Dear Referee #1!

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

The authors report experiments with some one-dimensional atmospheric column model, at the site of the Mars rover Curiosity. That includes model experiments in the warm season around solar longitude 271 and cold season around solar longitude 90. They use Curiosity data on the near-surface temperature, near-surface pressure, atmospheric dust, and atmospheric precipitable water content as initialization condition of the model. The authors study how sensitively their model results depend on these initialization conditions. This is done by increasing and decreasing the model initialization condition by certain values, re-running the model, and inter-comparing model data. Also, the authors use Curiosity data for evaluating their model experiments. The latter is done by comparisons between Curiosity data and model data.

Major revision of the manuscript is needed before publication. There are the following major comments.

Line 152-154: "On top of that, the VMR cycles (Figs. g and h) include the ChemCam-derived VMR (marked by x) estimated from the PWC assuming a well-mixed moisture profile (McConnochie et al., 2018)". Is the ChemCam-derived VMR in Figs. 4-7 (g) and (h) identical or similar to the initialization condition of the model moisture profile (Lines 109-113)? If yes, agreement between your model and ChemCam in Figs. 4-7 (g) and (h) may be trivial. And, it may not provide any new information. Please clarify (or remove the comparison with ChemCam VMR in Figs. 4-7 (g) and (h)).

Atmospheric models are always intitialized using actual observations and the model predicts future. This prediction can be compared against observations. Agreement between the model and observations provides infromation about the performance of the model. The same applies here. The column model is initialized at 00 LTST with the sol-averaged value of the previous sol. Then, the model predicts diurnal cycles for the next sol and those can be compared to observations from that sol. In addition, here we initialze the model based on only the information about the total column water content. The model then predicts the diurnal cycle of the water at each altitude. The output of the humidity profile is not well-mixed (as can be seen from Figs. 4-7 (e) and (f)). See also the answer below.

Line 150-152: "Modeled cycles of diurnal temperature (Figs. c and d) […] at 1.6 m include model runs with […] together with REMS-H values (black spheres)." Are the REMS-H temperature values in Figs. 4-7 (c) and (d) similar to the initialization condition of the model surface temperature (Line 102-103 and Line 129)? If yes, agreement between your model and REMS-H in Figs. 4-7 (c) and (d) may be trivial. And, it may not provide any new information. Please clarify (or remove the comparison with REMS-H in Figs. 4-7 (c) and (d)).

See the answer above.

To make the initialization of the column model clearer, we made small additions:

"The hourly REMS observations, described above, are used to initialize the column model **at 00 LTST.** The model's surface temperature and pressure are initialized with the sol-averaged values, calculated from the hourly **REMS-H and REMS-P** observations of the previous sol."

Line 9-10, lines 206-210 and lines 216-217: “Our analysis suggest that a slightly different shape of the model’s initial humidity profile could yield better results”, “… the model daytime humidity should be increased at low altitudes …”, “… good agreement with the experiments made by Savijärvi et al. (2019a), as initially "low-moist layer…"”, “The moisture profile from the MCD (Savijärvi et al., 2019a, Fig. 8) suggests … . Such an initialization of the moisture profile could work here as well … .” Yes, you made some interesting suggestions. Please make these suggestions happen. A revised version of the manuscript must include the following

- low-moist-layer model experiments, following Savijärvi et al. (2019a) or similar
- model initialization with the MCD moisture profile.

The authors have certainly the capacity to do that, as Dr. H Savijärvi is co-author of the manuscript.

