

Response to the comments of Reviewer #1

We are very much indebted and thankful to the reviewer for the very detailed reading of the manuscript and the very helpful comments and suggestions. Although the comments from the reviewer were rated as "minor", they were very helpful for revising the paper. After consideration of all of them a much-improved paper has resulted. Detailed responses (in italics) follow after each comment.

General comments

• Reviewer's comment:

General comments: overall quality of the discussion paper This study introduces and describes a PMC dataset from MIPAS IR emission observations, shortly presents the retrieval method that was already published in a previous paper, provides evaluation of its quality by comparison of MIPAS ice mass density and cloud altitude to AIM SOFIE observations, and discusses MIPAS cloud properties in relation to previous findings. The important advantages of an IR emission dataset like MIPAS compared to other (UV and/or VIS) remote sensing datasets are: o that observations are available during day and night o but also that retrieved cloud properties are independent of the highly uncertain PMC particle size distribution, which has to be assumed for the retrieval of cloud properties of many other satellite datasets. As such, this paper represent a substantial contribution in the form of new data to scientific progress within the scope of Atmospheric Chemistry and Physics. While the paper would profit from proof-reading by a native English speaker, there are also weaknesses in the discussion of the MIPAS measurement threshold, the discussion of the disagreement with SOFIE ice mass density profiles above 84km (Figure 5), and the discussion of MIPAS diurnal variations. A more thorough evaluation of the dataset could be achieved by additional comparison to another PMC dataset that also offer polar coverage, however, that would probably lengthen this paper too much. On the other hand, the paper can be shortened by emitting results that are not discussed in detail, for example Section 5. The conclusion should be more quantitative when summarizing the agreement with SOFIE. In summary, I think that all the here mentioned weaknesses can be resolved using the existing dataset, so I recommend this paper for publication in ACP with minor revisions.

Responses:

We are glad to hear that this work represents a "substantial contribution".

There was already available a new version revised by a native English speaker where many of the suggested corrections were already done. This was probably caused by the kind of double-review system of ACP.

Figure 5 has been revised, following the suggestion of the other Reviewer that we should considered the solar local time when comparing MIPAS and SOFIE observations. As a result the agreement is now much better. The text discussing this comparison and the Conclusions section have been revised (see comments below).

Also, the section on the diurnal variation has been significantly revised including the figures (Figs. 10) (see comments below).

We agree that comparison with other instruments should be done, specifically with the IWC of CIPS. However, as recognized by the reviewer, this would lengthen the paper too much.

About the shortening of Sec. 5, the other reviewer actually suggested to discuss the correlation separately for each hemisphere. This section is already short and reports a finding that we consider important. Hence we have kept the text (actually expanded as suggested by the other reviewer) but it has been merged with the previous section. Figure 9 has also been reduced to just one panel.

We have revised the conclusions about the MIPAS-SOFIE comparison being now more quantitative.

Specific comments: individual scientific questions/issues

• Reviewer's comment:

1. You compare the MIPAS PMC dataset to SOFIE, which observes PMCs at just one latitude each day. This latitude is slowly varying during the PMC season, but basically this restricts your comparison to a narrow latitude range. Have you considered comparing your dataset to other satellite observations, e.g., from CIPS, OSIRIS, SCIAMACHY, ...?

Response:

There are two major reasons of why not extending the comparison of MIPAS data to other instruments. First, the difficulty in choosing a common quantity that characterizes the PMCs. As discussed, MIPAS measures the ice volume density, irrespective of their particle size distribution. Most instruments measuring in the UV-VIS are not sensitive to the small ice particles existing in the upper part of the PMC layers; thus they are not the most appropriate instruments for comparison. CIPS might be also useful but provide, to our knowledge, only the integrated column ice (IWC), not profiles. BTW, we already considered the comparison with CIPS a couple of years ago, in conversations with Cora Randall, but comparing cloud coverage is difficult and certainly not very quantitative. Anyway, as you say, that comparison would lengthen the paper too much.

