Response to the comments of Reviewer #2

We are very much grateful to the reviewer for the very helpful comments and suggestions. After consideration of all of them a much-improved paper has resulted. Detailed responses to the Reviewer (in italics) follow after each comment.

- **Reviewer's comment:**

Major Comments:
Given the limited amount of data used from MIPAS, it would be useful to better quantify the observed variability in PMC properties throughout the paper. For example, what is the range of "top altitudes" from MIPAS? If there is a high degree of variability, the mean calculated from 12 or 19 days is likely insufficient to converge on the "true" mean.

Response:
We agree with the referee on this point. We stated already in several instances of the manuscript the large variability of PMCs in MIPAS measurements, caused not only by their natural variability but also for the large single measurement noise (already included in the text). The variability is also evident in the daily zonal mean examples shown in Fig. 1. It is true, however, that we did not provide a quantification of the variability in some cases (note that in most of the zonal mean figures and maps we already stated the estimated noise error). We have quantified the variability of the "top altitude" and found that it is rather large (1 sigma values changing from 1.6 to 2.7 km, depending on the mode of measurement and latitude).

The next couple of sentences have been added in Sec. 3.1:
"The highest altitude of PMCs derived from MIPAS NLC mode measurements is highly variable, as can be seen in the typical examples shown in Fig. 1. At 70N, it is about 88.5 km (Fig. 3b). Its variability depends on latitude and takes 1-sigma values from 2.7 km near 70º to 1.6 km near the pole."

We have also estimated the variability of the bottom altitude and of the mean altitude layers (see responses below).

- **Reviewer's comment:**

I would check Figure 5, as I would expect better agreement with SOFIE based on the rest of your analysis. In 2009, for example, the average of 175 MIPAS profiles at 87 km is ~15ng/mˆ2, compared to ~2ng/mˆ2 from 165 profiles from SOFIE. If this figure is correct, then I think the large differences and variability compared with SOFIE call into question the entire analysis. It also looks like you may even be seeing ice above 90 km in Figure 5 and are simply setting these values to 0. Also, Figure 4 and Figure 5 do not seem to be in agreement. 2009 and 2010 show a peak in mass density above 87 km that is not represented at all in Figure 4.

Response:
This is a very good point. We have revised the comparison following the reviewer's comment below suggesting that we should compare MIPAS measurements with a similar local time as those taken by SOFIE. Thus, we have re-done Fig. 5 including ONLY 10 pm measurements, which are closer to SOFIE's local time of observations in the NH. The agreement is much

better now (see new Fig. 5 as Fig. 1 below) because of the smaller ice concentration at 10 pm, particularly at altitudes of 85-87 km (see Fig. 12). The agreement is better now for all years in the region above 85 km. Still we obtain a significantly larger ice amount above around 85 km for 2009 and 2011, although, they are of similar magnitude than the peak below. It is also worth noting that MIPAS does not have a vertical resolution as good as SOFIE. However, the column amount is a more comparable quantity. We see that even for these years, the agreement in the column amount is very good 46 μg/m2 (SOFIE) vs. 41 (MIPAS) for 2009, and 60 μg/m2 vs. 56 for 2011. Thus, although the noise in MIPAS measurements is rather large and the vertical resolution is not as good as in SOFIE, we do think that our measurements are very valid and the analysis is trustworthy.

Action: Fig. 5 has been revised and the text has been accordingly revised.

- **Reviewer's comment:**

  It also looks like you may even be seeing ice above 90 km in Figure 5 and are simply setting these values to 0. Also, Figure 4 and Figure 5 do not seem to be in agreement. 2009 and 2010 show a peak in mass density above 87 km that is not represented at all in Figure 4.

  Response:

  That is correct. In some days there appear some small radiances at tangent heights above 90 km which appear as ice in the retrieval. We think this is not real but caused by the large variability in the offset of MIPAS spectra, which, when integrating over the large spectral range of 770–920 cm^{-1} (see Lopez-Puertas et al., 2009) results in some significant radiance-integrated offset. This offset changes significantly with latitude and season and also with altitude. We have improved significantly the offset correction from Lopez-Puertas et al. (2009) (see the text) but there are still some scans that show some signal above 90 km. Since we think there is no physical reason for attributing this signal to PMC's we did not show it.

