity over ice, not for the specific humidity.*
Response: Thanks, correct.

10) *L182, L183: The "h" in Merlivat and Jouzel (1979) (M&J79) does not refer to the relative humidity of the air, but to the relative humidity of the air with respect to the surface temperature, i.e., h =qair qsat,surface (instead of RHair =qair qsat,air). The formulation in M&J79 is really confusing, but their qs in the formula of h =q/qs (below Eq. 9 in M&J79), in fact, refers to the "saturated specific humidity at the air-water interface (z=0)", i.e., the saturation specific humidity with respect to the surface temperature, while q is the air specific humidity. It is, thus, not correct to use the relative humidity here, but instead h =qair qs,surface. If this was not the case in the simulations, please correct and re-run them. Otherwise, please be more precise in the description of RH_i.*
Response: Thanks for the valuable feedback provided by the reviewer regarding the term 'humidity'. We have carefully reviewed our equations and made the necessary corrections based on the definition provided in Merlivat and Jouzel (1979). The revised equations have been used to generate new simulated results. Furthermore, we have improved the clarity of the description of RH_i in the supplementary material. For more information on the corrections made, please kindly refer to our response to Comment # 51.

11) *L450 and 454. The authors state that the air temperature is controlling the isotopic fraction. This is not correct. It is the snow surface temperature, which is governing the isotopic fractionation. L189: Where does the expression for Rt EX come from? Because Eq. 2 in Jouzel and Merlivat (1984) is RtEX =af (Rtv + 1) − 1. Please correct this*
Response: Thanks for pointing out these mistakes. The necessary corrections have been done in the revised manuscript, including revising the Eq: (13) and updating L450 and L454.

$$R^t_{Ex} = \alpha_f(R^t_v + 1) - 1 \qquad (13)$$

12) *L200-201: Casado et al. (2016) does not present a snow dataset. If the authors refer to the Touzeau et al. (2016) dataset, please add the reference.*
Response: Thanks, we have added the reference.

13) *L209: I suggest replacing "representative" with "average". It was initially unclear to me what the authors meant by "stacking" the observed cycles.*
Response: We agree. The "representative" has been replaced by "average" in the revised manuscript.

14) *L210: Please remove the "e.g." and "etc." in the parenthesis since the given parameters are the only ones that can be downloaded from the CALVA program.*
Response: Thanks, delete.

15) *L211-216: Is there no surface temperature record available for DOME-C? And if not, why is the surface temperature calculated from ERA-5 model long wave data instead of using the ERA-5 model output of the surface temperature?*

Response: During the modelled period, surface temperature data were available for Dome C, as measured by a Campbell Scientific IR120 infrared probe and reported by Casado et al. (2016). In the revised manuscript, we used these observations as input for simulations at Dome C instead of the calculations based on the method from Brun et al. (2011).

However, for Dome A, surface temperature observations were not available from 2005 to 2011. Therefore, we used the method from Brun et al. (2011) to calculate surface temperature (Eq: (17) in the main text). We chose this method because it can accurately represent the observations at Dome C. To validate the calculations at Dome A, we compared them with observed 10cm firn temperature at the same location. The calculations matched well with the observed snow temperature for the top 10cm layer, as shown in Figure 2a.

$$Ts = \left(\frac{LW_{up}+(\epsilon-1)LW_{dn}}{\epsilon\sigma}\right)^{0.25} \qquad (17)$$

Furthermore, the direct output of surface temperature from the ERA-5 model can also be used as input for our model because the ERA-5 model output at Dome C is comparable to the surface temperature calculations based on the method used in this study, as well as the long-wave radiation data from the ERA-5 reanalysis data (Figure 2b).

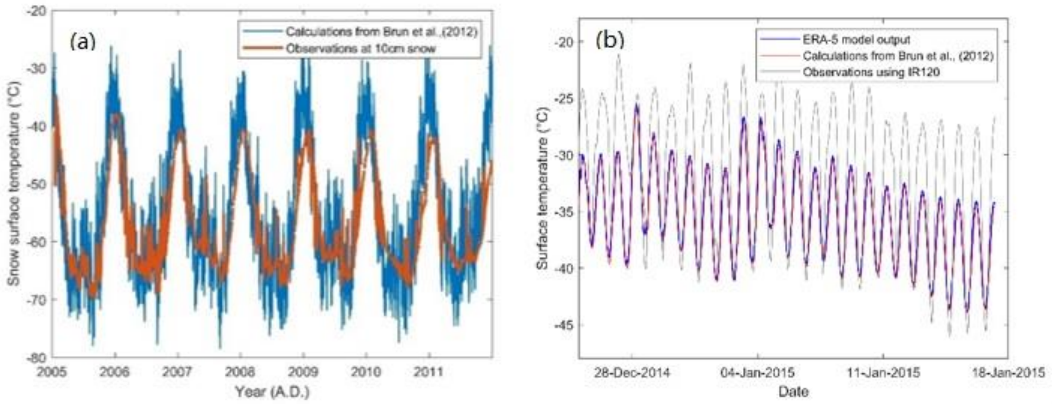

Figure 2. The comparison of the T_s results of different methods. (a) The calculated T_s and the observed snow temperature for top 10 cm snow at Dome A, during the period of 2005-2011 (b) The calculated T_s, the ERA-5 model output of T_s and the observed T_s at Dome C, during the period of 5th-16th January, 2015

16) *L214: An emissivity of 0.93 seems relatively low to me. Please indicate where this value originates from.*

Response: Thanks for this comment. The value of 0.93 for snow emissivity was cited from the Doctoral thesis of Ma et al. (2012), which calculated the surface snow temperature at Dome A. This value is lower than the snow emissivity of 0.99 at Dome

C (Brun et al., 2011; Vignon et al., 2017). Despite the significant difference between these two values, we still use the value of 0.93 as the snow emissivity for Dome A simulations. We have now included this difference between Dome A and Dome C in the revised Table S1.

17) *L216-217: The latent heat flux is calculated based on the averaged meteorological parameters. In my view, it makes more sense to calculate the latent heat flux based on the hourly data and (if needed) stack and average it afterward.*
Response: We concur that the fluctuations in latent heat flux over a period of multiple days are significant for subsequent simulations related to water isotopes. To that end, we recalculated the latent heat flux and then computed the average, which is illustrated in Figure 2 of the primary text (please see the revised version at the end of this response).

18) *L217: Please remove the "etc." if no further data is used.*
Response: Thanks, remove.

19) *L220: An average snow density from 2m+ deep snow pits might not be appropriate for the top 1.5 cm. Please provide a sensitivity analysis of the simulation using a range of realistic surface snow densities.*
Response: Thanks for the suggestion. We will test the isotopic values in response to varying snow density at Dome A and add results to the Section 2.2.4 and Section 3.4 of the main text.