• Reviewer's comment:

2. You retrieve the ice volume density as it is independent of the assumption of the particle size distribution, which is considered uncertain. Have you considered retrieving the particle size and number density, using the same assumption that the SOFIE team uses?

Response:

*MIPAS does not have information on the particle size. SOFIE extract that information using different channels at different wavelengths in absorption. Unfortunately this is not possible with MIPAS. This is a limitation of the **IR emission** technique. We are sensitive to the volume emission but cannot distinguish the particles' size.*

• Reviewer's comment:

3. Do you have plans for making this dataset publicly available?

Response:

Yes, we participate in MesosphEO, a project of ESA, and it is planned that we provide the PMC dataset as part of our duties.

• **Reviewer's comment:**

4. Your introduction should state clearly which SOFIE version you are using. I have found this information in the figure caption of Figure 5, but it belongs in the introduction.

Response:

We agree. We have now included in the introduction that we use version 1.3 of SOFIE data.

• **Reviewer's comment:**

5. P3 L24-25, also P8 L5: I'm missing a more careful discussion of the MIPAS measurement threshold as done by Hervig et al., Interpretation of SOFIE PMC measurements . . . (2009). Some points regarding the SOFIE detection threshold and how it affects the radius retrieval from that paper: - The SOFIE ice detection threshold corresponds to $M_{ice} \sim 0.06 \text{ ngm}^3$. - It is important to note that particle size is only determined when the extinction $\beta(1.037)$ is above the noise. - Although SOFIE can rarely determine particle size for the most tenuous clouds, size is characterized over the dominant range of measurements.

Response:

As an IR emission instrument, the detection limit is usually imposed by the instrument's noise mapped into the retrieved quantity, the ice volume density in our case, in the retrieval scheme. We have indicated this value in the single measurements as well as when doing any kind of averaging, which reduces it by the square root of the number of averaged data. Since we do not retrieve the particles' size, we have no detection threshold in the sense discussed in SOFIE measurements.

• **Reviewer's comment:**

6. P3 L30: It also operated with a high sensitivity – How do you define high sensitivity?

Response:

The noise equivalent spectral radiance (NESR) of the 5 MIPAS bands are detailed in the given reference of Fischer et al. (2009). In particular, in the spectral region where the measurements analysed were taken (shorter-wavelength end of band A), is about $20 \text{ nW}/(\text{cm}^2 \text{ sr cm}^{-1})$ (it slightly changes from spectrum to spectrum and with wavelength). This was already stated in L1 of page 5. The reference was also given already.

• **Reviewer's comment:**

7. P6 L8-10: Please comment on how a potential 5-10K cold or warm bias affects your retrieved clouds. Do you believe The MIPAS temperature measurements more/less than those of other instruments?

Response:

*This point is discussed in more detail and quantitatively in P7, lines 4-7. A 5 K error in temperature induces approximately (Planck function at 145 K and 800 cm^{-1}) an error of about 30%.
About your second question, it is difficult to answer. The detailed validation carried out by García-Comas et al. (2014) have shown that MIPAS temperatures are in between SABER, MLS and OSIRIS on one side, and ACE and SOFIE on the other. One major point here is the vertical resolution, rather good for SABER and ACE, moderate in the case of MIPAS and rather coarse for MLS. SABER, however, as well as MIPAS, are prone to non-LTE effects and to*

uncertainties in the atomic oxygen, while ACE is free of non-LTE. We are very confident in our retrieved MIPAS temperature but certainly cannot discard a systematic error of +/- 5 K near the polar summer mesopause.