  About the possible disagreement between Figs. 4 and 5, it is apparent. Fig. 4b shows the mean of all the years, not only 2009 and 2010 but also 2008 and 2011. The latter show also a peak (more pronounced in 2011) at lower altitudes. As a result, when averaging over all years, it results in a kind of broad peak near 70º N extending from 83 km up to 87 km, as shown in Fig. 4b. Actually, we do see a very small increase in this broad peak at about 85-87 km (follow the "10" contour line in Fig. 4b).

- **Reviewer's comment:**

  Comments:
  General: make sure you define each acronym once, the first time it appears.

  Response:

  We have revised the acronyms and have deleted a few double definitions. We have retained, however, the duplicity between the abstract and the rest of the text and also when they appear in the figure caption (in order to facilitate the reading). Also, we have duplicated the definition of uncommon terms like MA and UA, when they appear in the text far away from where they were defined. Again, we think the addition of these few extra words justify the much easier reading of the paper.

- **Reviewer's comment:**

Line 25: can't temperatures be lower that 150K?

Response:
Yes, they can. We have replaced "as low as " by "about".

- **Reviewer's comment:**
  The paragraph beginning at line 35 provides little information except to say that PMCs have been studied. What did these papers show?

  Response:
  The idea with this sentence was to express that PMCs have been measured by many instruments using different techniques and also from the modeling point of view. This is a kind of summary of major observations and modeling efforts. The particular aspects derived from some of these instruments are detailed in the following paragraphs.

- **Reviewer's comment:**
  Line 55: This paragraph could be reduced to say that similar results were found by Stevens and Hervig [2014] using SBUV.

  Response:
  The paragraph actually contains two pieces of information, one about the temporal evolution of PMCs and another one about the SBUV and SOFIE data comparison. We have re-written the paragraph to:

  Similar results were found by Hervig and Stevens [2014] by using SBUV data and a different method for calculating the ice water content (IWC). These authors also compared SBUV and SOFIE data and found good agreement in average IWC if an appropriate threshold was applied to the SOFIE data set and consistent day-to-day and year-to-year variations between both data sets were used.

- **Reviewer's comment:**
  Line 70: What do you mean by "the responses"? Are you saying that the 27-day solar cycle somehow accounts for long term PMC trends?

  Response:
  No, we meant to say that the 27-day solar cycle variations were not induced directly by the 27-day solar cycle variations in the solar flux but by 27-day variations in the vertical winds. The paragraph has been written to:

  Thomas (2015) have studied the solar-induced 27-day variations in polar mesospheric clouds using 15 seasons of SOFIE data and suggested that the 27-day variations in the PMCs are due to 27-day variations of vertical winds.

- **Reviewer's comment:**
  Titles of Figure 4 are not consistent with figure caption. It looks like the figure caption is wrong.

  Response:
  Correct. Sorry about that. It was a leftover of a previous figure.

- **Reviewer's comment:**
  In Figure 5, put the red line on top of the shading.

  Response:
  Done.

- **Reviewer's comment:**
  What is the reasoning for showing a single day in Figures 10 and 11? Wouldn't it make more sense to do this analysis using all the data, so you could more easily compare with previous work?

  Response:
  There are several reasons. First, not all the H2O middle and upper atmosphere data have been processed but only the period of the NLC season in 2005. Secondly, it was not the main aim of this paper to present a detailed study of the analysis of the PMCs (ice) and gas phase H2O data. This is beyond the scope of the paper. However, we thought it is useful to illustrate this as an example since very few (if any) instruments are able to measure global-latitude fields of temperature, ice, and water vapour simultaneously. A more quantitative and extended study is planned for the future.

- **Reviewer's comment:**
  Why do you show latitudes equatorward of 50◦ in Figure 12? Also, the temperature anomalies do not seem to correspond to the anomalies in ice volume density. Maybe this is because you are comparing January and July differences in temperature to full season differences in ice volume density.

