

Response to the comments of the Reviewer 1:

General comments

1. The paper is well written and easy to read. The topic is of interest for the Martian community (and also for people studying high altitude clouds on other planets, including the Earth). The introduction adequately summarizes previous works on the topic and sets up the questions raised by them. The figures are appropriate, although some minor improvements could be made to facilitate the comparison with observations (see below). The length of the manuscript is also appropriate. However, I find the discussion about the comparison with the observations to be flawed due to the incorrect assumptions the authors make about the observations in *Sefton-Nash et al.* [2013] (see details below). This is an essential aspect of the manuscript that absolutely needs to be corrected before publication.

We thank the reviewer for insightful and useful comments. In the manuscript and the response letter concerns of the Reviewer have been addressed.

Specific comments

1. The comparison between the observed cloud climatology and the model predictions should be based only on clouds spectroscopically unambiguously determined to be composed of CO₂. The differences between the observed and predicted CO₂ cloud climatology should be acknowledged, and the implications for the gravity wave activity in the model discussed.
This indeed was the point, which we did not fully realize at the time of writing. We thank both Reviewers for bringing to us this important point. The text and discussion have been modified accordingly.
2. Another interesting aspect that deserves further discussion is the altitude variation of the cloud formation probability, predicted to be significantly larger at 120 km than at 80 km. Although the altitude of CO₂ mesospheric clouds is not easy to determine for most of the datasets, the current observational knowledge is that, at least during daytime, they are placed at altitudes of about 70-80 km [Scholten et al., 2010; Määttä et al., 2010]. During nighttime SPICAM has detected mesospheric clouds with altitudes around 100 km. No clouds have been detected, to my knowledge, around 120 km or higher, where the model predicts the higher cloud formation probability. I would like to see a discussion about this discrepancy in the manuscript.
We already discussed the importance of explicitly considering microphysics of cloud formation. We hypothesize two possibilities: either clouds are not formed in the upper atmosphere due to the lack of nuclei or for other microphysical reasons, or they are formed but not detected being too thin and/or having too small particle sizes. This, of course, if the simulated GW-induced cold pockets are trusted, as we believe they are now. The text have been extended to reflect this point.
3. The GCM used in the study is very shortly described, apart from the gravity wave parameterization. While this is mostly OK given that the model has already been described in previous papers, I think the implementation of the physical processes affecting the temperatures in the mesosphere/lower

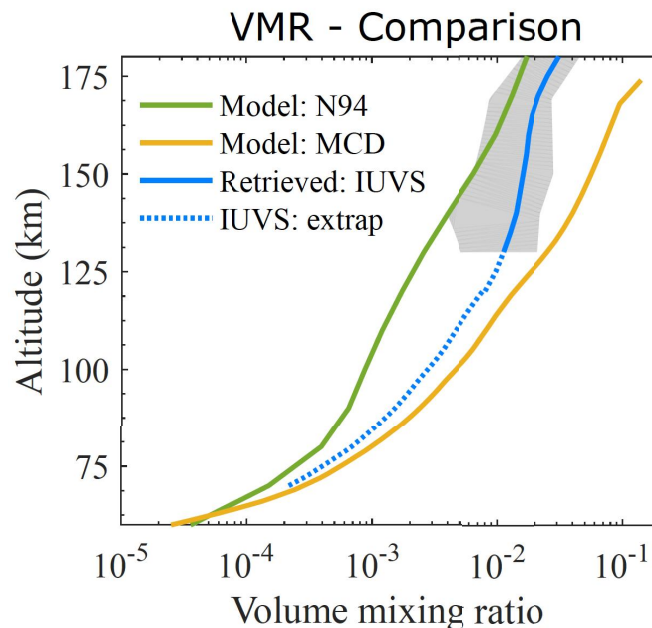


Figure 1: Comparison of 1-D volume mixing profiles, grey area indicates 1 standard deviation. The IUVS/MAVEN retrieval is given by the blue line, green and yellow ones are for the *Nair et al.* [1994] (N94) and Mars Climate Database (MCD) profiles based on photochemical modeling. From [Mockel et al., 2017].

thermosphere needs to be described to some extent. For example, what atomic oxygen distribution are you using among the different possibilities discussed in *Medvedev et al.* [2015]?

We have somewhat extended the model description.