• **Reviewer's comment:**

8. Regarding all figures plotted vs. altitude: shouldn't the vertical axis be tangent altitude, not altitude?

Response:

*No, it shouldn't. We normally use tangent height if we plot the measured **radiance** at the limb tangent heights (see, e.g., Fig. 4 in López-Puertas et al., 2009). However, after we perform the retrieval, the retrieved quantities (temperature, H₂O vmr and ice volume density) are all expressed in actual vertical heights in the atmosphere. See, for example, how radiances in the mentioned figure, due to the limb geometry are very large even below the PMC layer. Note this is not the case in the quantities shown in this paper.*

• **Reviewer's comment:**

9. Do you have enough lines of sight through one cloud volume that you could apply a tomographic algorithm? Such an algorithm has the advantage that it solves the problem of clouds in the fore- or back- ground being assigned anomalously low tangent altitudes. You mention this problem on P8/9. Due to this problem SOFIE discards all clouds below a tangent altitude of 79 km. Examples for tomographic algorithms applied to PMC data: Hultgren et al., First simultaneous retrievals of horizontal and vertical structures of Polar Mesospheric Clouds from Odin/OSIRIS tomography, 2013; Hultgren and Gumbel, Tomographic and spectral views on the lifecycle of polar mesospheric clouds from Odin/OSIRIS, 2014.

Response:

We did not try but it would worth to do, at least on an orbit-by-orbit basis. However, the current version of our retrieval processor does not have this capability. Extending the processor for including this option would take rather long, certainly beyond the deadline for submitting these responses. This is, however, a very point that we will consider for the future improvements of the retrieval.

• **Reviewer's comment:**

10. P9 L8-9: Please comment on why in the upper left panel of Figure 2, the clouds fill only a small portion of the region where the temperature is below the frost point temperature, while on the lower left panel (also SH) the cloud coverage seems to agree much better with the frost point temperature boundary.

Response:

The fact of the temperature being smaller than the frost temperature for having PMCs is a necessary condition but not sufficient. Other factors that influence the presence of PMCs are availability of nuclei for condensation, sedimentation, transport, ice growth and sublimation time dependence, and particle size effects on saturation vapor pressure. On the other hand, even assuming thermodynamic equilibrium, T_{frost} is the temperature below which ice formation is possible but only at 5-10K lower temperatures the ice growth really becomes asymptotic (Hervig et al., 2009). That is, we do not expect having ice clouds everywhere

temperature is below the frost temperature. Finally, we estimated T_{frost} from a constant mean water vapor profile, whereas water vapor changes. This variability has not been taken into account in the evaluation of our T_{frost} .

• **Reviewer's comment:**

11. P9 L8-9: Please comment about the possibility that you see the effect of 5-day planetary wave activity in Figure 2. You could also comment about the possibility to use your dataset to help track the effect of space shuttle exhaust on PMCs, e.g., Stevens et al., Antarctic mesospheric clouds formed from space shuttle exhaust, 2005; Stevens et al., Bright polar mesospheric clouds formed by main engine exhaust from the space shuttle's final launch, 2012; Stevens et al., Polar mesospheric clouds formed from space shuttle exhaust, 2003.

Response:

MIPAS was observing in the middle/upper atmospheric mode during and right after the launch of the space shuttle in July 2011 (see Table 1). We have quickly looked at that and found hints about increasing of the ice density near ALOMAR on 9th July 2011. However, this requires a more careful analysis that is beyond this work.

• **Reviewer's comment:**

12. P11 L12-13: reformulate sentence, e.g.: Using the NLC mode data at similar NH latitudes, we derive a mean bottom altitude of ~81 km, slightly lower than that of SOFIE. But I think "similar NH latitudes" is too imprecise. It is not clear how exactly you choose the MIPAS latitude for comparison to SOFIE: do you use one mean latitude value, or a daily changing latitude value based on the changing SOFIE latitudes? Please describe your method better.

Response:

We now write that the comparison is done with MIPAS zonal means in a latitudinal band extending ± 2 deg around mean latitude of SOFIE's measurements.

• **Reviewer's comment:**

13. P11 L13-14: Note, however, that we have not excluded any PMCs here, whereas in SOFIE those found below 79 km were excluded. – You're not comparing apples and apples: what happens when you treat MIPAS observations like SOFIE did? Do you then get a better agreement (as expected)?

