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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Local Time Dependence of Polar Mesospheric Clouds: A model study" *by* Francie Schmidt et al.

Anonymous Referee #2

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General Comments:

This manuscript reports results from the Mesospheric Ice Microphysics And tranSport (MIMAS) model using hourly output prescribed by the Leibniz Institute Middle Atmosphere (LIMA) model in order to draw a variety of conclusions on the variation of Polar Mesospheric Clouds (PMC) over the diurnal cycle. The authors compare their results to a suite of ground-based and satellite PMC datasets and extend their study to include all relevant PMC latitudes and cloud classifications. The authors furthermore draw conclusions about long-term trends in the amplitude of the migrating diurnal and semi-diurnal tidal components of PMC ice water content (IWC). The scope of the study is ambitious and if the results are robust, would significantly advance the state of knowledge on the spatial and temporal variation of some of the most important diagnostic

PMC properties.

However, the reviewer is skeptical that MIMAS is properly characterizing the reported PMC variations. Although the model shows agreement with many of the datasets included in the study, the reviewer is suspicious that in many cases the agreement is fortuitous and does not validate the model ice properties or the model inputs. This is because the authors demonstrate a curious disregard of a variety of relevant observational and modeling studies that show quite different results in both the ice properties and the model inputs. The reviewer lists the concerns below.

Specific Comments:

1. The LIMA inputs largely control the variation of cloud properties over the diurnal cycle. Therefore, Section 5.2 ("Atmospheric background conditions") should be moved to the beginning of Section 2 since everything else flows from those results. Figure 6 (left) is especially important to the rest of the study and shows that the variation of temperature over the diurnal cycle is about +/- 1 K at 83 km at 69 N. The amplitude of this variation is in direct contrast to many other studies showing a much larger observed variation of +/- 3-4 K [Singer et al., 2003; Singer et al., 2005; Stevens et al., 2010; McCormack et al., 2014; Stevens et al., 2017]. The authors need to clarify why they believe their results are more reliable than all of these previous studies. If they cannot, then they need to show how their PMC results respond to this larger amplitude of the thermal tide at PMC altitudes.

2. To further clarify comment #1 and for more direct comparison with previous studies, the reviewer requests an additional table (immediately prior to Table 1) showing the tidal variations at the most relevant altitude that enables the PMC variations. The reviewer suggests in rows "All clouds", "faint", "long-term" and "strong" and in columns "T24 (K)", "T12 (K)", "H2O24 (ppmv)" and "H2O12(ppmv)".

3. The authors need to provide additional details on the vertical distribution of condensation nuclei (CN) used in their simulation. There is reference to a Hunten distribution

on page 3, line 4. If they refer to Hunten et al. [1980] they need to cite this work and they also need to evaluate the reliability of their results against more contemporary studies that include global-scale transport, that have much smaller CN densities [Bardeen et al., 2008; Megner et al., 2008; Rapp and Thomas, 2006].

4. On the top of p. 14 (line 1) the authors state that "the amplitude of the local time dependence increases in absolute IWC values towards the pole". Figure 9 is shown in support of this statement. The reviewer does not understand this result and would like an explanation. Are the authors saying that the magnitude of the thermal tide increases toward the pole? If so, that is in direct contrast to previous modeling and observational studies [Chang et al., 2008; Stevens et al., 2017]. If there is some other reason, then they need to state it explicitly.

5. It would be very useful to see a comparison of IWC from CIPS against the results in Figure 8. To the author's knowledge such has a model-data comparison has not yet been done. The authors should also know that Bailey et al. [2015] directly compared CIPS and SOFIE IWC and found CIPS was a factor of 2-3 too low when measuring at the same local time as SOFIE. This is also relevant to their comparison in Figure 3. The values near 80 N look comparable to the results of Stevens et al. (2017) but a large diurnal variation is inferred by the authors and this needs to be discussed in the text.

6. In Section 7 and Figure 10 the authors report a long-term trend in the amplitudes of the diurnal and semi-diurnal tide. To the reviewer's knowledge this has not been shown before. The reviewer is therefore frustrated that the authors reserve their explanation of this for a future study. If they cannot explain what causes this long-term trend, then they need to withdraw this conclusion from the manuscript until they know the cause.

Technical Corrections:

1. General comment. In all figure captions and table captions for IWC, please explicitly indicate whether values of "IWC=0" are included in the results to avoid any confusion.

Some in the field do not weight their IWC with PMC occurrence frequency and others do so it is important to be clear wherever possible.

2. Abstract, p. 1, line 3. Do the authors mean "…good agreement between model and lidar observations at 69 N"? Please be explicit.

3. Abstract, p. 1, line 5. "…from satellite observations" should be clarified. Please state which satellite observations. Also, the AIM satellite is in a sun synchronous orbit so both CIPS and SOFIE observations are locked in local time. Therefore, these observations are not easily tested against results from a model study on local time dependence. That does not mean that the AIM observations should not be used, but the authors need to better clarify how they are used.

4. Abstract, p. 1, line 7. The maximum to minimum ratio is strongly dependent on the threshold used and this need to be clarified here or the statement should be removed.

5. Abstract, p. 1, line 7-8. This conclusion will depend strongly on how the condensation nuclei are prescribed (see specific comment #3 and Rapp and Thomas (2006, Table 1)). If the conclusion is too uncertain given the model inputs then it should be removed.

6. Abstract, p.1, line 8-9. The reviewer is particularly skeptical of the conclusion about the absolute tidal variation increasing to the pole. Please see specific comment #4 and re-evaluate.

7. Abstract, p. 1, lines 9-12. Please see specific comment #6 and re-evaluate.

8. Abstract, lines 12-13. Please see specific comment #1 and re-evaluate. Also, to avoid confusion the authors need to state a temperature amplitude (i.e. +/- X K or +/- X ppmv) and the dominant tidal component.

9. p. 2, line 15. "Opposite to satellites" should be "In contrast to satellite measurements".

10. P. 2, line 32. “with same” should be “with the same”.

11. P. 3, line 8. “In case. . .” should be “In the case. . .”

12. Figure 1 caption (and throughout manuscript). In order to clearly distinguish what is observed and what is modeled, the reviewer requests that the authors not use the word “data” when reporting their model results. In the middle of the Figure 1 caption therefore “model data” should be “model results” and at the bottom of the Figure 1 caption, “MIMAS data” should be “MIMAS results”.

13. P. 8, line 12. In order to avoid all confusion, the authors should state here whether PMC frequency (or IWC=0 values) is included in the IWC results presented. This is clarified later but should be stated here.

14. P. 9, lines 14-15. The reviewer understands what the authors are trying to say, but this could be confusing. After all, if the PMC threshold is raised high enough then there will be no detections at the mimimum so that the maximum/minimum is infinity. Perhaps it would be more clear instead to say “Hence, the strength of the local time variations is sensitive to the PMC occurrence frequency”.

15. P. 10, Figure 4. The reviewer is a little skeptical that A24/A8 can be determined to 3 significant figures. Could the authors please expand on their decision to include 3 components? For example, what does the solution look like with only a diurnal and semi-diurnal fit?

16. P. 14, Table 2. It appears from the discussion in the text that no threshold was applied to these numbers. If so, please say so explicitly in the table caption. Also, the numbers for A24/A12 seem quite a bit different from those reported by Stevens et al. (2017) for the same time period. Since the approach to simulating the ice particle formation is quite different between the two studies, it would be illustrative to show A24/A12 for temperature and A24/A12 for H2O, perhaps in a separate table, analogous to the request in specific comment 2.

17. Please re-evaluate and revise the conclusions given the specific and technical comments listed above. Thank you.

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