Dear Referees and topic editor!

Thank you for your comments and improvement ideas! Please find below our responses.
Referee' comments are in blue and authors' responses are in black.

Dear Dr. Leino, dear co-authors,

I thank you for considering the comments of the referees and for making adequate modifications to the text. It is my pleasure to inform you that after the second revision both opponents require only minor additional modifications (please, see suggestions below). Once these corrections have been made, your paper will be recommended for publication. Please make these edits as soon as possible and send back the text with the modifications highlighted.
Kindest regards

D. Buresova

#Referee 1

The manuscript has improved compared to the previous version thanks to the authors' work, but there are still a few points that need to be addressed before publication.

-In my initial review, I suggested showing the results in more detail in the 0-1 km range, as it was difficult to distinguish between the different curves in that range, and the paper discusses important results there. I did not mean to modify the upper limit of the figure from 5 km to 1 km, but rather to include an additional figure specifically focusing on the 0-1 km range. Additionally, it would be beneficial for readers to have access to the figures from the previous version of the manuscript that extend up to 5 km, perhaps as supplementary material.

Added figures that extend up to 5 km in the appendix. Modified sentences:

"The upper limit of 1 km was selected to see the effect of initialization near the surface. **Appendix A shows the profiles up to 5 km.**"

"Compared to the default model run (lines), this causes the atmosphere to warm above about 3 km at 12 LTST (**shown in Fig. A1**)..."

"...decrease above 25 m up to about 4.5 km **(Fig. A1)**, and..."

"That can be seen from the water vapor volume mixing ratio (VMR) being constant with altitude **(see also Figs. A2e and A2f).**"

-Regarding the errors in the observations, I pointed out the need to include uncertainties in the data to assess how well the model reproduces it. However, the authors mentioned that uncertainties for these data are currently unavailable. If this is the case, sentences like 'The model's 1.6 m VMR cycle was close to the MSL-observed values, but they were slightly higher in the cool season and slightly lower in the warm season compared to the model prediction' need to be justified. If uncertainties cannot be included when comparing data vs. model, it should be acknowledged that this comparison is limited by the absence of uncertainties.

Modified the text a little:
"The predicted diurnal 1.6 m T cycle is relatively close to the REMS-H-observed values in both seasons (Figs. 4c and 4d)**, but this comparison is limited by the absence of uncertainties.**"

“The model's 1.6 m VMR cycle was close to the MSL-observed values, but they were slightly higher in the cool season and slightly lower in the warm season compared to the model prediction. **However, this comparison is slightly limited by the absence of uncertainties.**”

-Regarding the use of MEDA data, it's important to note that acceptance from the MEDA team is not necessary for using data that has already been published or is available in the PDS. I encourage the authors to consider extending this work using MEDA data for future studies.

We will consider this for future studies.

-The authors mentioned that 'at the moment, it is not possible to add a diurnal cycle of the aerosol opacity to the column model.' In this case, it would be beneficial to include a discussion in the manuscript about how such an aerosol opacity cycle would affect the simulations, based on the aerosol opacity cycles observed in Gale crater.

Added some text in the Summary and discussion:

“A diurnal cycle of aerosol opacity has been observed in the Gale Crater (e.g., Lemmon et al., 2024). Nonetheless, this cycle is not simulated in the column model, but we assume that it should affect in a similar fashion as in the sensitivity experiment with varying tau. Higher opacity during the day would decrease near-surface temperatures as the atmosphere absorbs more solar radition. By contrast, during the nighttime higher amount of aerosols in the atmosphere would increase near-surface temperatures due to increasing thermal radiation. In the future column model simulations, it would be interesting to test this feature in practice.”

#Referee 2
The authors have significantly improved the manuscript.
The reviewer has only minor comments.

Line 29: There may be some typo. It should be "sensible heat flux". Right?
Done.

Line 53-55: "The main features of the Martian water cycle may be succesfully reproduced by the climate models. However, surface observations at various locations as well as several model simulations have suggested that the near-surface moisture cycle in a diurnal timescale is dominated by the adsorption/desorption and/or salt hydration". Why do you use the word "however" here? That is not fully clear.

Removed.

Line 103: " due much lower temperatures". Do you mean "due to"?

Done.

Line 120-121 and line 158-159: "measurements. (Martínez et al., 2017)" and "variables. (Hamilton et al., 2014)". Shall the citation be before the full stop?

Yes, done.

Line 204-215: Please check whether some rearranging of the information in this paragraph is needed. Possibly, you jump forth and back between different LTSTs. Just as a thought.

Decided to split the parapgraph into two paragraphs to make it more clear to the readers.

Line 234: “The model’s humidity profiles or near-surface cycles are not affected by the initialization of surface pressure (Fig. 7)”. Isn’t there a small effect in Figures 7e) and f)?

Actually, yes there is a very small effect. Modified the text:

“The model’s humidity profiles and near-surface cycles **are affected a little** by the initialization of surface pressure (Fig. 7) and temperature (Fig. 6). **The very small effect by the initialization of surface pressure to water vapor VMR is very likely caused by the fact that VMR value depends on the pressure value (VMR = RH · esat (T)/P). Moreover,** the water vapor…”

Made also a small modification to the abstract:
“Based on our analysis, variations in surface pressure initialization are negligible for the model’s temperature **and almost negligible for the model's** humidity predictions.”

Modification to Summary and discussion:
“We also showed that the initialization of **surface pressure and temperature have a very small effect on** the predicted diurnal moisture cycle. **This is very likely due to the temperature and pressure dependence of the model's moisture quantities.**”

“...looks like to be **almost** negligible for both variables...”

Line 236-238: “which is at least partly due to the fact that they are a function of temperature. Therefore, if the temperature value increases at a given altitude, it immediately increases the mass mixing ratio and VMR values at that same altitude.” Isn’t VMR temperature-independent and pressure-independent? For instance, think of the gas in a balloon. The balloon expands as it rises. But, the mixing ratio of different gases in the balloon remains constant. Please check.

See the equation in the first paragraph of Section 2.2 for VMR. Here are also some formulas from Savijärvi et al. (2016):
$e = RH \times e_{sat}$,
where e is the water vapor partial pressure. The water vapor mass mixing ratio q is given as follows:
$q = \frac{\varepsilon e}{p}$,
where ε=18/44 (ratio of the molecular weight of water to that of dry air) and p is the pressure. In addition, VMR can be calculated from q:

VMR=q/ε

Therefore, e depends on temperature as e_{sat} depends on temperature (Savijärvi et al. 2016, eq. 1). Thus, also q and VMR depend on temperature.

Line 257: Please add that this is their Figure 4, not yours.

Added.

Line 270: “out”. Do you mean “our”?

Yes, done.