Added new model experoments (Figures 8 and 9) and the following text:

“To test these hypotheses, column model simulations with "low-moist layer" initialization at Ls 90°, and "high-moist layer" initialization at Ls 271° were performed. These initialization profiles are shown in Fig. 8 so that the "low/high moist layer" PWC is the same as the PWC for the corresponding well-mixed profile. This "low-moist layer" assumption is based on GCM aphelion season results (e.g. Montmessin et al., 2017, Fig. 11.18), which suggests that the moisture is concentrated nearer the surface at the equatorial latitudes. However, GCM-based MCD suggests the moisture to be more well-mixed at low altitudes during the warm season (Ls 271°), and peaking at about 35 km. Hence, out "high-moist layer" assumption is based on the MCD moisture profile. “

“Figure 9 shows the simulated 1.6 m VMR cycles for Ls 90° (left panel) and Ls 271° (right panel) with REMS-H-derived VMR values (spheres) and ChemCam-derived VMR values (marked by x). Simulated cycles include "well-mixed" assumptions (red) and "low/high moist layer" assumptions. Figure 9 indeed shows that these tuned assumptions perform better compared to the "well-mixed" assumption. At Ls 90°, the "low-moist layer" initialization now matches with the REMS-H derived VMR at about 05 LTST, as well as with the ChemCam-derived VMR. Similar matches at about 06 LTST REMS-H VMR and daytime ChemCam VMR for Ls 271° is visible when using "high-moist layer" initialization.”

Changed the last sentence of the abstract:

“Our additional model experiments with different shape of the model's initial humidity profile yielded better results compared to the well-mixed assumption in the predicted water vapor volume mixing ratios at 1.6 m.”

Added some text to summary and discussion based on new model experiments:

“**Column model simulations with initial moisture concentrated nearer the surface ("low-moist layer") at Ls 90° and initial moisture concentrated higher in the atmosphere ("high-moist layer") at Ls 271° provided good matches with REMS-H VMR observations and ChemCam-derived VMR values.** This **seasonally varying humidity profile at the MSL site** is likely…”

“The **shape of the** model’s moisture profile **should be adjusted to the location and it** can also…“

That is a major comment. More model experiments and some basic rewriting and/or extension of the article are needed.

Done.

Specific Comments

Line 2-3: “Model experiments were compared with observations from the Curiosity Rover Environmental Monitoring Station humidity (REMS-H) device.” You could add the comparisons with ChemCam (unless removing them, following the major comment on line 152-154).

Added: “…(REMS-H) device **and ChemCam.**“

Line 15: “testing new numerical algorithms”. Please mention briefly what numerical algorithms you mean here.

Added: “…new numerical algorithms**, such as adsorption/desorption scheme and adiabatic heating modification** (e.g., Savijärvi et al., 2016; Paton et al., 2019)…”

Line 38-39: “The REMS-H device humidity measurements will be re-evaluated, which will modify the calibration coefficients. Thus, the humidity values will change somewhat, but they still serve in their current form in the sensitivity analysis performed here.” What is this information based on? Personal communication or is there any reference? Some more clarification would be helpful.

Added a sentence: “The REMS-H is designed and built at the FMI, where sensor testing and calibration are also performed (Harri et al., 2014a).”

Line 50-51: “we focus on parameters whose sensitivity has not been studied before”. Please provide all such parameters.

Added a sentence: “These include surface temperature and pressure, dust optical depth (τ) and column precipitable water content (PWC).”

Line 58-59 and line 80: “The predicted quantities are horizontal wind components, potential temperature and mass mixing ratios of water vapor and ice.” (line 58-59) and “driven by the predicted ground heat flux” (line 80). Are lines 58-59 and line 80 consistent? Is the ground heat flux also a predicted quantity? Are comparisons between REMS wind data and your model possible (or not)?

The ground heat flux in the model is calculated using the predicted quantities (old lines 58-59). Therefore, it can be said that the ground heat flux is also “predicted”, but it is not “predicted quantity”. Comparisons between REMS wind data is not possible due to damage of the wind sensor occurred upon landing (https://atmos.nmsu.edu/data_and_services/atmospheres_data/MARS/curiosity/rems_wind.html).

Line 67-68: “and the surface transfer coefficients are defined with the same stability functions as above the lowest model layer.” Please explain how stability functions depend on the height. Otherwise, that may not be immediately clear to an external reader.

