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Interactive comment on "Influence of gravity waves on the climatology of high-altitude Martian carbon dioxide ice clouds" *by* Erdal Yiğit et al.

Anonymous Referee #2

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Review of "Influence of gravity waves on the climatology of high altitude Martian carbon dioxide ice clouds" by Yiğit et al.

General comments

This article presents a modeling study of the influence of gravity waves on the formation of CO2 ice clouds. The signature of gravity waves has been observed in the Martian atmosphere and modeling has shown that they can have a large impact on the thermal structure of the upper atmosphere. This work links the cooling due to gravity wave breaking/saturation with the probability of formation of CO2 ice clouds.

As most climate models of the Mars atmosphere have a horizontal resolution larger than the scale of gravity waves, a parameterization is used to represent these sub-

grid scale effects on the model winds and temperature. The MPI-MGCM is used for the study, which includes a whole atmosphere gravity wave parameterization and a scheme for CO2 condensation/sublimation.

This is a continuation of the work done in Yiğit et al. (2015), extending the analysis from of Ls 0-20° to the rest of the year.

The main aspects of the model and methodology are presented clearly, followed by model results for the first half of the year. The analysis is continued for the full year focusing on results at the 80, 100 and 120 km levels.

The first reviewer has already brought up an important point about the discussion and comparison with observations. It should be more clearly stated that there are very few positively identified observations of CO2 clouds in the second half of the year (and also above 90 km). That does not make this modeling study irrelevant; it is still useful to the community and is a small step towards a better understanding of gravity waves and the formation of CO2 ice clouds in the Martian atmosphere.

Specific comments

I agree with the first reviewer that perhaps the Sefton-Nash et al. (2013) paper is not useful to discuss the results for the second half of the year as that study was not able to distinguish the aerosol type and in fact, found that nearby temperatures tended to be warmer than the CO2 ice threshold. As summarized in Gonzalez-Galindo et al. (2011), some mid-latitude clouds were seen between Ls 200-300° by THEMIS-VIS, but again, the composition could not be determined. Also, maybe the SPICAM stellar occultation measurements by Montmessin et al. (2006) should be in the list of observations for completeness.

The lack of full diurnal coverage of observations makes this kind of comparison difficult. It might be useful to look at daytime and nighttime averages of the shown model quantities (as was done in Yiğit et al., 2015) to better understand the high probabilities

in the second half of the year and at the higher altitudes.

In terms of the discussion, the possible reasons for discrepancies were well presented, for example, the uncertainty in sources and the degree of supersaturation. Two other possible uncertainties perhaps could also be mentioned, one is the radiative impact of water ice clouds in the first half of the year and the other is the vertical distribution of dust in the second half of the year. Both are lower atmosphere phenomenon but do affect the strength of the global circulation patterns. It might be useful to discuss how sensitive the parameterization is to these effects. This may also help to explain discrepancies seen in the comparison of temperatures at 80 km to MCS (figure 4a with Sefton-Nash 2013 figure 10).

In section 2.2 (page 4), it is mentioned that 'This formulation requires also a prescription of the characteristic horizontal scale λ_h of GWs for calculating τ_i', it might be useful to state what is used for this study. Is this value a source of uncertainty as well?

Below are my minor technical comments:

Page 1 line 13: May I suggest: "Thus, Mars has seasons similar to those one is familiar with on Earth."

Page 2 line1: 'on average,' and yes, warmer than what?

Page 2 line 10: suggestion: 'with the exception of harmonics with zero horizontal phase velocities with respect to the surface generated by the flow over topography'

Page 5 line 18: "P must be treated as a certain metric introduced"

Figures: Agree with reviewer 1, figures 4 and 5 x-axis label in Ls would be more useful than day number.

Figures 3c,d and 5a,b,c some contour lines to help distinguish?

Figure 4a very difficult (almost impossible) to compare with Sefton-Nash et al., 2013 figure 10. A change in color scale to match would be useful.

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