Response:

*When treating MIPAS observations like SOFIE the change is very marginal, only 0.1 km. We have re-written the sentence to:
"In SOFIE measurements the PMCs with a peak extinction altitude below 79 km were excluded (Hervig et al., 2009b). Applying a similar threshold to MIPAS data, however, does not change significantly the bottom altitude.*

• **Reviewer's comment:**

14. P11 L28: at those latitudes – please be more precise: what is your coincidence criterion? It may be worth showing a histogram of SOFIE and MIPAS ice mass. This would be helpful in convincing the reader of the nice agreement.

Response:

The coincidence criterion has now been specified, within 2 degrees of SOFIE latitude measurements.

• **Reviewer's comment:**

15. P12 L1-3: I don't understand your explanation why MIPAS and SOFIE are expected to observe less ice mass density than the lidar: if MIPAS and SOFIE are able to observe a BIGGER population of the total ice mass by ALSO observing the smaller particles (that the lidar does not observe), shouldn't the resulting ice mass density be BIGGER than the lidar ice mass density?

Response:

Our reasoning (see also Hervig et al.) is that if one instrument samples only the larger clouds, it is clear that one would get a mean bias to larger values with respect to another that sample all values, large and small.

• **Reviewer's comment:**

16. P13 L9-10: ice particles are the smallest and it could be that MIPAS is more sensitive than SOFIE to those particles. – Here you argue that a more sensitive instrument should result in higher values of ice mass density. On the previous page you have argued the opposite: that the larger sensitivity of MIPAS to the smaller ice particles than lidar will lead to lower mean ice mass density values. It makes the impression as if you are contradicting yourself.

Response:

*We agree that it looks contradictory.
We have removed that sentence.*

• **Reviewer's comment:**

17. If you haven't done that yet, I would suggest talking to Mark Hervig directly about possible reasons for the disagreement in ice mass density above 84km as seen in Figure 5. Please also discuss possible reasons for this disagreement in more detail, e.g., the role of geophysical differences.

Response:

Thank you very much for the suggestion. We did not have the opportunity to talk to Mark Hervig about these differences. The differences in the revised version, however, are much smaller. As suggested by the other reviewer, we have now considered the criterion of having the closest possible solar local time. This has greatly improved the comparison.

• **Reviewer's comment:**

18. P17 L15-17: The discussion of Figure 9 is very short (2 sentences) and contains only a description of Figure 9. Do you have an overall point you want to get across with Figure 9? Is this a new result or do you show this to relate MIPAS observations to previous studies (which?)? Otherwise, please consider omitting Figure 9 and Section 5.

Response:

It is true that the discussion is short and may do not deserve a Section on its own. We have merged it with the previous section.

However, the result is clear, we think. The major point being that when the atmospheric temperature is below the frost point temperatures at lower altitudes, the PMCs are dense (IWC is larger). We think this point is already clear in the text. To our best knowledge, we are not aware of any previous study on this.

• **Reviewer's comment:**

19. Last paragraph of Section 6: the discussion about column abundance of gas phase H₂O around 70° is not supported well by Figure 10, which shows the gas-phase H₂O vs. altitude and latitude.

Response:

That is correct. There are several points here. First, around 70 deg, the distinction between the hydration and dehydration layers in MIPAS data is not so clear as reported by Hervig et al. However it is evident at higher latitudes.

*Secondly, the quoted values of MIPAS H₂O anomalies in both regions should be given as integrated columns and not as the peak values in both regions. When integrating, the column in the hydration region, 6-7 ppmv*km, is about twice larger than in the dehydration region, 3.5-4.5 ppmv*km. On this point MIPAS and SOFIE do not agree well.*

Third, these anomalies in the gas-phase water are both much smaller than the amount in ice. On this point both SOFIE and MIPAS agree very well.