Added a senrence: “These stability functions depend on the height as the bulk Richardson number depends on the buoyancy and wind shear (Louis, 1979; Stull, 1988).”

Line 74: “trace gases are not taken into account”. Please specify exactly what trace gases you mean here.

Added: “…trace gases **(O2 , O3 , CO)** are…“

Line 81: “at eight levels” Are these sub-surface levels? They seem to be different from the model grid points in lines 57-58. Please clarify.

Added: “…eight **sub-surface** levels…“

Line 86: “median of the first measurements of RH”. How many of the first measurements of RH? What does that mean for data accuracy?

Added: “…first **four hourly** measurements…“

Line 87: “average”. Are these 5 minute averages? Do you calculate medians, arithmetic means, or something else? Please give some more details.

Added: “…and **hourly 5 minute** average…”

Line 87-88: “Here we use only the last measurements of P as the stable sensor (LL type) needs long warm-up time”. How many of the last P measurements do you use? What does that mean for data accuracy? Do you calculate the median, arithmetic means, or something else? Please give some more details.

Modified the sentence: “Here we use **median of** the last **20** measurements…”

Line 102: “The model’s surface temperature and pressure”. Do you mean here the temperature and pressure exactly at the surface (at zero meters altitude) or at the lowest model level, which is 0.3 meters (as follows from line 58)?

We mean temperature and pressure exactly at the surface. The lowest model level is 0 meters. Added this to old line 58.

Line 101-103: “The hourly REMS observations, described above, are used to initialize the column model. The model’s surface temperature and pressure are initialized with the sol-averaged values, calculated from the hourly REMS observations of the previous sol.” What REMS measurements are used? Is the REMS ground temperature sensor, air temperature sensor, or REMS-H sensor temperature used for initializing the surface temperature and REMS-P for the surface pressure? See also the below comment on Line 129.

REMS-H and REMS-P observations are used. Modified the sentence:

“The model’s surface temperature and pressure are initialized with the sol-averaged values, calculated from the hourly **REMS-H and REMS-P** observations of the previous sol.”

Line 103-104: “lapse rate of 1 K/km”. Provide evidence why a lapse rate of 1K/km is reasonable here. Is that consistent with measured or theoretical lapse rates on Mars?

Modified the sentence: “The temperature profile **at the MSL site** is initialized from the surface value with **a typical** lapse rate of 1 K/km **(Savijärvi et al., 2019a, 2020b)** and the pressure profile is calculated hydrostatically from the temperature profile.”

Line 105-108: Do you use a single value or daily mean value of the Mastcam dust optical depth? Please explain.

Added: "…and **daily mean** is used…"

Line 109-113: Do you use a single value or daily mean values of ChemCam PWC or not. Please explain.

Added: "…ChemCam measurements **(single values for both sols)** are used…"

Line 113: Is the surface pressure p_s based on REMS-P? Please make that clear.

Added: "… ps is the **REMS-P** surface pressure…"

Line 129: "for REMS-H mean temperature". Does that mean the REMS-H sensor temperature is used for initializing surface temperature? If yes, please state that clearly in the manuscript. Explain also why not using ground temperature sensor and air temperature sensor data. See also the above comment on Line 101-103.

Yes, REMS-H sensor temperature is used. Modified the some text in the introduction:

"Here, the REMS-H temperature sensor reading is used as a proxy for the atmospheric temperature in a similar fashion as in Savijärvi et al. (2016, 2019a, b)**, since they are estimated to deviate from the ambient temperatures by at most 1 K (Savijärvi et al., 2015).**"

Added some text:

"In this study, we use REMS-H internal temperatures instead of REMS-T air temperatures or REMS-GTS ground temperatures due to additional uncertainties of REMS-T and REMS-GTS measurements. REMS-T sensor is located only about 0.6 m above the rover deck. Thus, the heating of the rover by solar radiation and by the Radioisotope Thermoelectric Generator (RTG) may affect the air temperature measurements (Martínez et al., 2017). REMS-GTS measures the ground temperature on a small patch of nearby ground which may be different from the larger region of ground influencing the atmosphere. In addition, the field of view of the GTS is within the area of the ground heated by thermal radiation from the RTG (Hamilton et al., 2014; Martínez et al., 2017)."