20) *L234: What does "to fully assess the accumulated isotope effects of atmosphere-snow water vapor exchange." mean? Please rewrite this sentence to clarify on this.*
Response: In order to illustrate the impact of cloud presence on the simulation results at Dome A, we have conducted two simulated cases: one with cloud and one without cloud. However, we understand that the original sentence in L234 may have been unclear. Therefore, we have completely rewritten the sentence as follows:
"Therefore, in the model simulations for Dome A, we simulated two representative cases with and without cloud (i.e., cloudy vs. clear-sky conditions) in order to accurately assess the isotopic variations associated with atmosphere-snow water vapor exchange."

21) *L250-251: I hardly see any diurnal cycle in the wind speed. In addition, I would argue that the diurnal cycle of the LE differs from the diurnal cycles of Ts and q, since it has a local minimum at 07:00UTC.*
Response: Thanks for providing a different perspective, as suggested by the reviewer. The wind has a diurnal cycle under clear-sky conditions at Dome A. However, due to the large range of the y-axis in Figure 2a of main text, the significant pattern for wind was unclear. We have made necessary corrections to Figure 2 of main text to improve its clarity.

Regarding LE, we recalculated it following the reviewer's suggestion. The results show that high LE values are observed during the warming phase, and lower values during the cooling phase, similar to T$_s$ and q as depicted in Figure 2 of the main text (at the end of this response). We acknowledge that the original manuscript may have had unclear sentences or descriptions for LE changes. We have revised the manuscript by rewriting the sentences to make it more precise and clear in expressing our viewpoint.

22) *The argument that the use of Pang et al. 2019 is a reliable approach is a circular argument since you are using the estimate of Pang et al. 2019 to compare with the data that Pang et al uses to create the relationship between isotope and temperature.*
Response: Thanks for this comment. To support our estimate, we used simulation data from ECHAM5-wiso (Werner et al., 2011), which calculated precipitation isotopes based on temperature and other factors. We compared the results of our calculation with the simulation data, and the comparison is presented in Figure 2 of the main text. As shown in the figure, the two methods agree with each other quite well.

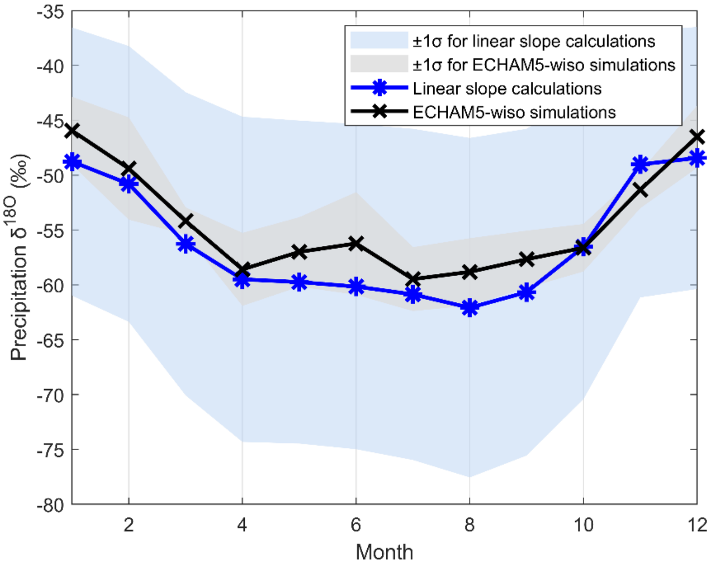

Figure 3. The estimated precipitation δ^{18}O and its standard deviation during the period of 2005-2011. Blue solid line with star marks represents the calculations using the temperature-isotope slope, and the light blue shaded area is the uncertainties. Black solid line with x marks and light grey shaded area displays the ECHAM5-wiso simulation data and its uncertainties, respectively.

23) *L251, L266: It is not correct to say that the meteorological data are less variable in winter. In fact, all meteorological variables are similarly variable as they have about the same standard deviation. Maybe reformulate to "none of the meteorological variables shows a diurnal cycle" or "in the winter data does not show a diurnal signal."*
Response: We appreciate your valuable suggestion. The sentences mentioned in the comment have been revised in the new version of the manuscript.

24) *L260: Please give the value of the used snow density. How does this value compare to the density taken from Laepple et al. (2018) for the DOME-C simulations?*
Response: In Table 1 of main text, we have listed the snow density values at Dome A and Dome C. The snow density value at Dome A (380 kg/m^3) is slightly higher than

that at Dome C (329 kg/m^3).

25) *L265-266: How is winter defined? Are all hourly data from June-August used?*
Response: Yes, the winter period corresponds to June-August in Antarctica. During the winter period in Antarctica, hourly meteorological data from clear-sky days were retrieved and then averaged for running simulations at Dome A.

26) *L272: Please provide the value of the used δ-T slope in the text.*
Response: For non-summer seasons, the isotopes of precipitation were also estimated using the regression line (slope of 0.64±0.02, R^2=0.59) of the non-summer precipitation isotopic composition and near surface air temperature at Dome F, Vostok and Dome C compiled by Pang et al. (2019). In the main text, we added the used δ-T slope following the comment.

27) *L273-274: Where is this comparison presented, and why is this relevant here? Did this comparison influence the initial values of $\delta^{18}O_s$? If not, I suggest to remove this.*
Response: We appreciate this suggestion. We used a comparison of $\delta^{18}O_s$ values between the ECWMF-wiso dataset and linear calculations using the δ-T slope to validate the $\delta^{18}O_s$ estimation. The results of this comparison are presented in Figure 3. We observed a strong correlation between the monthly $\delta^{18}O_s$ variations in these two data sources, and their values were similar in each month, indicating that the linear calculations are reliable. Based on this finding, we can confidently state in the main text that the setting of $\delta^{18}O_s$ values are accurate at Dome A. Thus, it is necessary to mention the comparison between $\delta^{18}O_s$ calculations from the δ-T slope and the ECWMF-wiso dataset in the text.

28) *L277: Please add the reference (Ma et al., 2020) behind "measurements" again*
Response: Thanks for reminding this. We have checked and added the reference.

29) *L292: Please clarify: What does the "disequilibrium was included" mean?*
Response: The term "disequilibrium" in the original manuscript refers to the isotopic composition of water vapor being in thermodynamic imbalance with the snow isotopes at the snow-atmosphere interface. During modeling, we assumed that the isotopic composition of water vapor was in equilibrium with the snow isotopes under the initial conditions. However, published observations from other polar sites indicate that "disequilibrium" conditions are common. To test how "disequilibrium" conditions affect simulations of water vapor isotopic composition and snow isotopes, we designed sensitivity experiments. In the section 2.4 of main text, we used the phrase "disequilibrium was included" to accurately describe the case. However, this description may not be clear to readers. In the revised manuscript, we replaced it with "the isotopic composition of water vapor being in thermodynamic imbalance with the snow isotopes was included" to make it easier to understand.