We should also have in mind that in order to calculate accurately the excess and deficit of water vapour in the lower and upper regions, we should use the H₂O gas profile corresponding to the same geolocations but with no ice. Such profile (the background profile of Hervig et al.) was estimated by Hervig et al. from measurements where no ice was present. To determine such profile in the case of MIPAS is very difficult because of the high noise in the single profiles of the retrieved water vapour. Hence, we used as the "no-ice" H₂O gas profile the zonal mean profile corresponding to all latitudes. This could partially explain the SOFIE and MIPAS differences.

The text has been revised along these lines.

• **Reviewer's comment:**

20. P20 L1: exhibit a very good latitude/longitude spatial correlation – I don't agree: while the dehydrated "hole" in H₂O at 90 is neatly centered on the pole, the clouds' center of mass is shifted towards northern Greenland, and at 80km the center of mass of the hydrated region is over the northern Pacific. I wouldn't call this "very good latitude/longitude spatial correlation", but expect a comment on this "rotation".

Response:

The comment is very pertinent. We agree that we should not say a "very good ... correlation". It is the first order broad feature what we want to highlight. Also, we do not expect a perfect correlation, as is not expected an immediate response neither the same structure at different altitudes (e.g. propagation of gravity and planetary waves). The text has been changed in this sense.

• **Reviewer's comment:**

21. P20 L4-5: I don't agree with your statement that the location of the hydration region agrees well with SOFIE observations. From Figure 10 it looks like the MIPAS peak altitude

of the hydration region is at 80km, whereas the bottom of the PMC layer is at 81 km. If anything, then the MIPAS peak in hydration lies BELOW the bottom of the PMC layer. Or do you also count the dark blue shading as PMC? Then I would agree. But for that it would be useful to know if these dark blue PMC observations are above the noise threshold.

Response:

You are fully right. We understood that the peak altitude of the hydration region in SOFIE is 0.3 km BELOW the bottom PMC layer but it is ABOVE.

We have corrected the text stating now that they do not agree.

• **Reviewer's comment:**

22. P20 L9-10: What do you mean with being "more pronounced"? That the ice layer contains even less H₂O than the hydration/dehydration layers? At higher latitudes the dehydration (-0.3 or -0.4 ppmv maybe) looks much less pronounced than the hydration (1.4 ppmv), which does not agree with the SOFIE results that they are roughly equal. But again: my misunderstandings could be solved by showing a plot of the H₂O column abundances.

Response:

Sorry for the misunderstanding. We meant that the excess/deficit of H₂O gas phase in MIPAS data are both larger (in absolute values) at latitudes closer to the pole; i.e., they increase from 70°N towards the pole.

We did not include extra figures but calculate the columns and included the values in the text. See also the response above.

The text has been revised along these lines.

• **Reviewer's comment:**

23. Section 7 (Diurnal variation of ice volume density, Figure 12) is lacking clarity and not convincing:

Response:

We have re-written the whole section and corrected several typos. We think that the section reads now more easily.

o P20 L26-28: as you write, there is an altitude difference between the morning/evening clouds, and you note that this altitude difference leads to the altitude bipole structure in Figure 12. Is it possible to correct for the altitude difference in order to get rid of the bipole structure?

The alternating negative-positive differences are actually indicative of a change in the mean cloud altitude with the NH pm clouds being on average at lower altitudes at 65-75N. Additionally, the shape of the average Vice is not the same during am and pm. Therefore, a simple vertical displacement of the am clouds would not make the bipole structure to disappear.

o P21 L1-2: These ice volume density differences are remarkably anti-correlated with the 10 am-10 pm differences in the kinetic temperature measured by MIPAS – I don't agree: there is a positive difference in T at 60-70S and 85-90 km, which should result in a negative

difference in the clouds in that region, but I see a dipole structure there (possibly only due to clouds being at different altitudes!). In the opposite hemisphere, I don't see any temperature differences, but a big positive signal in the ice volume density and also a (weaker) dipole structure.