Line 132-145: These paragraphs may need some rewriting. Their structure should be more logical. Take for instance the paragraph from line 132-138.

- There are the sentences "As there are lots of data gaps in the measurements, some sols may miss essential observations for determining the sol-averaged T and P. The seasonal pressure 135 cycle is well known at the MSL site, as there are more than 3000 sols of pressure data. Thus, the sol average pressure can be estimated relatively accurately, even from some other Martian year." That does not make clear what is the reason for varying the model initialization by ± 10 Pa.

- There is the sentence "The reported accuracies of the REMS-P pressure and REMS-H temperature sensors are ± 3.5 Pa and ± 5 K (Martínez et al., 2017)." Maybe, that can be used as a minimum value for varying the model initialization (also, there could be questions such as the following. Are data uncertainties randomly distributed? Is the data uncertainty much smaller if calculating data

averages over many REMS measurements?). Maybe, you do not want to use just the minimum value. That is why you eventually select ± 10 Pa and ± 10 K.

Please think about how to rewrite the paragraphs from lines 132-145.

Paragraphs from old lines 132-145 are now rewritten. They are now as follows:

"The reported accuracies of the REMS-P pressure and REMS-H temperature sensors are ± 3.5 Pa (Martínez et al., 2017) and ± 0.1 K (Gómez-Elvira et al., 2012). **These REMS-H temperatures are estimated to deviate from the ambient temperatures by at most 1 K (Savijärvi et al., 2015). By contrast, the reported accuray for REMS-T is ± 5 K (Martínez et al., 2017) and the accuracy of the ground temperature sensor (GTS) temperatures is affected by a number of environmental variables. (Hamilton et al., 2014).**

In this study we want to estimate the performance of the model if the initialization is not well known. As there are lots of data gaps in the measurements, some sols may miss essential observations for determining the sol-averaged T and P. The seasonal pressure cycle is well known at the MSL site, as there are more than 3000 sols of pressure data. Thus, the sol average pressure can be estimated relatively accurately, even from some other Martian year.

Dust optical depth measurements by Mastcam have an accuracy of ± 0.03 (Martínez et al., 2017), but there are only 1160 measured values during sols 33–2575. There are even fewer PWC observations, with only 184 ChemCam PWC retrievals available during sols 230–3111. The extremely small number of measurements causes a rather large inaccuracy in the initialization of the model if there are no measurements in the vicinity of the simulated sol. The indicated precision for the ChemCam-retrieved PWC is ± 0.6 pr-µm (McConnochie et al., 2018)**, with values typically on the order of 10 pr-µm (cf. Fig. 3) at the MSL site.**

Since we want to see the performance of the model if the initialization is unknown, we choose the sol-averaged surface pressure to vary ± 10 Pa around the default value, whereas the sol-averaged surface temperature is allowed to vary ± 10 K around the default value. In addition, variations of ± 0.3 in τ and ± 3 pr-µm in PWC are used in this study. These values are based on the sensor uncertainties but are slightly higher as we do not want to only use the minimum values."

Line 149: "local time (LT)". Please use local true solar time, if not done yet. Change LT to LTST (consistent with Figs. 4-7 (c)-(d) and (g)-(h); their x-axis caption is "LTST (h)").

Done. Changed all "LT" to "LTST" in the text.

Line 148-149: "at 06 (black), 08 (blue), 10 (red) and 12 (orange) local time (LT)". Please explain why selecting these times.