30) *L300-301: The authors mention snow samples for Dome-C in L200-201. An*

evaluation of the snow isotopic composition development to observations would be very beneficial for the analysis. The simulated changes in snow isotopic composition seem very small compared to variations observed in surface snow samples.
Response: We acknowledge that the simulated changes in the isotopic composition of snow do not match well with the observations at Dome C. This error can be attributed to the absence of certain physical mechanisms in the original model. To address this issue, we utilized an updated model, which is mentioned in Figure 1, to re-run simulations during the Jan 5th -16th, 2015 at Dome C. As depicted in Figure 3 of the main text (see details at end of this response), the averaged magnitude of the simulated snow isotopic variations aligns with the stacked observations within 24 hours.

31) *L314-315: It is not correct to say diurnal cycle here, instead, Fig. 4 shows the simulated change isotopic composition within 24 hours when applying an average summer day observed in January 5-12th.*
Response: Thanks, we corrected the L314-315 following the reviewer's suggestion. The details are as follows:
"The modelled snow δ^{18}O and δD follow a diurnal pattern where higher values occur during the warming phase and lower values during the cooling phase (Fig. 3d). The diurnal range of simulated snow δ^{18}O are ~2‰ on average. This value is close to the observations in the order of magnitude during a typical frost event, but smaller than that of the simulated water vapor δ^{18}O."

32) *L319: What does "diurnal variations" mean? Diurnal maximum minus diurnal minimum? Please define. Maybe the term "diurnal range" is more suitable?*
Response: Thanks for this helpful suggestion. The "diurnal variations" in this sentence means the diurnal maximum minus diurnal minimum. To make it more clear, we used the "diurnal range" to replace the "diurnal variations".

33) *L339: As mentioned above, the changes in isotopic composition in winter are comparable to the ones in summer.*
Response: Thanks for the comment. We have revised this sentence as follows:
"As a result, in comparison with the simulated results in summer, there is no significant diurnal variations in snow isotopes in winter, but the changes in water vapor isotopic composition in winter are comparable to the ones in summer."

34) *L354-355: I cannot confirm this statement based on the figures. The different axis ranges make it difficult to compare.*
Response: Thanks for pointing out this. In the revised manuscript, we replotted the Figure 7 to clearly show the sensitivity of simulated results to changes in initial conditions.

35) *L359: Please discuss how the simulated results compare to other similar modeling studies, e.g., Wahl et al. (2022) (for Greenland) and Ritter et al. (2016)?*
Response: Thank you for your helpful comment. We have revised the manuscript to

include a discussion of the similarities and differences between our calculations and the simulated results of other studies. One significant similarity we found with two similar studies you mentioned is that diurnal variations in snow isotopes and water vapor isotopic composition in the boundary layer can be mainly explained by the atmosphere-snow water vapor exchange through modeling results. Additionally, these studies suggest that the accumulation of isotopic effects from the atmosphere-snow water vapor exchange can lead to isotopic enrichment of the snow layer during the summer, if the snow layer remains consistently exposed at the surface. One main difference we noticed between these studies is the magnitude of diurnal changes in water vapor isotopic composition and snow isotopes. For instance, the diurnal range of snow isotopic composition at Dome C is larger than that at Kohnen station and Dome A, which can be attributed to the stronger variability of humidity gradient and wind speed at Dome C. We have added these comparisons and related discussions to the main text's Discussion section.

The detailed comparison in the main text is shown as follows:

"We also compared modelled water vapor δ^{18}O, δD, and d-excess data at Dome A with those observations from other East Antarctic interior sites, such as Kohnen station, Dome C, and a location about 100 km away from Dome A (Ritter et al., 2016; Casado et al., 2016; Liu et al., 2022). In general, both our simulations and observations show diurnal patterns, with high values during the daytime warming phase and low values during the night-time cooling phase. However, we noticed that the observed diurnal changes in water vapor δ^{18}O and d-excess at sites near Dome A are very large, over 40‰ and 200‰, respectively. This is probably due to calibration drifts caused by the extremely cold and dry conditions during the measurements at the nearest Dome A site which influence the measurements (Liu et al., 2022). The averaged δD observations of 36±6‰ at Kohnen station and the in-situ measurements of 38±2‰ at Dome C are higher than our modeled δD value of 28.78±19.06‰ at Dome A. This difference can be attributed to atmospheric dynamical conditions linked with wind speed in addition to other meteorological conditions. At Dome A, the daily mean wind speed of 2.8 m/s is lower than 3.3 m/s in Dome C and 4.5 m/s in Kohnen station during summer. A lower wind speed corresponds to relatively weak air convection in the horizontal orientation. Due to the coupling between upper and lower atmospheric layers, vertical turbulent mixing may decrease with the weakened air convection in the atmospheric boundary layer (Casado et al., 2018). This change can attenuate molecular exchange between surface snow and water vapor. In parallel, the decrease of vertical turbulence may result in a less efficient turbulent diffusion of water molecules and an elevated contribution of molecular diffusion during atmosphere-snow water vapor exchange. Changes in water vapor diffusion pathways increase kinetic fractionation and reduce effective isotopic fractionation of water isotopes, leading to a muted fluctuation of modelled water vapor δD in combination with less mass exchange."

36) *L356-358: This basically means that the simulated snow isotopic composition does not significantly change after 24 hours of simulation? How much does it change when letting the simulation run longer?*

Response: Thanks for this constructive suggestion. We have conducted simulations for Dome A over the course of one week during summer, using the updated model. We observed that the isotopic composition of snow became more enriched compared to its initial state (Figure 4).

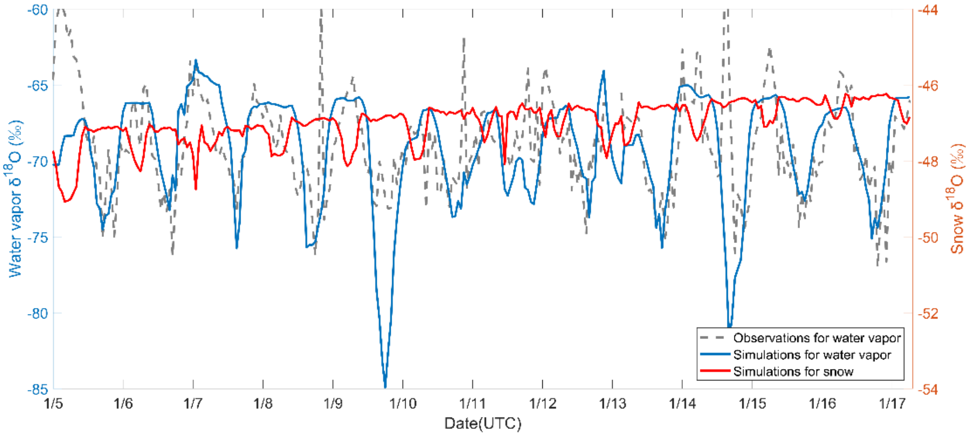

Figure S4: The simulated changes in snow and water vapor isotopes in an 11-day period (Jan 5-16th, 2015) under Dome C conditions

37) *L364-366: Please reformulate this sentence more clearly.*

Response: Thanks for this suggestion. We reformulated this sentence as following: "In general, in the period of mass exchange dominated by sublimation, snow $\delta^{18}O$ and δD are enriched as lighter isotopes are preferentially sublimated to the atmosphere. Meanwhile, sublimates mixing with vapor water lead to increases in vapor $\delta^{18}O$ and δD because the sublimates are of higher $\delta^{18}O$ and δD than atmospheric vapor.".