  Response:
  The main reason for showing latitudes equatorward of 50º was to illustrate global tidal features and that the temperatures differences at mid-lat. and near the polar region are due to tides. We agree that it is much easier for looking at correlation between ice concentration and temperature to show the same altitude/latitude coverage in both figures. Thus, this figure has been re-plotted along this line. Note also that the temperature differences have been plotted for the same days of measurements of the PMCs so they are directly comparable now. We have also included new panels with the relative am-pm ice volume densities differences and removed a few outliers (with V_{ice}>4-sigma).
  In the revised version of the text we have also refined the discussion on the temperature and the ice anti-correlation and state that it is not clear in the SH.

- **Reviewer's comment:**
  Also, how do your results in Figures 12a and 12b affect your comparison to SOFIE in Figure 5? Would it make sense to only compare am or pm to SOFIE? SOFIE observes sunrise in the NH summer and sunset in the SH summer.

  Response:
  This is a very good point. As described above we have now compared MIPAS pm with SOFIE NH measurements, taken around 23pm on average, (revised Fig. 5) and the agreement is much better. Thank you very much for pointing this to us.

- **Reviewer's comment:**
  Figure 9 seems to show two distinct populations for the NH and SH. I don't think it makes sense to do a regression analysis of both hemispheres. Looking at the left panel, it seems that there is a strong linear trend in the NH, but in the SH, ice water content seems independent of frost point altitude. Maybe expand your analysis to discuss hemispheric differences and compute the correlation in each hemisphere separately.

  Response:
  If it is a physical reason for such a correlation it is not clear why it should happen in one hemisphere and not in the other. For that reason we presented the analysis together. It is true that for the NLC mode (left panel) there is a stronger correlation in the SH than in the NH (note that this is contrary to what you state). However, the correlations for NH (black) and SH (red) are similar for the MA+UA modes (right panel).
  We have now repeated the analysis for each hemisphere separately and found the same conclusion (see figures 2 and 3 below). We find overall a rather good correlation except in the case of the NLC mode in the NH. This could be caused by the smaller statistics we have for this case.
  Thus, because we have found the same behavior separately in each hemisphere, and the other reviewer suggested to shorten/remove this section, we have replaced the previous 2-panel figure by one panel showing the analysis for all modes + the two hemispheres together.

- **Reviewer's comment:**
  Line 217: There is no 70◦N in the bottom-left panel of Figure 1

  Response:
  Correct. It refers to a previous figure showing the zonal mean distribution for the 5th July 2009 in the NLC mode. We have removed this reference in the revised manuscript.

- **Reviewer's comment:**
  Figure 2: Any thoughts on what drives the zonal variability observed here (i.e., planetary waves such as the 2-day or 5-day wave)? See Siskind, Nielsen, and Merkel

  Response:
  Yes, it could be driven by PW activity. However, MIPAS was operating in the middle/upper atmospheric modes only occasionally (see Table 1) and unfortunately the time sampling does not allow for unambiguous determination of individual periods of zonal oscillations. Effects of the 2-day and 5-day waves are aliased with stationary waves and tides and cannot be isolated. Nevertheless, we now mention the possibility of the wavenumber-1 longitudinal variations in the plot being due to planetary waves, but mainly based on findings from other authors.

- **Reviewer's comment:**
  Line 261: What is the standard deviation for MIPAS?

  Response:
  The standard deviation of the bottom altitude of PMCs in MIPAS varies with the mode of observation from +/-1.2 km for the NLC mode to +/-1.8 km for the MUA mode. Thus, the bottom altitude in MIPAS is 80.9+/-1.2 km for the NLC mode and 80.0+/-1.8 km for the MUA mode. These data have been included in the text.

- **Reviewer's comment:**
  You talk about Figure 4 like it is a 4 panel plot, but it only has 2 panels

  Response:
  Correct. Sorry about that, it is a leftover of a previous version of the figure. The text has been changed: "Fig. 4b and 4d" to "Fig. 4a and 4b", and the legend of the figure appropriately corrected.

- **Reviewer's comment:**
  Line 295: Bardeen et al. [2010] has done this exact analysis.

  Response:
  *I think this is a misunderstanding. We refer to a detailed comparison between CARMA and **MIPAS data**, covering high latitudes, not with SOFIE data. We have clarified this in the text: "A thorough comparison with the CARMA model and MIPAS data, including higher latitude regions, would be very useful but is beyond the scope of this paper."*

- **Reviewer's comment:**
  Line 355: What lidar measurements, and at what latitude?