We have now zoomed-in Fig. 12 in order to clarify the discussion. We have also refined the discussion. We write now that the good anti-correlation is only found in the NH. That is more clearly seen in the relative V_{ice} differences, that we now include in the manuscript. We also more clearly state that the SH ice differences are not well anti-correlated with temperature.

o P21 L3-4: The negative am-pm difference OF WHAT at 80-85 km at latitudes below (DO YOU MEAN EQUATORWARD?) 80°N is well anti-correlated to the am-pm ice differences OF WHAT. – Don't understand this sentence.

We re-structured and completed the sentence: 'The positive am-pm ice differences at 80-85 km equatorward of 80N are anti-correlated to negative am-pm temperature differences.

o P21 L4-5: In the NH temperature panel of Figure 12, I see temperature differences around 0K, are they even statistically significant? Also, shouldn't a positive temperature difference lead to a negative ice volume density difference, but the Vice NH plot shows a positive one? Don't understand this sentence.

As written in the previous version, there is a tendency of positive temperature differences that is reflected in negative ice differences. We think that it is more clearly seen in the new zoomed-in Fig. 12.

• **Reviewer's comment:**

24. P22 L3: I wouldn't call Figure 5 showing a "very good agreement" overall

Response:

Even if the new comparison (Fig. 5) is much better than before, we agree, we should not say "very good agreement" overall. Change to "good agreement".

• **Reviewer's comment:**

25. P22 L4: slightly larger – please quantify

Response:

With the new comparison the differences in IWC are small, ~10%.

Technical corrections:

We thank again to the Reviewer for all the technical corrections that resulted in an improved manuscript. They have been all included except a few exceptions as explained below. We also respond below to the questions raised in this Section.

2. You mostly use both terms PMCs and NLCs, while I think it would be more consistent to stick to one term throughout the paper.

We mainly used the term PMC. We only use "NLC" once in the introduction, to say that PMC and NLC are the same phenomena. To avoid misunderstanding, we have deleted 'as seen from the ground'. We use NLC in many cases when referring to the MIPAS "NLC-mode" measurements. This "NLC-mode" was defined in the MIPAS mission plan as a particular observation mode and hence we have kept its name.

P8 L6: Noise errors in these plots are about $0.3 \times 10^{-14} \text{ cm}^3/\text{cm}^3$. – How do you calculate this noise error?

By the standard error of the mean, i.e., by dividing the single noise error by the square root of the number of averaged profiles.

We state this now explicitly in all figure captions and in the text.

Please comment on the low latitude clouds detected outside regions colder than the frost point temperature: why there?

We include the following text: "Weak PMCs located at latitudes equatorwards of about 60 degrees and outside of the frost point temperature contour are likely false detections caused by instrumental (most likely offset) errors."

62. Figure 4: It seems you have forgotten to put the SH results as in Fig. 3. Also the ordering is wrong (the two NH plots should be below each other, not next to each other).

Correct. The caption was wrong. It has been corrected in the revised version.

Figure 5: why do you only show results from the MA and UA modes (MUA), and not the NLC mode?

Because we did not want to mix up measurements with different vertical resolution and those of MUA have a better statistics.

93. P13 L5: (except in 2011) - I don't agree: also in NH2011, the MIPAS ice mass density is higher than the SOFIE ice mass density above 85 km.

This has changed in the new figures (using only MIPAS pm data). The agreement is better now.

P13 L25: since, to our knowledge, the water ice content has not been measured at latitudes higher than ~ 75 . – I don't agree: AIM CIPS measures the IWC at latitudes higher than 75, see e.g., <http://lasp.colorado.edu/aim/browse-images.php>.

Thank you. The paragraph has been removed. Has this been reported in published papers?

P17 L29 – P19 L3: This text interrupts the discussion of Figure 11 and should be moved to the introduction (Section 1).

We agree that, as it is written, the H₂O retrieval description interrupts the discussion. However, it is too short and would also be isolated if moved to the "MIPAS measurements" section, which is mainly devoted to ice volume density retrieval. We have re-arranged the text in Section 6 so it does not interrupt the discussion that much now.