38) *L369-370: How is this evident? The authors do not provide evidence for what drives the isotopic composition, neither within their 24-hour simulation nor in a more realistic simulation of a longer time period. The latent heat flux is driven by (1) the near-surface humidity gradient (which, of course, is closely related to the near-surface temperature gradient) and (2) the wind speed. However, this study lacks any evidence that the temperature and humidity drive the surface snow isotopic composition. Please remove this statement.*

Response: Thanks for this suggestion. We acknowledge that original manuscript did not accurately reflect the relationship between temperature, humidity, and water vapor isotopic composition. After calculating the latent heat flux, we agree that the water vapor and snow isotopic composition are likely controlled by the near-surface humidity gradient and wind speed. We have revised this statement to reflect the discussion after this sentence, rather than deleting it. The new statement is as follows: "Based on Fig. 2, 4c, and 5c, it is clear that the diurnal isotope cycles in surface snow and vapor water have a strong correlation with temperature and humidity."

39) *L371-372: The authors suggest that wind speed doesn't seem to affect the isotopic composition of the surface snow. However, I'd like to point out that they're using an* *average wind speed over 11 days, which doesn't show the hourly changes. Thus, such simulation does not allow for a statement that wind speed does not drive the snow isotopic composition at Dome-C. For example, let's say, just to make my point, that 90% of the changes in snow type are due to wind speed. If the wind speed increases linearly from 2 to 7 m/s over the first 5.5 days and then decreases from 7 to 2 m/s in the next 5.5 days, the snow isotopes would change mainly driven by the wind speed. However, the daily average of this wind change would always be 4.5 m/s for all 24 hours. So, when they use the daily average wind speed in their simulation, it makes it seem like wind has no effect on the snow isotopic composition, even though in this example, wind was defined to be the main factor driving the isotopic changes.*

Response: We completely agree with the reviewer's viewpoint. The original simulations, which used averaged meteorological conditions over a 24-hour period, failed to accurately reflect the impact of wind on the water vapor and snow isotopic composition at the atmosphere-snow interface. To address this issue, we re-ran the simulations to obtain continuous isotopic variations during the studied period.

Furthermore, we conducted a sensitivity test by varying with a significant diurnal cycle of wind and comparing it with the ones with averaged wind speed. The results, as shown in Figure 5 (i.e., Figure S2 of the supplementary information), suggest that strong variability in wind speed will enlarge the variations in latent heat, leading to a more significant diurnal change in water vapor isotopes and snow isotopes.

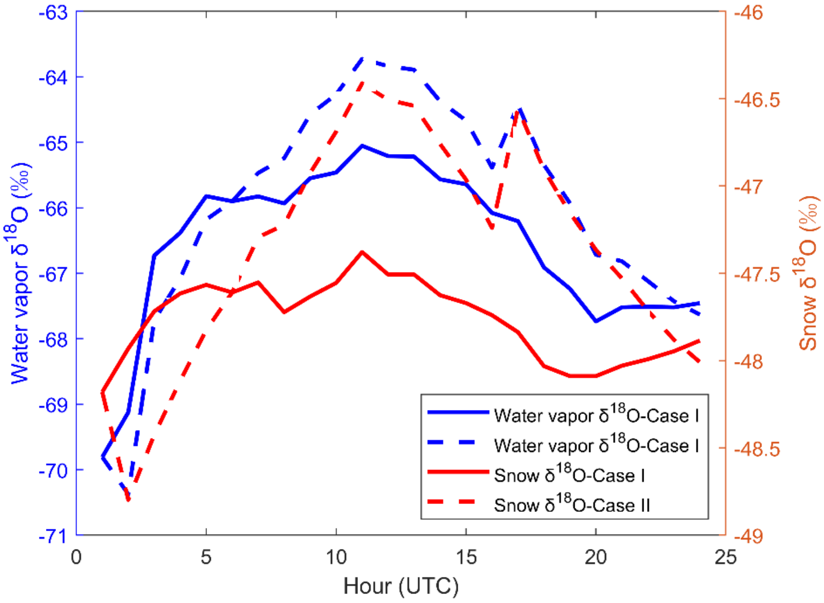

Figure 5: The comparison of water vapor isotopic composition between two simulated cases at Dome A. The simulations in two cases were driven using the averaged wind speed (Case I) and the strong diurnal changes in wind speed (Case II).

40) *L386: What does this mean: "This could adversely affect changes in atmospheric dynamical conditions between day and night"? Please clarify*

Response: The statement in this comment suggests that smaller temperature changes within a cloudy day can create relatively stable atmospheric dynamical conditions. As a result, the diurnal variations of latent heat flux in summer cloudy days are less significant than those in summer clear-sky days. This leads to less mass exchange as well as isotope effects during atmosphere-snow water vapor exchanges. To make the statement clearer, we have reformulated it as follows:

"With the presence of cloud, the differences between the air temperature and surface temperature during the day and night become less pronounced (as shown in Fig. 2). This could have a negative impact on the changes in atmospheric dynamics between day and night, as evidenced by the relatively small magnitude of diurnal variations in Richardson number (as shown in Figs. 4a and 5a)."

41) *L387-389: The authors cannot state that: There is no diurnal cycle when averaging, but of course, the wind speed varies on an hourly and daily basis, and the standard deviation is not zero.*

Response: Thanks for pointing out this inappropriate statement. After careful consideration, we have decided to remove it as this sentence does not contribute to the following discussion.

42) *L427-429: Again, the simulated change in the isotopic composition of the vapor is of a comparable magnitude as the changes in summer. What do the authors base this statement on?*

Response: It is unclear for the statement in the L427-429 of the original manuscript. We have revised it based on the response to Comment #33.

"The results indicate there is small diurnal changes for snow isotopes over the 24-hour simulation period**".**

43) *L444-446: The CALVA program states a sentence on its website on how to acknowledge them for the dataset correctly.*

Response: Thanks for reminding this. We will use the standard way to express the acknowledgement for the CALVA program in the revised manuscript.