  Response:
  With ALOMAR lidar measurements in Norway (69ºN). This has been added to the text.

Reviewer's comment:
Line 390: I don't understand how this result is consistent with Figures 7 and 8, which have nothing to do with the bottom altitude.

Response:
We wanted to say that the denser PMCs, which, as Fig. 8 shows, are closer to the poles, are located at lower mean altitudes, as shown in Fig. 7.
We have clarified this in the revised version: "This is consistent with the behaviour shown in Figs. 7 and 8 where the denser layers are usually found near the poles and at lower mean altitudes."

- **Reviewer's comment:**
  Line 460: This anti-correlation doesn't seem apparent to me.

  Response:
  We now clearly state that the anti-correlation is generally good only in the NH but not in the SH. As mentioned above, we have re-written Section 9.

- **Reviewer's comment:**
  Line 513: Didn't you show this in Figure 1 and 3?

  Response:
  Not really in Figs. 1 because it shows some examples which might be or not a general behavior. About Fig. 3, yes, it shows the inter-hemispheric differences qualitatively although

not quantitatively. In any case, we are recapping the major results in the Conclusions' section and find it useful mentioning this again.

- **Reviewer's comment:**

  Minor Changes:

  All the minor changes have been included. Thank you very much.

  FIgures:

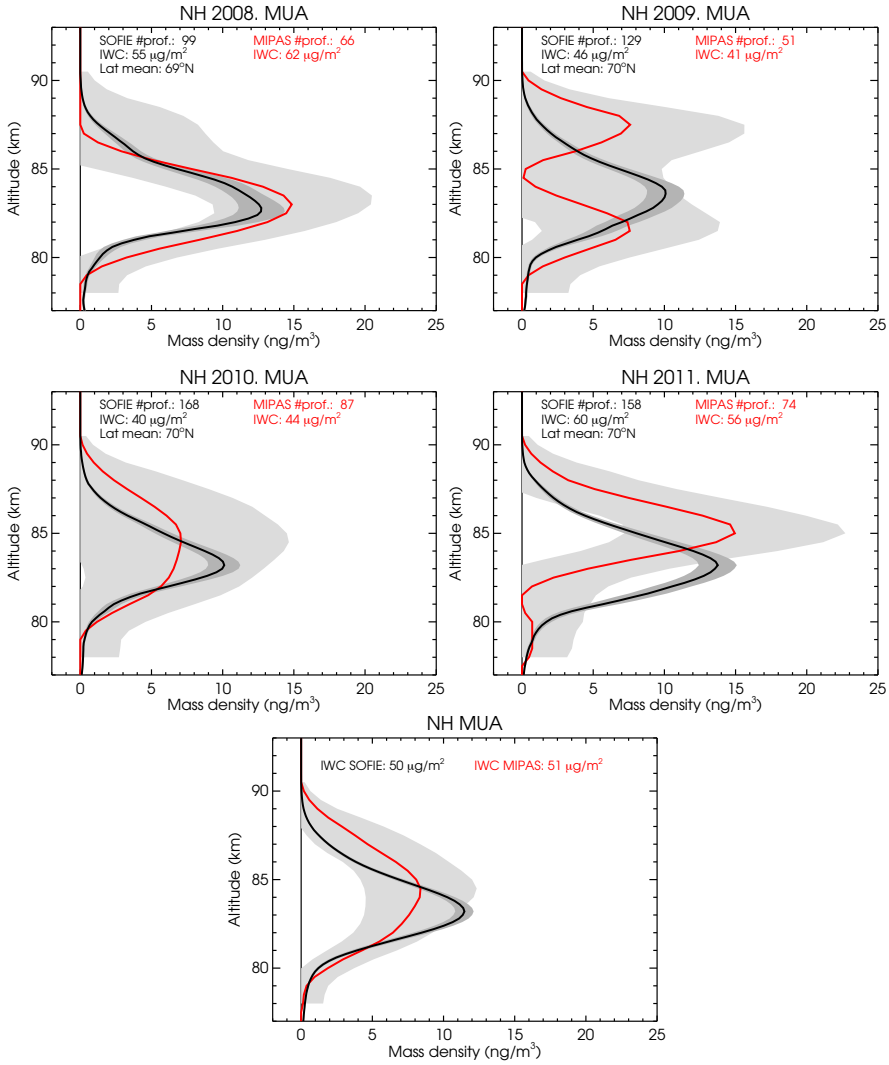

Figure 5. Comparison of the ice mass density of MIPAS MUA modes of measurements (see Table 1) with SOFIE v1.3 L2 data for the 2008 to 2011 period and the mean of the four years in the NH. The solid lines show the mean profiles, SOFIE in black and MIPAS in red. The shaded areas are the standard deviations divided by the square root of the number of profiles. The means of the IWC are also shown.