Added: "These times were selected because the convection is strongest during the morning hours as the sun starts to heat the surface of Mars."

Line 149: "up to 5 km". Please explain why selecting 5 km as the upper limit.

Changed the upper limit to 1 km based on the second referee comment. Added: "The upper limit of 1 km was selected to see the effect of initialization near the surface."

Line 172-182: High dust seems to give higher near-surface temperatures at night and cooler near-surface temperatures during the day. Is that correct? If yes, that may be consistent with the effects on the near-surface-temperature, known from dust storms. Any consistency with dust storms may be pointed out in the paragraph from lines 172-182 (if any). And, the paragraph may be rewritten, accordingly.

Added a new paragraph after that:

"This is consistent with known effects of the dust storms on near-surface temperature cycles. Savijärvi et al. (2020b) clearly showed an increase in near-surface temperatures at night and a decrease during the day from MSL measurements during the MY 34 global dust storm. During the same time period, Viúdez-Moreiras et al. (2020) showed the same effect of increased amount of airborne dust at the InSight location."

Line 201-202: "The nighttime VMR derived from the REMS-H, in Figs. 4g and 4h, is relatively close to the model simulation in both seasons". There seems to be some disagreement in the first half of the night, around 18-24 LTST, in Figs. 4g and 4h. More explanation is needed.

Added some text: "However, some disagreement with modeled and REMS-H derived VMRs around 18–24 LTST are visible. This is very likely related to the low RH values, as they have not yet increased enough after the extremely low daytime values. For example after the dusk at Ls 271°, observed RH is only slightly above 5 %. In contrast, observed RH during early morning hours is about 8-11 %."

Technical Corrections

Line10: Change "our analysis suggest" to "our analysis suggests"?

Changed.

Line 52: Change "summarized and discussed" to "discussed and summarized"?

Changed.

Line 55: Does the model have a name? That is just to make sure. If not, it is alright.

No, it's just 1-D column model.

Line 76: "The long-wave radiation scheme is described using a fast broadband emissivity approach." Does it mean that "The long-wave radiation scheme uses a fast broadband emissivity approach"?

Yes, changed.

Line 81: Change "Savijärvi et al. (2016, 2019a, b, 2020); Savijärvi and Harri (2021)" to "Savijärvi et al. (2016, 2019a, b, 2020) and Savijärvi and Harri (2021)"?

Done.

Line 91: "The REMS-H VMR values are most accurate at minimum VMR, which usually occurs during the night at maximum RH." REMS-H measures RH, not vmr. Right? Does it mean the following? REMS-H is most accurate at maximum RH. The maximum RH occurs at night and thus may coincide with minimum vmr. That may be misunderstandable. Please rephrase. Also, you could provide some more explanation for external readers, on why maximum RH and minimum vmr occur at night.

Changed the sentence: ”The REMS-H is the most accurate at maximum RH, which typically occurs at night due much lower temperatures compared to daytime.”

Line 92-93: “Thus, Figure 2 shows the REMS-H maximum RH (black) and derived VMR (purple) at the same time of sol during Martian year (MY) 32”. You take the daily maximum of RH. Then, you convert the daily maximum of RH into VMR. Right? If so, it is self-explaining that the daily maximum of RH and its derived vmr are at the same time of sol. But, they do not occur at the same time on any sol. Right? That may be misunderstandable. Some rewriting may be needed.

Removed: “at the same time of sol”

Line 94-95: “the warm perihelion period is at around Ls 220°–280°”. The red curve in Fig. 2 seems to have some dip from LS220-280.

Added som text: “In Fig. 2, the daytime maximum near surface temperatures (red curve) appear to show a small decrease during this period, due to the increased amount of airborne dust (Martínez et al., 2017). Lower daytime temperatures due to increased amount of airborne dust are shown in Sect. 3.”

Line 98-99: “reach a minimum around the southern hemisphere winter solstice.” Please add the related solar longitude (Ls 90°).