"We also acknowledge using Dome C data from the CALVA project and CENECLAM and GLACIOCLIM observatories (http://www-lgge.ujf-grenoble.fr/~christo/calva/)."

44) *References: The two given references for Ma et al. (2020) can currently not be distinguished in the text.*

Response: Thanks for the comment. We would like to clarify that the two papers referenced are published by Ma Bin et al. (2020) and Ma Tianming et al. (2020), respectively. To avoid confusion, we have used the formulation "Ma B. et al. (2020)" and "Ma T. et al. (2020)" when citing these two studies in the text.

45) *Figure 2b: Why is the standard deviation of the latent heat flux so low for cloudy conditions?*

Response: Under cloudy conditions, the relatively low values in the standard deviation of the latent heat flux is mainly attributed to the calculated method (Monte-Carlo method). In the revised manuscript, we directly estimated the standard deviation by stacking the simulated diurnal variations of the latent heat flux at the given days. The corrected results can be seen in the Figure 2b of the revised manuscript.

46) *Figure 3: What is σ for the simulations? Is it the calculated range from the Monte Carlo simulations, or is it the standard deviation of the Monte Carlo simulations?*
Response: The σ in Figure 3 represents the standard deviation of the Monte Carlo simulations. According to the reviewers, the estimates for uncertainty provided in the original manuscript is inappropriate. In the revised manuscript, we have directly estimated the standard deviation by stacking the simulated diurnal variations of snow and water vapor isotopic composition in the individual days. The details can be seen in the Text S2 of the supplemental information (response to Comment #52) and Figure 3 of the main text (at the end of this response).

47) *Figure 3 caption: Add water "vapor" isotopic composition.*
Response: Thanks, Correct.

48) *Figure 4: Again, please be more precise on what "uncertainty" means.*
Response: We have given a detailed explanation in the Comment #46. Please see the response to that comment.

49) *Figure 7: Please provide an explanation of the red lines.*
Response: The red lines in Figure 7 represent the modeled magnitudes of $\delta^{18}O$ diurnal variations in water vapor and snow with the changes in initial conditions. They in fact show the same meanings as the color bar in each panel. Given that, we remove these red lines in the revised manuscript.

50) *Figure 7 caption: Change "6c and 6d" to "7c and 7d".*
Response: Thanks, Correct.

51) *Supplement material S1: The description of the post-processing of the relative humidity (RHw to RHi) is very difficult to understand. – L51-52: Why do you normalize RHw? – L52: Which surface temperature is used? The calculated Ts based on ERA-5? If so, please discuss the introduced error by normalizing the observations using model data. – L54: (Eq. 15): Do you refer to Eq. 13? – L60: What is an "ideal maximum"? – L60, L61: What do you mean by "each temperature point"? – L63-64: The description of the factor is incomplete (the ratio of es with respect to water to es with respect to ice. Moreover, why do you only apply this factor for super-saturated conditions? The relative humidity should be corrected with respect to ice for sub-saturation as well. – L64: What do you mean by "the rising amplitude of the temperature"?*
Response: We appreciate a lot for the reviewer#2's careful checking and valuable comments for Supplement material S1. This part has been rewritten as follows:

"The raw data of relative humidity (RH) at height z is the relative humidity with respect to the water surface (RH_w), measured with the HMP35D humidity probe (Xiao et al., 2008; Ding et al., 2022). The RH_w can be expressed as a percentage:

$$RH_w = e_w/e_w^s \times 100\% \qquad (S2)$$

where e_w is the water vapor pressure of air (Pa), and e_w^s is the saturated vapor pressure with respect to the water surface at the air temperature (Pa) which can be calculated using the Clausius-Clapeyron equation. When calculating the effective fractionation factor (α_f) in the model (Eq: (15) in the main text), the RH_w were converted to the relative humidity over ice at the temperature of the air (RH_i). The conversion between RH_i and RH_w was proposed based on the calibration procedures of Anderson et al. (1984). The details are as follows: 1) The RH_w observations were firstly rescaled using the maximum RH_w of all measured values at each air temperature point (T_a),

$$RH_w' = RH_w (T_a) / RH_w^{max} (T_a) \qquad (S3)$$

2) RH_w' values were then converted to RH_i using Eq: (S4) :

$$RH_i = (e_w^s (T_a) / e_i^s (T_a)) \times RH_w' \qquad (S4)$$

where e_i^s represents the saturated vapor pressure with respect to ice at the air temperature (Pa). Like e_w^s, e_i^s was calculated by the Clausius-Clapeyron equation. Based on Eq: (S3) and Eq: (S4), we obtained RH_i as the final result.

In addition, the relative humidity of the air with respect to the surface temperature (h) in Eq: (14) can also be converted from RH_w observations. The first step of procedures for h conversion is the rescaling RH_w based on Eq: (S3), same to the RH_i conversion. The second step is h calculation using the saturated vapor pressure with respect to ice at the surface temperature (Eq: (S5)).

$$h = (e_w^s (T_a) / e_i^s (T_s)) \times RH_w' \qquad (S5)”$$

52) *Supplement material S2: The description of the uncertainty estimate/error propagation is partly unclear and could be improved. Furthermore, the simulation uncertainties are not sufficiently mentioned and discussed in the main manuscript. A Figure in S2 that shows the calculated uncertainties for all variables could be helpful. – L70: How are the ”uncertainties” calculated? Is it the standard deviation? – L72: Which are ”those days”? – L75: Which error the standard deviation is applied? Please provide more details.*

Response: We would like to express our gratitude to the reviewer for reviewing the supplement material S2. The term "uncertainties" in our study represents the standard deviation of each variable. We have estimated them directly by stacking the observations and calculations on the given days in the revised manuscript. The corrections have thus been made in the supplementary document as we have updated our method of estimating uncertainties. The revised Text S2 is as follows:

“At each time step, we first calculated the standard deviation as the uncertainties (1σ) of wind speed, air temperature, relative humidity by stacking the hourly observations from AWS on the selected days for each parameter. The same method was then applied to determine the uncertainty of surface temperature using hourly calculations from Brun et al., (2012). We also used the stacking method to estimate the uncertainties of other calculations such as the latent heat flux (Q_{LE}). These estimated uncertainties were plotted in Figures 2 of the main text (shaded areas).