Fig. 1. New Fig. 5.

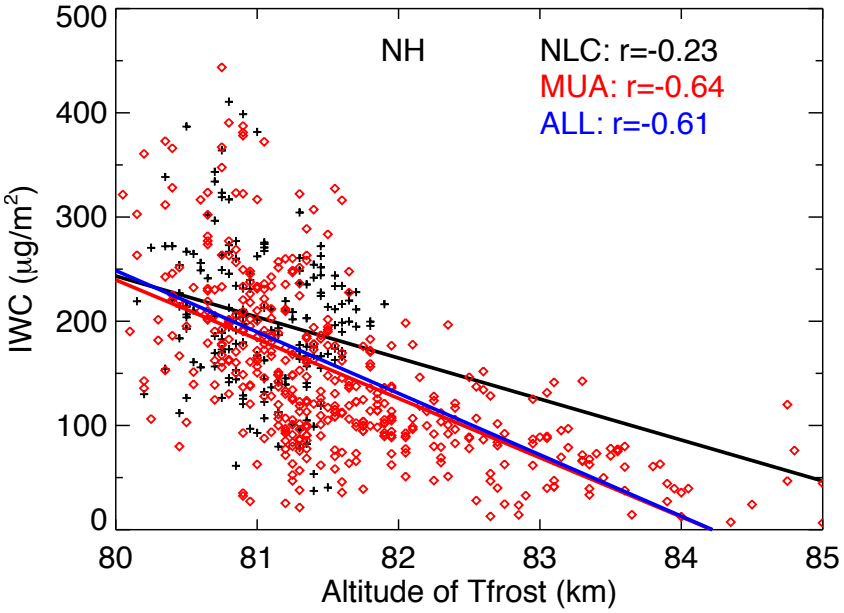

Fig. 2. Correlation between IWC and the altitude of the lower branch of the frost point temperature contour for the data taken in the NLC (black '+') and MUA (red diamonds) in the NH.

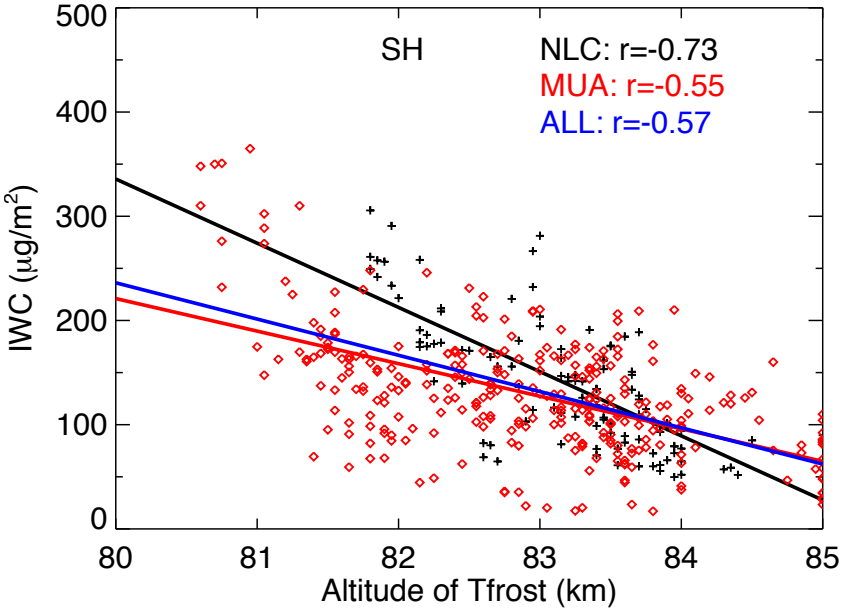

Fig. 3. As Fig. 2 but for the SH.