(*Author responses are in italics. Line numbers refer to the revision without tracked changes. In the tracked version deleted sequences are marked red. New text is marked in blue.*)

General comment to reviewer I

We want to thank the two reviewers for the detailed reviews with many useful ideas and suggestions which, we think, have significantly increased the quality of the manuscript.

We have rewritten a substantial portion of the manuscript. In particular, we have added three new tables.

Table 1: Local time variations of background temperature,
Table 2: Local time variations of background water vapor,
Table 5: Local time variations of ice water content.

We shifted section 5.2 (old: Atmospheric background conditions) to a new section 2.2 (Mean state and local time variations of atmospheric background temperature and water vapor). The new section 2.2 discusses in detail new Tables 1 and 2.

We have rewritten section 6 (Latitudinal variations of local time dependence for ice water content) where we now discuss in detail the local time variations of IWC in terms of different thresholds and different latitudes. This includes a new discussion of SBUV thresholds presented in the new Table 5.

The abstract and conclusion sections have been adapted. Also, we have included several new references.

Finally, we decided to remove the old section 7 (Long-term variations 1997 - 2013) which contained a short presentation of possible trends in tidal IWC amplitudes. The reasons for this withdrawal are:

1) This section was rather short, included only one figure, and showed simply a trend behavior of one special IWC parameter, i.e. tidal amplitude, for one latitude and one threshold. The section lacked any discussion and physical interpretation regarding possible sources and causes of such trends.

2) We investigated in more detail the subject of trends in local time variations. It turned out that this is a complex topic which certainly needs further investigations. Several parameters, like latitude and thresholds, play a role which needs to nailed down regarding the impact on local time variations of different ice parameters. Furthermore the effects of possible tidal trends in temperature and water vapor have to be taken into account. Having all this in mind, we decided to cover these topics in near future in a separate paper, which appears to be a better and more systematic way compared to the previous manuscript version.

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 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.

The reviewer addresses an important point about SBUV-type thresholds. The SBUV instrument is typically measuring IWC with a threshold 40 g/km^2. SBUV has observed PMC since 1979. Long term variations in IWC derived from SBUV measurements have been presented by Hervig and Stevens [2014] and DeLand and Thomas [2015].
Most important is the local time correction of SBUV data in order to investigate long-term changes in PMC. We decided to add Table 5 and address this point in detail in section 6 (Latitudinal variations of local time dependence for ice water content).

See new text from page 17, line 23 to page 20, line 10.

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.

Yes, according to classical theory published in the textbook by Lindzen and Chapman (1970) the diurnal tide is mainly excited by solar absorption of tropospheric water vapor whereas the semidiurnal tide is mainly excited by solar absorption of stratospheric ozone.

We mention the report by Lindzen and Chapman (1970). Please, have a look in the textbook at p. 139 ...show that ozone is considerably more important than water vapor in exciting semidiurnal oscillations. This is because ozone excitation occurs over a greater depth than water vapor excitation, and at higher altitudes…

and p.153, ... Thus we are not surprised that the contributions to the modes with negative h's from water vapor (near the ground) are larger than the contributions from ozone (far above the ground). However, the contributions from water vapor absorption to the modes with *positive h's are also larger. This is due to the short vertical wavelengths associated with these modes. The ozone excitation is distributed over a very considerable depth of the atmosphere (ca. 40 kin). Thus, waves excited at one level can destructively interfere with waves excited at another level (see Buffer and Small (1963), and Lindzen (1966b), for a more detailed discussion of this process). For the (1,1) mode (wavelength ~ 28 kin) the region of water vapor excitation is not sufficiently thick (ca. 18 km) for this process to be of great importance. This, however, is no longer true for the (1,3) and subsequent modes…*

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

Done: This sentence was considered redundant and was removed.

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).

Done: We insert … are geographically restricted but …

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.

We agree that local time variations are a perturbation on existing clouds. We discuss this sensitivity in terms of background conditions of temperature and water vapor, see section 6, page 19, line 15.

As shown in section 2.2, phase positions of minimum temperature at PMC altitudes move to some extent during early morning hours backwards in time in poleward direction. Also the phase of the daily water vapor maximum tends to follow this time shift. We conclude that both temperature and water vapor phases cause the general early morning hour structure in IWC and its shift towards higher latitudes.

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.

We comment: In the introduction we only wanted to give a basic information about the fact that diurnal and semidiurnal tides can be related to different heating by water vapor and ozone which deserve special consideration.

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.

Comment: Here we compare model variations with ground-based lidar backscatter data.

A comparison of modeled IWC with satellite data is presented in section 4 which shows that MIMAS values are consistent with those reported by AIM-SOFIE and AIM-CIPS.
Second, Stevens et al. (2017), see their Fig.6, published modeled IWC results for different latitudes including also one SOFIE point.
We added a new Table 5 (section 6), see our response to your general comment. This allows now to compare our modeled IWC with the Stevens results. We see that both model runs describing the local time variation of IWC with a threshold of 40 g/km^2 have similar absolute values and are consistent, page 18, line 11 to page 19, line 8.