The standard deviations of water vapor and surface snow $\delta^{18}O$, δD, and d-excess serve as the uncertainties of simulated isotopic values (Q_{δ}). In the Dome C simulations, these values were calculated by stacking continuous simulations of water isotopes for each day between January 5th and January 16th in 2015 (as indicated by the shaded area in Figure 3). However, clear-sky and cloudy days selected for Dome A simulations are not continuous. Therefore, we were only able to use the model for one day to simulate the diurnal changes in snow and water vapor isotopes, after a week of spin-up time. This make it difficult to estimate the uncertainties of water isotopes using the simple stacking method. To determine the uncertainties, we used error propagation method as an alternative solution, as referred to by Radic et al. (2017). First, we calculated the uncertainties of the fractionation coefficient (Q_{α}) based on the standard deviation of surface temperature. Then, we used the uncertainties of latent heat (Q_{LE}) and Q_{α} to determine Q_{δ}. The equations used to calculate Q_{α} and Q_{δ} are shown as below:

$$Q_{\alpha} = \alpha' * Q_{Ts} \qquad \text{(S6)}$$

$$Q_{\delta} = \sqrt{\left(\frac{\partial \delta}{\partial \alpha} * Q_{\alpha}\right)^2 + \left(\frac{\partial \delta}{\partial LE} * Q_{LE}\right)^2} \qquad \text{(S7)}$$

where α' is the derivative of fractionation coefficient (Eq:(13) of the main text), the $\frac{\partial \delta}{\partial \alpha}$ and $\frac{\partial \delta}{\partial LE}$ represents the derivative of fractionation coefficient and latent heat flux in the equation of isotopic balance of the model (Eq: (10) of the main text). The final results are shown in the Figures 4-6 of the main text."

End of the responses to Reviewer #2

Reference

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Response to Reviewer #3's comments

General comments

1) *This manuscript describes a closed box model assuming no atmospheric mixing and simulations of the effect of a mean diurnal cycle at Dome C (using observations) and Dome A (using atmospheric reanalyses and assumptions as inputs). The current title does not reflect the content and the conclusions are not well supported by the analyses and the underlying assumptions in the modelling methodology.*

Response: Thanks for reviewing the manuscript and the valuable comments. In the revised manuscript, we have addressed the issue of "no atmospheric mixing" by including exchanges between the boundary layer and free troposphere. Additionally, the calculations of isotope mass balance have also been modified following the new model structure. Using this modified model, we have conducted new simulations at Dome C and Dome A. The discussion has been reformulated in the revised manuscript based on the new simulated results and the feedback from the reviewers.

2) *The long introduction gives a good scene setting for the study, which addresses an important topic, but fails to describe the modelling framework in the context of other studies, and fails to provide a clear comparison of the meteorological and snow conditions between Dome C and Dome A (and what are the similarities and differences that need to be accounted for in comparing results for these two sites, for diurnal variations, clear and cloud sky, and winter vs summer conditions).*

Response: We would like to express our gratitude to the reviewer for this comment. In response to the comment, we have made some revisions to the manuscript. Specifically, in the revised manuscript, we have added a comparison of the meteorological and snow conditions between Dome C and Dome A in Section 2.2.2. The comparison of isotopic values for these two sites were also conducted at the result section (Section 3.2). Additionally, we have included a new paragraph in Section 4 to discuss the similarities and differences in diurnal variations between these two sites. We hope that these revisions will enhance the clarity and comprehensiveness of our work.

The added statement in Section 4 is as follows:

"We compared our Dome A simulations with water vapor $\delta^{18}O$, δD, and d-excess data from other East Antarctic interior sites, such as Kohnen station, Dome C, and a location about 100 km away from Dome A (Ritter et al., 2016; Casado et al., 2016; Liu et al., 2022). Both our simulations and observations show diurnal patterns, with high values during the daytime warming phase and low values during the nighttime cooling phase. However, we noticed that the observed diurnal changes in water vapor δ18O and d-excess at sites near Dome A are very large, over 40‰ and 200‰, respectively. This could be due to calibration drift caused by the extremely cold and dry conditions during the measurements at the nearest Dome A site. The averaged δD observations of 36±6‰ at Kohnen station and the in-situ measurements of 38±2‰ at Dome C are higher than our modeled δD value of 28.78±19.06‰ at Dome A. This difference can be attributed to atmospheric dynamical conditions linked with wind speed. At Dome A, the daily mean wind speed of 2.8 m/s is lower than 3.3 m/s in Dome C and 4.5 m/s in Kohnen station during summer. A lower wind speed corresponds to relatively weak air convection in the horizontal orientation. Due to the coupling between upper and lower atmospheric layers, vertical turbulent mixing may decrease with the weakened air convection in the atmospheric near-surface layer (Casado et al., 2018). This change can attenuate molecular exchange between surface snow and water vapor. In parallel, the decrease of vertical turbulence may result in a less efficient turbulent diffusion of water molecules and an elevated contribution of molecular diffusion during atmosphere-snow water vapor exchange. Changes in water vapor diffusion pathways increase kinetic fractionation and reduce effective isotopic fractionation of water isotopes, leading to a muted fluctuation of modeled water vapor δD in combination with less mass exchange.”

3) *The description of the model has flaws in the equations for latent heat flux and possibly in the use of relative humidity in the atmosphere and not relative to surface temperature for fractionation coefficients. The information provided in supplementary information is very difficult to understand.*

Response: Thanks for this comment. We found that the formulations used in the latent heat flux calculation is not correct, following Berkowicz and Prahm (1982) (B&P82) and the suggestion from the reviewer #2. In addition, the fractionation coefficients calculations should rely on the humidity with respect to the surface layer and surface temperature, rather than relative humidity and air temperature.

In the revised manuscript, we first have made modifications to the calculations of latent heat flux. Specifically, we have revised the calculations as follows:

“$$Ex = LE/Ls = -\rho_a u^* q^* \qquad (1)$$

where ρ_a is dry air density varying with observed air temperature (T_a) and pressure (P_a), L_s is sublimation heat constant, u* and q* are friction velocity and specific humidity turbulent scale, respectively. Where u* and q* are respectively defined as:

$$u^* = \frac{k u_z}{log\left(\frac{z}{z_0}\right) - \Psi_M\left(\frac{z}{L}\right)} \qquad (2)$$

$$q^* = \frac{k(q_a - q_s)}{log\left(\frac{z}{z_0}\right) - \Psi_M\left(\frac{z}{L}\right)} \qquad (3)$$”

We would also like to apologize for any confusion caused by our imprecise description of the relative humidity correction and fractionation coefficients. In the new version of the supplementary information, the text description for the relative humidity correction have been rewritten to be more clear and accurate. Additionally, we would like to clarify that it is the surface snow temperature (T_s) that controls isotopic fractionation during air-snow vapor exchange. Thus, the surface temperature were used to calculate fractionation coefficients, instead of air temperature. We will make the necessary corrections to the related description in Section 2.1.1 of the revised manuscript.