In particular, we used the so-called *Nair et al.* [1994] scenario for the vertical distribution of atomic oxygen. The rationale for that is the following. Recently, retrievals of [O] from airglow emission measurements by Imaging Ultraviolet Spectrograph onboard the MAVEN orbiter (IUVS/MAVEN) became available. They have insufficient spatial and temporal resolution, however they allowed for constructing an oxygen scenario by averaging into a single profile [Mockel et al., 2017]. The comparison between thus obtained “IUVS” scenario and the two discussed in the paper of *Medvedev et al.* [2015] is shown in Figure 1. It is seen that the IUVS oxygen profile lies approximately between the other two available scenarios. However, the IUVS/MAVEN observations were performed during daytime only. It is well known that the production of [O] maximizes on the Sun-lit side as well as its concentration. Therefore, it is plausible to assume that the day time-based IUVS scenario overestimated concentrations, and the daily averaged value would be closer to the *Nair et al.* [1994] scenario.

4. Page 4, lines 8-9. “In the middle and upper atmosphere of Mars, wave damping occurs due primarily to nonlinear wave-wave interactions (breaking and/or saturation) and molecular diffusion and thermal conduction, which are accounted for through the transmissivity”. Eckermann et al., *Icarus* 211, pp. 429-442 (2011) showed that radiative damping can be a dominant process in the middle atmosphere of Mars. Do you consider it in your model?

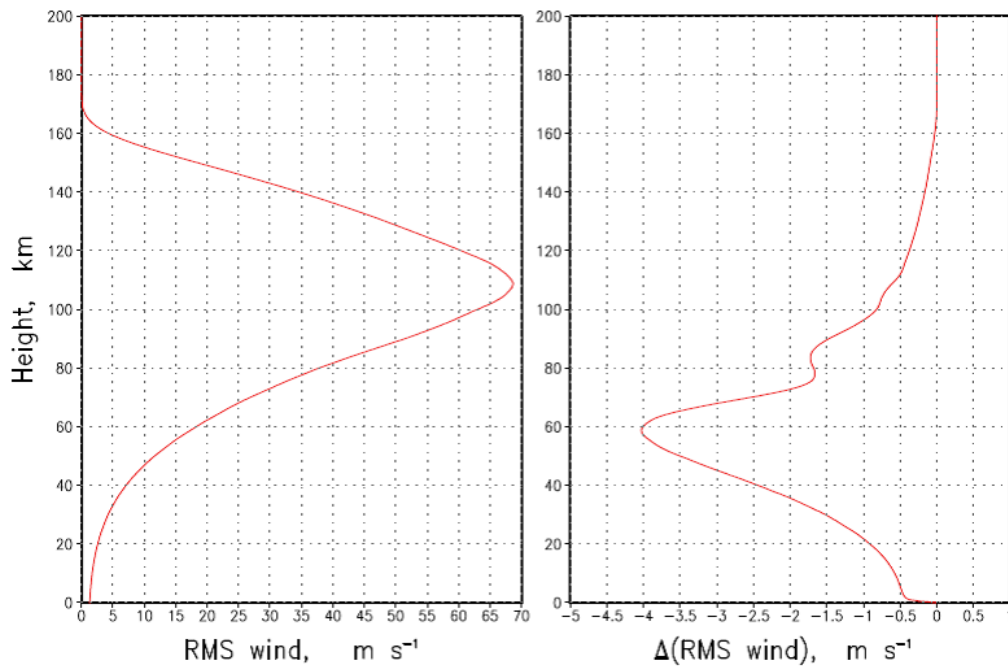


Figure 2: Vertical profiles calculated with the column GW model for a spectrum of GWs with horizontal phase velocities between 0 and 60 m s⁻¹: (a) Rms fluctuations of the GW-induced horizontal wind without accounting for CO₂ radiative cooling; (b) Difference (negative) in the amplitude (rms) produced by the included radiative damping.

The influence of the radiative damping by CO₂ has been studied and results described in our earlier paper [Medvedev *et al.*, 2011, Section 7]. There, the radiative damping rates τ_d^{-1} calculated by Eckermann have been implemented into our parameterization of GWs, which is used in the current study. The results did not confirm quantitatively the hypothesis of Eckermann. They revealed that CO₂ does produce some GW damping in the lower and middle atmosphere, but its effect on wave amplitudes and the produced drag is small. Figure 11(j,k,l) of the paper by Medvedev *et al.* [2011] demonstrates that the radiative damping has only minor net impact on the fields simulated with the MAOAM MGCM. Since wave amplitudes are important within the context of the current study, we attach Figure 2 showing a typical profile of GW activity (rms wind fluctuations for a spectrum of waves) calculated with a column model. These results were closely discussed with Dr. Steve Eckermann. Note that Steve does not pursue his hypothesis since 2011.

The answer to your question is “No, we do not include radiative damping by CO₂ in our simulations.”