Recently, Stevens et al. (2017) reported about model results of PMC IWC calculations with the NOGAPS-ALPHA model using a 1-d bulk ice model (Hervig et al., 2009b). The authors show that the IWC is largest at highest latitudes and yields a morning peak between 5 and 7 LT and a late afternoon minimum equatorward of 80 N regardless of threshold. Diurnally averaged IWC values (threshold of 40 g/km2) are near 100 g/km2 and consistent with those calculated by MIMAS. NOGAPS-ALPHA results of IWC over a diurnal cycle show at 68 N a ratio between IWC maximum and minimum 5 of about 1.5 for a threshold of 40 (see Figure 6a,b in Stevens et al. (2017)) similar to a ratio of 1.7 from MIMAS calculations. Concurrently, absolute IWC local time variations in NOGAPS-ALPHA increase towards higher latitudes and are threshold dependent. Again, these features are confirmed by MIMAS.

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

Again, see our response to your comment 4 and 5.

8. p. 9, line 5: This variation in IWC is still much larger than the fit to the SBUV data (~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.

We are a bit confused since there is neither a fit nor a comparison with SBUV data. We discuss two CIPS and one SOFIE data point. Indeed, the satellite values are compatible with model data. Perhaps you think about the factor of 2. We want to answer that local time variations strongly depend on thresholds, see discussion of new Table 5.

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]?

Done: Yes, increasing thresholds will decrease (relative) local time variability, see new discussion of section 6. The results from our new Table 5 show a ratio between maximum and minimum of about 1.7 at latitudes 64°-74°N which might be not too far away from a value of 20%-30% reported by DeLand and Thomas [2015], see their Fig.8, 9 showing ratios of descending and ascending points.

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?

Your conclusion is highly speculative. Tropospheric water vapor and its longitudinal variations, tropospheric cloud coverage through release of latent heat will induce variations in the source strength of the tidal excitation of migrating and non-migrating diurnal tidal component. But propagating upwards, any tidal motion (being a sum of different Hough-modes) will experience different thermal background conditions. Also a variable structure of horizontal winds in vertical direction will vary tidal propagation conditions. Finally, one has to consider all kinds of dissipation mechanisms, e.g. turbulence, infrared cooling, wave-wave interaction with gravity waves etc., that will influence amplitude and phase of tides. So we have to state that the complexity and diversity of all these processes make individual and manual analysis impossible.

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?

*Comment: Here we discuss the PMC parameter of ice mass density with an n*r^3 dependence. We simply try to analyze which of the two quantities (n versus r) has a larger relative contribution to local time variation in ice mass density.*
*Also note that the remarks on page 8 , line 19, focus on the discussion of a n*r^6 dependence in backscatter.*

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].

The local time variation in PMC altitude is about 500 m. The long term trend in PMC altitude is about -150 m per decade, see by Berger and Lubken [2015] their Fig. 3c. Indeed, local time variations of NLC heights are in a comparable range as long-term trends.

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).

Done: We address this issue in the discussion of our new Table 5, see section 6, that shows the local time dependence of IWC > 40 for three different latitude bands. Our model results suggest that relative effects in local time increase towards the pole.

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.

Done, see page 19, line 9.

On the other hand, DeLand et al. (2011) published local time observations by the Aura OMI (Ozone Monitoring Instrument) satellite instrument which indicates maximum frequency and albedo values at approximately 9-10 h LT at 70 N for the NH 2007 season, with a smaller amplitude and a slight phase shift to 8 h LT at higher latitudes. Hence, model results from MIMAS deviate to some extent from these satellite measurements for 2007. Here we refer to some year-to-year variations of phases in MIMAS (not shown here) which might explain to some extent these differences.

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.

In section 6 we now discuss all kinds of threshold, with and without frequency weighting, e.g. see discussion of Table 5, section 6.

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?

Comparable is the latitude band for 67.-71 from Table 2 (now Table 4) with Table 1 (now Table 3). But have in mind, Table 4 applies for IWC zero counting, i.e. frequency weighting, whereas Table 3 uses brightness threshold intervals. We think, the identification of IWC with faint clouds is not justified.

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)?

According to this point we have rewritten section 6 (Latitudinal variations of local time dependence for ice water content) where we now discuss in detail the local time variations of IWC in terms of different SBUV thresholds, see Table 5 .

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?

This section has been removed, see our general comments.

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?

No, we don't see different mechanisms. Your question about thresholds has been answered in the conclusions. See page 21, line 13.
We calculated a climatology of IWC local time variations from a 35-y average from 1979 to 2013 for different thresholds and latitude bands, which might be useful for satellite data analysis in order to perform local time corrections. Local time variations are found to depend on latitude and threshold conditions. For the latitude band 64–74 N and a threshold of IWC > 0 g/km2 IWC maximum and minimum values occur around 3 LT and 19 LT, respectively, with a ratio maximum to minimum of 6.6. For a threshold of IWC>40 g/km2 the local times for maximum and minimum are identical, but the ratio changes to 1.7. A phase shift exists for the IWC local time behavior towards the pole, which is independent of the threshold value.We find the absolute IWC local time variation to generally increase with latitude.

Furthermore, the IWC maximum moves backward in time from 8 LT at mid latitudes to 2 LT at high latitudes.

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.

Conclusions have been rewritten, ‘remarkable’ is absent.