Added.

Line 105: “(Lemmon, 2014)”. Another very recent publication may be relevant here

M.T. Lemmon, S.D. Guzewich, J.M. Battalio, M.C. Malin, A. Vicente-Retortillo, M.-P. Zorzano, J. Martín-Torres, R. Sullivan, J.N. Maki, M.D. Smith, J.F. Bell, The Mars Science Laboratory record of optical depth measurements via solar imaging, Icarus, Volume 408, 2024.

https://doi.org/10.1016/j.icarus.2023.115821.

That is just to let you know.

Changed.

Line 127: “The diurnal surface pressure cycle is not predicted in the model”. External readers could have the following questions. What does that mean exactly? Why does the model need surface pressure initialization then? Please make that clear.

Modified some text: “The diurnal surface pressure cycle is not predicted in the model. However, the initialization of the surface pressure is necessary to calculate the pressure profile, which is further used in the model calculations. Hence, we choose surface pressure as the last parameter to estimate the importance of initialization accuracy.”

Line 144: Change “cf. 3” to “cf. Fig 3”.

Changed.

Line 151: Change “VMR” to “water vapor VMR”?

Changed.

Caption of Fig. 4: Change “default (-)” to “default (continuous line)” or similar?

Changed.

Caption of Fig. 4: Change “VMR” to “water vapor VMR”?

Changed.

Caption of Fig. 4: Change “local time” to “local true solar time”?

Changed.

Figure 4-7: Change “MSL” to “REMS-H” in the legend of sub-figures and the caption of Fig. 4?

Changed.

Line 159: External readers may need some help for seeing the temperature inversion in Figs. 4(a) and (b) (temperature increases with altitude, close to the surface, …). Please add some more details.

Added text: “… inversion**, since temperature increases with altitude close to the surface** between…”

Line 159-160: “while at 12 LT it is no longer present”. The inversion is already not present at 10 LTST. Right? If right, please rephrase.

Done.

Line 161: “At 08 LT (blue line) convection has already started as solar radiation has started to strongly heat the surface of Mars.” External readers may need some help. Make clear that can be seen from the lower end of the blue curve in Figs. 4-7 (a) and (b) (temperature has changed from increasing with altitude to decreasing with altitude). A close look is needed.

Added a sentence: “This can be seen from the lower end of the blue curve in Figs. 4-7 (a) and (b), since temperature has changed from increasing with altitude to decreasing with altitude.”

Line 162: Change “On top of the stronger convection in the warm season” to “In addition to the stronger convection in the warm season”?

Done.

Line 183-184: “The humidity profiles of both seasons (e.g. Figs. 4e and 4f) display a well-mixed layer in the boundary layer (BL). At 06–08 LT, the well-mixed layer is very shallow and grows thereafter due to strong convection in both seasons.” At 10 LTST (red curve), there seems to be a shallow well-mixed layer from ca. 100-500 meters in Fig. 4e) and 100-800 meters in Fig. 4f). That can be seen from the water vapor mass mixing ratio not changing with altitude. A similar feature is not obvious for 6 and 8 LTST (black and blue curve). Please clarify.

Added text: “At 10 LTST (red curve), there seems to be a shallow well-mixed layer from ca. 100-500 meters in Fig. 5e) and 50-750 meters in Fig. 5f). That can be seen from the water vapor volume mixing ratio (VMR) being constant with altitude. A similar feature is not obvious for 06 and 08 LTST (black and blue curve)”

Line 188-189: “Increased solar radiation near the surface in the morning”. Increased solar radiation near the surface means model initialization with less dust. Right? Please say that clearly.

Changed the sentence: “...in the morning**, due to model initialization with less dust,** drives...”

Line 183-192: Please do not move back and forth between Figs. 4 and 5 in this paragraph.

Splitted the paragraph into two parts.

