Review of manuscript tc-2023-76 "A model framework on atmosphere-snow water vapor exchange and the associated isotope effects at Dome Argus, Antarctica: part I the diurnal changes" by Tianming Ma et al.

General comments:
The impact of vapor exchange on the surface snow isotopes is uncertain. In their study, the authors present a box model to simulate the impact of vapor exchange on the isotopic composition of the vapor and surface snow. They evaluate a simulation driven by average meteorological diurnal conditions from 11 summer days at DOME-C using vapor isotope measurements and conclude that the model reproduces diurnal variations of the isotopes in the vapor. A similar simulation is repeated for DOME-A for cloudy summer, non-cloudy summer, and winter conditions. A sensitivity analysis to the model input parameters is performed.

The topic is relevant and has the potential to improve our understanding of the impact of water vapor exchange on the surface snow isotopes, in particular, because simulations of the vapor exchange impact on the vapor/snow isotopes are rare. However, there are severe mistakes in the model theory, and the conclusions drawn by the authors are not supported by the applied methodology and presented results. I suggest major revisions in their methodology prior to publication.

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.

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.

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.

The figures are generally well-made and described. Relevant previous research is cited and discussed. The structure of the manuscript is clear, but the language could be clearer in some parts. The description of the humidity post-processing and the uncertainty analysis needs to be more detailed and clearer. I am looking forward to the revised version of this study.

Detailed comments:

- 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.

- 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.

- 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".

- 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".

- 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)?

- 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 Θ^* from Eqs. 11a and 11b, $\Delta u = u_{air} - u_{surface}$ with $u_{surface} = 0$, and $\gamma = \frac{c_p}{L_s}$ 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, L_s should not show up on the right side of the formula when giving the expression for $\frac{LE}{L_s}$. 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 ($q_a - q_s$).

- L135: Please change "ρ_V" to "ρ_a"

- 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.

- L172: Above (in L138), RH_i is defined as the relative humidity over ice, not for the specific humidity.

- 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., $h = \frac{q_{air}}{q_{sat,surface}}$ (instead of $RH_{air} = \frac{q_{air}}{q_{sat,air}}$). The formulation in M&J79 is really confusing, but their q_s in the formula of $h = \frac{q}{q_s}$ (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 $h = \frac{q_{air}}{q_{s,surface}}$. 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.

- L189: Where does the expression for R^t_{EX} come from? Because Eq. 2 in Jouzel and Merlivat (1984) is $R^t_{EX} = \alpha_f(R^t_v + 1) - 1$. Please correct this.

- 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.

- L209: I suggest replacing "representative" with "average". It was initially unclear to me what the authors meant by "stacking" the observed cycles.

- 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.

- 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?
- L214: An emissivity of 0.93 seems relatively low to me. Please indicate where this value originates from.
- 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.
- L217: Please remove the "etc." if no further data is used.
- 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.
- 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.
- 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.
- 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."
- 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?
- L265-266: How is winter defined? Are all hourly data from June-August used?
- L272: Please provide the value of the used δ-T slope in the text.
- L273-274: Where is this comparison presented, and why is this relevant here? Did this comparison influence the initial values of δ^{18}Os? If not, I suggest to remove this.
- L277: Please add the reference (Ma et al., 2020) behind "measurements" again.
- L292: Please clarify: What does the "disequilibrium was included" mean?
- 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.
- 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.
- L319: What does "diurnal variations" mean? Diurnal maximum minus diurnal minimum? Please define. Maybe the term "diurnal range" is more suitable?
- L339: As mentioned above, the changes in isotopic composition in winter are comparable to the ones in summer
- L354-355: I cannot confirm this statement based on the figures. The different axis ranges make it difficult to compare.
- 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)?
- 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?
- L364-366: Please reformulate this sentence more clearly.
- 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.

- 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.
- L386: What does this mean: "This could adversely affect changes in atmospheric dynamical conditions between day and night"? Please clarify.
- 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.
- 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?
- L444-446: The CALVA program states a sentence on its website on how to acknowledge them for the dataset correctly.
- References: The two given references for Ma et al. (2020) can currently not be distinguished in the text.
- Figure 2b: Why is the standard deviation of the latent heat flux so low for cloudy conditions?
- 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?
- Figure 3 caption: Add water "vapor" isotopic composition.
- Figure 4: Again, please be more precise on what "uncertainty" means.
- Figure 7: Please provide an explanation of the red lines.
- Figure 7 caption: Change "6c and 6d" to "7c and 7d".
- Supplement material S1: The description of the post-processing of the relative humidity (RH_w to RH_i) is very difficult to understand.
  - L51-52: Why do you normalize RH_w?
  - L52: Which surface temperature is used? The calculated T_s 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 e_s with respect to water to e_s 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"?
- 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 propagation method is applied? Please provide more details.

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