Review of the manuscript "Influence of gravity waves on the climatology of high-altitude Martian carbon dioxide ice clouds" by Yiğit et al., submitted to Annales Geophysicae

I am generally satisfied with the modifications implemented by the authors in the revised version of the manuscript. I particularly appreciate the efforts to improve the quality and readability of the figures, which I think are significantly better now. However, I still have problems with the statements made about the comparison with observations, which was the main point in my previous comment.

The authors have introduced an statement acknowledging that the observations in Sefton-Nash et al. (2013) do not discriminate between CO2 and H2O clouds (page 9, lines 26-31 of the revised manuscript), which is good. But it is important to note that Sefton-Nash et al. (2013) use MCS temperature measurements to find that clouds detected during the second half of the year are not CO2 clouds, but very likely H2O clouds of even dust. However, the authors insist in comparing the observations in Sefton-Nash et al. (2013) during the second half of the year (H2O clouds) with their derived probability of CO2 cloud formation. I think that sentences such as "The model reproduces more favorable conditions for CO2 condensation in the midlatitudes regions during wintertime. It agrees with observations in that mesospheric clouds occur more frequently during perihelion (Figure 5g)" (page 10, lines 7-9 of the revised manuscript) are misleading, as they are not comparing apples to apples. CO2 cloud formation probability should not be compared to H2O clouds observations, as the conditions for forming CO2 and H2O clouds are very different.

In my opinion, the two first paragraphs in page 10 still need to be rewritten, focusing the comparison only on the observed distribution of unambiguously detected CO2 clouds. In particular, I think it should be explicitly acknowledged that the prediction of elevated CO2 cloud formation during the second half of the year does not agree with observations of CO2 clouds. Note that, in my opinion, this difference does not decrease in any point the merits of the model. Often the most interesting discoveries come from differences between observations and models. It is also possible that there biases in the observations, and the predictions of the model may motivate future observational searches for clouds in the regions/seasons pointed by the model. The authors already discuss possibilities for the model/observation differences, and possible limitations of the model, so no additional discussion would be needed (although I think the use of a constant O density profile for all conditions, seasons, latitudes and Local Times could be considered another limitation affecting the seasonal variability of the temperatures).