The revised supplementary information for humidity correction are as follows: “The raw data of relative humidity (RH) at height z is the relative humidity with respect to the water surface (RH_w), measured with the HMP35D humidity probe (Xiao et al., 2008; Ding et al., 2022). The RH_w can be expressed as a percentage:

$$RH_w = e_w/e_w^s \times 100\% \qquad (S2)$$

where e_w is the water vapor pressure of air (Pa), and e_w^s is the saturated vapor pressure with respect to the water surface at the air temperature (Pa) which can be calculated using the Clausius-Clapeyron equation. When calculating the effective fractionation factor (α_f) in the model (Eq: (15) in the main text), the RH_w were converted to the relative humidity over ice at the temperature of the air (RH_i). The conversion between RH_i and RH_w was proposed based on the calibration procedures of Anderson et al. (1984). The details are as follows: 1) The RH_w observations were firstly rescaled using the maximum RH_w of all measured values at each air temperature point (T_a),

$$RH_w' = RH_w(T_a)/ RH_w^{max}(T_a) \qquad (S3)$$

2) RH_w' values were then converted to RH_i using Eq: (S4) :

$$RH_i = (e_w^s(T_a)/e_i^s(T_a))\times RH_w' \qquad (S4)$$

where e_i^s represents the saturated vapor pressure with respect to ice at the air temperature (Pa). Like e_w^s, e_i^s was calculated by the Clausius-Clapeyron equation. Based on Eq: (S3) and Eq: (S4), we obtained RH_i as the final result.

In addition, the relative humidity of the air with respect to the surface temperature (h) in Eq: (14) can also be converted from RH_w observations. The first step of procedures for h conversion is the rescaling RH_w based on Eq: (S3), same to the RH_i conversion. The second step is h calculation using the saturated vapor pressure with respect to ice at the surface temperature (Eq: (S5)).

$$h = (e_w^s(T_a)/e_i^s(T_s))\times RH_w' \qquad (S5)$$

4) *The choice of performing simulations driven by a mean diurnal cycle instead of using the actual wealth of observations is unclear and the implications should be discussed. I am puzzled by how wind effects are accounted for when averaging conditions.*

Response: Thanks for this comment. We chose to use the mean stacked conditions to conduct simulation since we wanted to highlight the effects of air-snow exchange in a general case. But in order to avoid confusion, in the revised manuscript, the simulations were conducted using continuous meteorological input for each individual day during the studied period at Dome C. This allowed us to calculate the average diurnal changes in water vapor isotopic composition and snow isotopes. However, for the Dome A case, the selected days for clear-sky, cloudy, and winter conditions were not continuous, making it difficult to conduct simulations as was done for Dome C. Instead, we were only able to use the model for one day to simulate the diurnal changes in snow and water vapor isotopes, after a week of spin-up time (as shown in Figures 4-6 in the revised manuscript). This allows to evaluate the effects of air-snow exchange under representative meteorological conditions.

In addition, we also reconsidered the effect of wind speed on simulations during atmosphere-snow water vapor exchange. In the revised manuscript, a new case simulation was presented to test the effect of wind speed variability on atmosphere-snow water vapor exchange. Specifically, we analyzed the response of water vapor and snow isotopic composition to the conditions of a significant diurnal cycle of wind versus that with averaged wind speed. The results, as shown in Figure 1, suggest that strong variability in wind speed will enlarge the variations in latent heat, leading to a more significant diurnal change in water vapor isotopes and snow isotopes, but for a longer time, there would be days with diurnal wind cycle both smaller or bigger than the mean, so the result with the mean wind pattern is more representative. These discussion has been added into the Supplementary Information (Text S3)

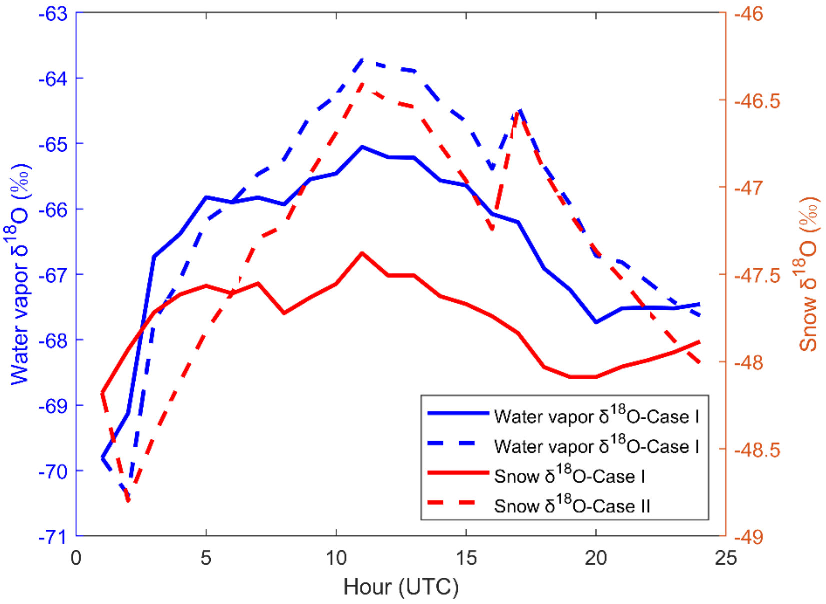

Figure 1: The comparison of water vapor isotopic composition between two simulated cases at Dome A. The simulations in two cases were driven using the averaged wind speed (Case I) and the strong diurnal changes in wind speed (Case II).

5) *There should be at least a more detailed comparison between the Dome C and Dome A characteristics (including comparison of meteorological conditions and ERA5 results at both sites), instead of current Table 1 (where assumptions versus observational based information should be differenciated).*

Response: Thanks for this suggestion. In the revised manuscript, we have added content to compare the meteorological conditions at Dome C and Dome A in Section 2.2.2, and the impacts of these conditions on the modeled water vapor and snow isotopes are discussed in Section 4.

6) *The assumptions displayed in Figure 1 should be discussed in the context of available information, including the Richardson number, regarding atmospheric exchanges (the closed box assumption validity).*

Response: Thanks for the valuable suggestion. We have incorporated these into the revised manuscript by discussing the assumptions related to the occurrence conditions of the air-mass renewal process associated with the Richardson number, as well as the isotopic fractionation during sublimation and deposition. Additionally, we have addressed the setting of initial conditions through some original and new sensitivity tests.

Here, we will provide the discussion of the occurrence conditions of the air-mass renewal process in the supplementary information (Text S3):

"To determine the correlation between mixing occurrence conditions and Richardson numbers, we ran simulations for Dome C, taking into account mixing when Ri<0 and Ri<0.1. As shown in Figure 2, the case with Ri<0 did indeed underestimate the water vapor isotopic composition in the near-surface atmospheric layer during the cooling time. Based on this comparison, we incorporated mixing into the modeling once Ri<0.1."