Line 195: “which is at least partly due to the fact that they are a function of temperature.” Some more explanation is needed. Does it mean that adsorption is a function of temperature? How does it change with increasing surface temperature?

This sentence mean that water vapor mass mixing ratio and VMR are a function of temeprature. Added a sentence: ”Therefore, if the temperature value increases at a given altitude, it immediately increases the mass mixing ratio and VMR values at that same altitude.”

Figure 4-7: Why do you use mass mixing ratio in Figs. 4-7 e) and f) and volume mixing ratio Figs. 4-7 g) and h)?

Changed mass mixing ratio profiles in Figs. 4-7 e) and f) to VMR profiles. Added a sentence into the first paragraph of section 2.1:”In this study, model's water vapor mass mixing ratios are converted to volume mixing ratios (VMR).”

Figure 4-7: Why do the model data have some gap from 0-1 LTST in Figs. 4-7 (c)-(d) and (g)-(h)?

The model output is stored at 1,2,3… LTST

Line 205: Change “marked by x” to “marked by x in Figs. 4-7 (g) and (h)”?

Changed.

Line 208: Change “sphere” to “(sphere, Fig. 5g)”?

Changed.

Line 209: “as initially "low-moist layer" in the model increased 1.6 m VMR values”. Make clear that humidity values were increased at low altitude relative to the well-mixed model experiment in Savijärvi et al. (2019a). That may not be immediately clear to an external reader.

Added: “… "low-moist layer" (**where humidity values were increased at low altitude relative to the well-mixed model experiment)** in the model…”

Dear Referee #2!

Thank you for your comments and improvement ideas! Please find below our responses.

Referee' comments are in blue and authors' responses are in black.

This paper reports a number of sensitivity analyses conducted using a one-dimensional atmospheric column model and comparisons with data acquired by REMS: pressure, near-surface temperature, and VMR at 1.6 m. The authors investigated the impact of dust optical depth, precipitable water content (PWC), surface temperature, and surface pressure values on the model results by varying these parameters in different ranges chosen based on observations. The comparison is conducted for two Ls values: 90° and 271°. Although the manuscript is generally well-written and provides new and valuable information for column model simulations on Mars, further analyses and comparison (listed below) are needed before publication.

Major comments:

-Vertical profiles of Figures 4-7: In the discussion, the authors refer to altitudes that are not shown (e.g., lines 175-182) and that are key to understanding the performance of the model. Additionally, as the comparison is with near-surface data, I would like to see the model results near the surface. If the authors want to keep the sensitivity analysis up to an altitude of 5km, then additional figures focused on the 0-1000 m range should be added as it is hard to distinguish the different curves in that range.

We decied to change the altitude from 5 km to 1 km as suggested.

-The manuscripts states that one of the most sensitive initial parameters for the column model temperature profile are the dust opacity and surface temperature. Here, I would like to see a comparison with the MCD. Also, what would be the effect if part of the aerosol opacity is due to water ice? For Ls=90° simulations, a notable % of the total opacity should be ice whose single scattering albedo is close to 1. Would it be possible to add in the model a diurnal cycle of the aerosol opacity?

The 1-D column model can only take into account local conditions. Condensation to fog and boundary layer clouds are allowed but they did not occur in any of the present integrations, due to the fairly dry equatorial Gale environment. At the moment, it is not possible to add a diurnal cycle of the aerosol opacity to the column model.

-Conclusions section: 'An earlier study by Savijärvi et al. (2019a), large-scale model moisture profile from the MCD (Fig. 8 in Savijärvi et al. (2019a)) and our sensitivity experiments (Figs. 5g and 5h) suggest that the model's initial humidity profile at the MSL site should vary with the season to provide a better moisture prediction near the surface.'. I think the authors should address this in this study. Why not taking the MCD profiles and see if the simulations improve with those model profiles? I don't think that "…the model's initial humidity profile at the MSL site should vary with the season to provide a better moisture prediction near the surface.." is demonstrated in this work, and it is not clear what this study contributes beyond the cited work. This point is also mentioned at

the end of the abstract but again no demonstration of how the humidity profile can affect is given. Please use MCD profiles and see of results change.