5. Figure 1. The gravity wave cooling is generally below 60 K/day except for the strong peak at 140 km reaching 120 K/day. Could you provide an the explanation for this strong peak? It apparently affects only one or two model layers, could you confirm this? Can it be due to any boundary effect? Note also that Fig. 1b) horizontal axis is labeled as K/day, but in the caption you state it is in units of K/sol, which is slightly different. Please correct.

This zigzag appeared due to differentiating at he top two model layers and was caused by the

plotting software - the “non-existing” data were treated as having particular numbers. The plot is now corrected along with the label “K/sol”. All values throughout the paper are assumed to be in sols. We have fully revised Figure 1 and fixed plotting bugs.

6. Page 8, lines 4-5: “The model generally reproduces the observed temperature well, except that it overestimates it in the southern hemisphere winter by up to 20 K”. Other data-model discrepancies are evident by comparison of Fig. 4a with Figure 10 in Sefton-Nash et al., (2013). In particular, the temperature at 80 km in the polar regions can be higher than 180 K in MCS observations, while apparently (but this is maybe an artifact of the chosen color scale) do not go much higher than 150 K in the model. Could you please clarify it?

This is indeed an artifact of plotting in Grads software that we have originally used. We have redone Figure 4 and added higher temperature shading levels in the figure. Now, it can be seen that the temperatures in the polar regions can occasionally exceed 175 K, showing a reasonable agreement with MCS.

7. -Page 8, lines 10-11: “It is seen that the coldest temperatures of down to 90-100 K are found around the summer high-latitudes at solstices and during equinoxes”. I do not see those low temperatures during equinoxes, when apparently temperatures do not go below 120 K, as can be seen also in Fig. 2a. Please clarify/correct.

Yes, this indeed is obvious from Figure 4c. The words “and during equinoxes” have been removed.

8. -Page 9, lines 14-16. “Although the vast majority of studies report on cloud observations in the Martian mesosphere below 80 km, there are some studies that extend their analysis to higher altitudes presenting detections of CO₂ clouds at around the mesopause (100 km) and above (e.g. Sefton-Nash et al., 2013)”. Sefton-Nash et al. (2013) only detected clouds up to 90 km (e.g. their figure 9).

We have now removed this statement.

Technical Comments:

1. Page 2, line 1: “Because the Martian mesosphere is, in average, warmer ...” Warmer than the terrestrial one, or warmer than the CO₂ frost point? Please specify.

Here we refer to the mean temperature in comparison with the condensation threshold. However, there are significant variations around the mean. So, one way of explaining the occurrence of thermodynamically favorable conditions for cloud formation is that there are variations induced by tides and gravity waves around the mean temperature. This is now clear in the text.

2. Page 3, line 5 “variations variations”. Please remove one.

Done

3. Page 3, line 30: It was developed in detail in the work of Yigit et al. (2008), the general principles of I think either removing “in detail” or changing to “described in detail” would be more correct. Also please add “and” after the comma.

Done

4. Page 4, lines 29-30. This launch level is around 260 Pa. Please provide an average altitude for this pressure level.
It is ~ 8 km. Added in the text.
5. The different shades of blue and red in Figs 3, 4 and 5 are not always easy to distinguish (maybe it is a problem with my printed copy). You could consider adding black labeled contours to improve legibility.
We have redone these figures and tried to improved the quality. We have now confirmed that in the printed version, the contours are clearly visible.
6. Page 7, line 25. A more closer examination. Please remove more.
Done
7. Figure 4. These temperatures are daily and zonally averaged, or shown instead at a given local time? Please mention it in the figure caption.
Added in the caption.
8. The comparison with the observed seasonal variability would be eased if Figs. 4 and 5 used the solar longitude L_s as a measure of time, instead of the Sol number. At least, please consider adding an additional horizontal axis displaying L_s .
We have replaced the axis to the solar longitude, which is more useful, as the referee pointed out.
9. Page 8, line 29. “During southern winter solstices” \rightarrow solstice
Done.
10. The text states (page 5, line 8) that “instantaneous values of the parameterized (unresolved by the model) temperature disturbances T' are impossible to determine” so that an average value $|T'|$ is used instead. However, in all later mentions to these temperature perturbations, T' is used, and not $|T'|$ (e.g. eq. (2), page 7 line 27, page 8 line 18, labels in Figures 3 and 5,...). Please be consistent with the nomenclature across the paper.
Consistency is now provided in the entire text.
11. Page 11, lines 5-6: “without subgrid-scale effects effect included”. Please remove “effect”
Removed .

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