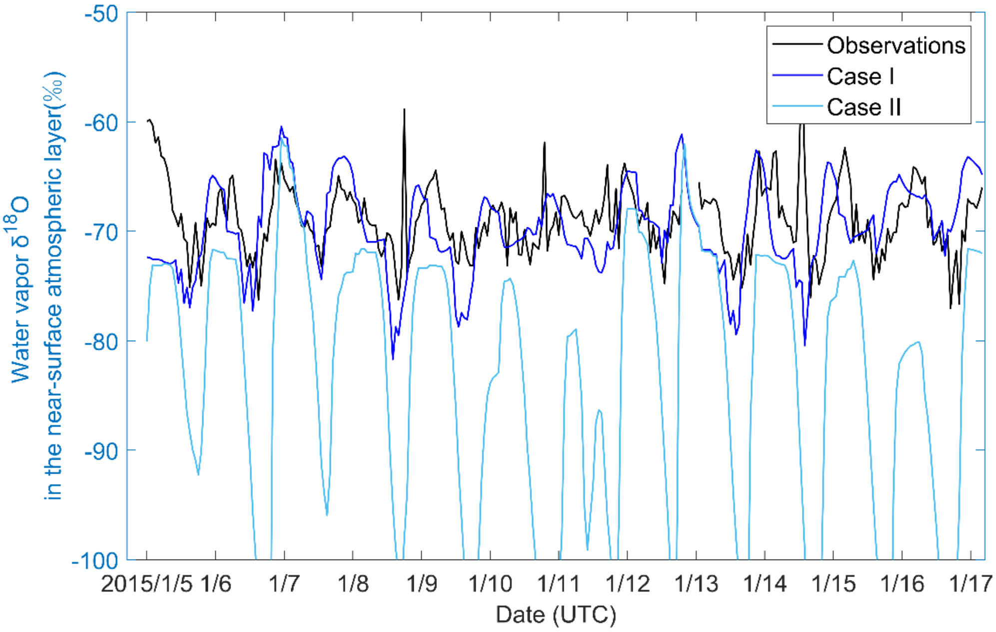

Figure 2: The comparison of water vapor isotopic composition between the simulated and observed changes at Dome C. Two simulated cases are presented here to discuss the occurrence condition of mixing. In the case I, the mixing is assumed to happen when Ri<0 in the cooling phase. The case II for the occurrence conditions of mixing is Ri<0.1 in the cooling phase.

7) *The authors should reflect on what their model explicitely implies in terms of behaviour, and what is effectively "validated" from their approach which does not resolve the diurnal variations in snow measured at Dome C. This physics-based approach is missing.*

Response: Thank you for bringing this to our attention. We have resolved the issue by making modifications to the physical mechanism of our model (Figure 1), as outlined in our previous response to general comments. We then conducted simulations under Dome C conditions and three different cases at Dome A using the updated model. The simulated results for a 24-hour period are presented in Figures 3-6 of the main text (at the end of this response). The new results indicate that the changes in snow isotopic composition are significantly greater than the original $\delta^{18}O$ simulations of 0.02‰ at Dome C. During a typical night, such as the frost event on January 6-7, 2015, the diurnal changes of the newly simulated results between the maximum and minimum can reach

2‰ for snow δ^{18}O (as shown in Figure 3). This magnitude is consistent with the observations for snow isotopes from Casado et al. (2018).

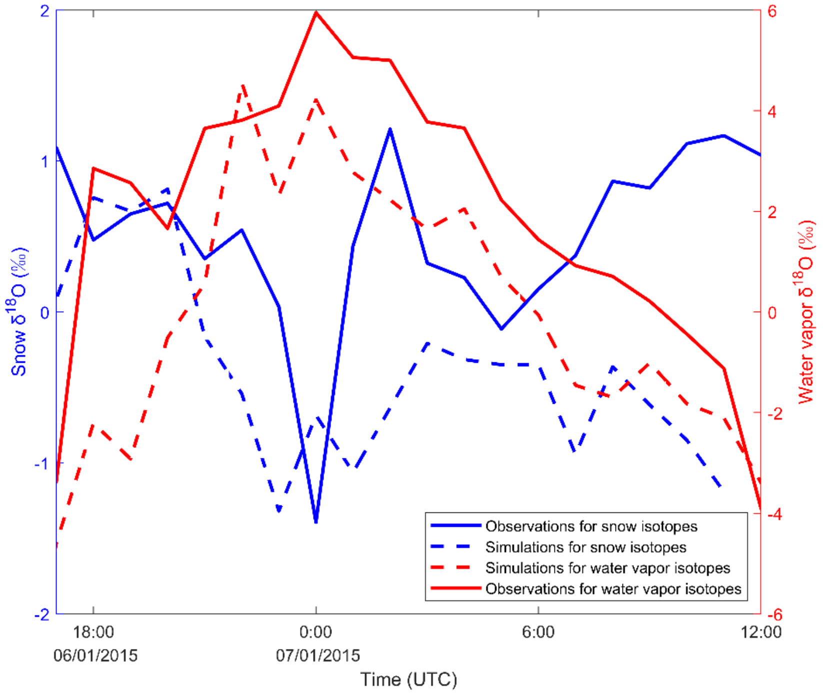

Figure 3: The changes of snow isotopes and water isotopic composition in the near surface atmospheric layer during the 6-7th Jan, 2015 at Dome C.

8) *For these reasons, major revisions are needed, first to ensure accurate equations in the model, and then to reflect on the limitations and suitability of the core assumptions of the closed box model to address these questions, and third regarding the average diurnal cycle approach, and fourth regarding the detailed comparison between Dome C and Dome A (well beyond "validating" and "applying" this model at the two sites).*

Response: Thank you for the helpful comment. Several significant changes were made to the model structure to reflect reviewer's suggestions. Specifically, we have added a third box to represent the free atmosphere layer. The calculations and equations were also updated to reflect the modifications made to the physical mechanism of the model. We also have presented new assumptions for initial conditions and air mass renewal occurrence conditions, which enable the model to run effectively. Furthermore, the simulations were continuously conducted using meteorological observations recorded hourly. Finally, we have included a comparison between Dome C and Dome A in the Discussion section of the revised manuscript (Details can be seen in response to Comment #2 and Comment #6). After all of these modifications, in addition to that arisen by other reviewers, the main conclusion of the manuscript stays the same: The diurnal variations in atmospheric water vapor δ^{18}O and δD can reach 4.75±2.15 ‰ and 28.79±19.06 ‰ under summer clear-sky conditions at Dome A, with corresponding diurnal variations in surface snow δ^{18}O and δD by 0.81±0.24 ‰ and 1.64±2.71 ‰, respectively. After 24-hour simulation, snow water isotopes were enriched under clear-sky conditions. However, there is no or very little enrichment for snow water isotopes under cloudy conditions. Under winter conditions at Dome A, the model still indicates the diurnal change in atmospheric and surface snow water isotopes are not significant, but the model predicts more or less depletions in snow δ^{18}O and δD in the period of 24-hour simulation, opposite to the results under summer clear-sky conditions. This suggests that the air-snow vapor exchange tends to enlarge snow water isotope seasonality.

End of the responses to Reviewer #3

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