Added new model experoments (Figures 8 and 9) and the following text:

“To test these hypotheses, column model simulations with "low-moist layer" initialization at Ls 90°, and "high-moist layer" initialization at Ls 271° were performed. These initialization profiles are shown in Fig. 8 so that the "low/high moist layer" PWC is the same as the PWC for the corresponding well-mixed profile. This "low-moist layer" assumption is based on GCM aphelion season results (e.g. Montmessin et al., 2017, Fig. 11.18), which suggests that the moisture is concentrated nearer the surface at the equatorial latitudes. However, GCM-based MCD suggests the moisture to be more well-mixed at low altitudes during the warm season (Ls 271°), and peaking at about 35 km. Hence, out "high-moist layer" assumption is based on the MCD moisture profile. “

“Figure 9 shows the simulated 1.6 m VMR cycles for Ls 90° (left panel) and Ls 271° (right panel) with REMS-H-derived VMR values (spheres) and ChemCam-derived VMR values (marked by x). Simulated cycles include "well-mixed" assumptions (red) and "low/high moist layer" assumptions. Figure 9 indeed shows that these tuned assumptions perform better compared to the "well-mixed" assumption. At Ls 90°, the "low-moist layer" initialization now matches with the REMS-H derived VMR at about 05 LTST, as well as with the ChemCam-derived VMR. Similar matches at about 06 LTST REMS-H VMR and daytime ChemCam VMR for Ls 271° is visible when using "high-moist layer" initialization.”

Changed the last sentence of the abstract:

“Our additional model experiments with different shape of the model's initial humidity profile yielded better results compared to the well-mixed assumption in the predicted water vapor volume mixing ratios at 1.6 m.”

-Conclusions section: The authors basically summarize the findings in the sensitivity analysis (some previously reported in previous works) but they discuss vaguely the reasons behind and do not detail the impact of their results. For instance, page 14: “We found that the initial value of surface temperature affects the entire temperature profile with a slightly larger effect at Ls 90.”. Why is that? Also, is this the case at all altitudes? It is complicated to say below 1000 m from the figures.

Changed the upper limit to 1 km and added some text: “This seems to be the case at all altitudes and it is propably related to the smaller variations in the diurnal temperature cycles during the cold season compared to the warm season.”

-I believe the manuscript would benefit from the addition of more data from other Ls in the comparison.

We chose 2 opposite seasons to study the sensitivity of the model as comprehensively as possible. Ls 90 is dry and cold while Ls 271 is wet and warm. Therefore, we think that these two seasons are enough to study the model’s sensitivity.

General comments:

-Why the authors are not included in the comparison data from MEDA??

MEDA is not used since this would require additional acceptance from the MEDA team and the main purpose of this study is to study the sensitivity of the model. With the help of this study, we can then use the model at the Perseverance landing site in the future studies.

-Include the errors in the observations, as otherwise, it is hard to figure out how well the model reproduces the data.

Uncertainties for these data are not currently available (see https://atmos.nmsu.edu/data_and_services/atmospheres_data/MARS/curiosity/rems_humidity.html). We decided to mark VMR values with very low RH (< 5 %) as gray spheres in Figs. 4–7 (g) and (h).

-It is confusing to use in the paper terms like ‘profile initialized…’ for parameters that do not change during the run. For the model parameters that do not change during run, please just use ‘fixed profile…’ or ‘fix values of …’

Done.

-Section 2.2: please add information about the sampling when describing the REMS data.

Changed sentence: “The REMS instrument, onboard MSL, measures pressure (P), relative humidity (RH) and temperature (T) **at the rate of one sample per second** for 5 first minutes of each hour, at an altitude of about 1.6 m.”