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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 #1

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SUMMARY

This paper presents an analysis of local time variations in polar mesospheric cloud (PMC) properties using a 3-D atmospheric model (MIMAS). The results are compared to local time variations derived from lidar data at a single location (ALOMAR in Norway), as well as zonal average results from the SOFIE and CIPS instruments on the AIM satellite. MIMAS also calculates many parameters describing the background atmosphere [e.g. temperature, water vapor, ice particle radius] that are examined for their contributions to local time variations.

GENERAL COMMENT: For better or worse, we may never get a satellite measurement of PMCs with simultaneous SOFIE-level sensitivity and comprehensive global coverage. So if these model results are to be validated against satellite data, I think

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that presenting curves based on some of those higher thresholds would be quite valuable. The authors might wish to primarily use qualitative statements in the main paper, and provide extra figures in an appendix or on-line supplement (since this paper is a "model study"). But since there is the possibility of non-linear behavior in going from no threshold in IWC to a SBUV-type threshold (for example), I think that providing such information somewhere would help the acceptance of the large variations shown in some aspects of this analysis.

This paper is well-written. Some suggestions and comments related to specific items are provided below.

SPECIFIC COMMENTS

1. p. 1, lines 23-24: So the relative strength of these components (where both are present) is actually a guide to lower atmosphere structure? This is relevant to comment #10.

2. p. 2, lines 13-14: Please clarify that this limitation is due to local time sampling, not spatial coverage.

3. p. 2, lines 15-18: Please note also that in contrast to the previous statement, the restricted spatial coverage of lidar data presents a limitation in terms of how well results from any single location can be generalized to other locations (both latitude and longitude).

4. p. 5, lines 18-22: This seems like a reasonable choice because the model can probably form clouds more easily. However, the next paragraph (e.g. lines 25-27) seems to give a different result. Since local time variations are a perturbation on existing clouds, they presumably indicate increased sensitivity to the effectiveness of formation mechanisms. This sensitivity should be addressed later.

5. p. 6, lines 12-13: Please connect this concept to the ideas mentioned on the bottom of page 1 regarding how mesospheric thermal variations are being forced.

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6. p. 6, lines 23-25: The magnitude of the model variations is significantly larger than the satellite results. Stevens et al. [2017; J. Geophys. Res. Atmos. 122, doi:10.1002/2016JD025349, Section 3.1] discuss the potential differences depending on whether "zero" values are included in averages, but these differences seem large even when that issue is considered.

7. p. 6, lines 28-30: These results can be related to the diurnal and semi-diurnal mechanisms discussed on p. 1.

8. p. 9, line 5: This variation in IWC is still much larger than the fit to the SBUV data ($\sim$15-20% p-p), even given the uncertainty in that result because of the nature of the local time coverage. This makes me question the strength of the statement "compatible to a high degree" on lines 10-11.

9. p. 9, lines 14-15: See "General Comment" at the beginning of this review. Does a threshold of 40 g/kmˆ2 reduce the local time variation down to the magnitude shown in DeLand and Thomas [2015]?

10. p. 10, line 1: The physical arguments presented on p. 1 imply that large ratio values of A24/A12, as listed here, mean that tropospheric forcing of tidal variations is much more important for PMC formation and growth than stratospheric forcing. Is this an appropriate statement?

11. p. 12, lines 4-5: This result seems surprising given the discussion of high sensitivity to particle radius on p. 5, lines 13-15. Even a few nm matters with an rˆ6 dependence. Comments?

12. p. 12, lines 15-16: This seems like a significant variation in PMC altitude, considering the small magnitude of quoted long-term variations in z_PMC by Berger and Lubken [2015].

13. p. 13, lines 16-17: What happens with a higher IWC threshold? DeLand et al. [2011] used OMI data (with IWC > 40 g/kmˆ2) and found very little latitude dependence

in the harmonic fits (although they did not plot change in IWC/brightness vs. latitude, as shown here).

14. p. 14, lines 1-4: Compare this figure to OMI results. The slope between 3-6 h LT is indeed very steep, but it includes many faint PMC and thus potentially larger variations in occurrence frequency.

15. p. 14, lines 7-8: You can also consider the Stevens et al. [2017] discussion regarding definition of occurrence frequency and how it folds into such analysis.

16. p. 14, lines 13-14: Are the differences between these results for A24/A12 and the brightness ratios listed in Table 1 significant? Should the results in Table 2 for 61.5-64.5 N be considered as comparable to the "faint" cloud class in Table 1?

17. p. 14, lines 17-19: You have already discussed the importance of threshold selection (beta_max, IWC) in deriving such local time variations. Can models give some guidance as to whether these variations are more (or less) important in such an analysis (e.g. SOFIE threshold vs. CIPS vs. SBUV)?

18. p. 15, lines 7-8: Recent intervals of 3-4 years in Figure 10(c) with locally larger amplitude and more year-to-year variability (e.g. 1993-1997, 2007-2010) are mostly correlated with solar minimum. Could the internal mechanism for model variations be tied to the level of solar activity?

19. p. 17, lines 13-14: Please add a note that increasing the IWC threshold to satellite measurement levels does change this amplitude significantly. Are different mechanisms (e.g. proportional to number of particles vs. proportional to particle size) more important for either the "no threshold" vs. "satellite threshold" analysis?

20. p. 17, lines 22-23: I don't consider a 4 hour shift "remarkable" here, particularly when the overall variation is a superposition of three harmonic terms.

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