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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of Global Distributions of Polar Mesospheric Clouds during 2005–2012 by MIPAS/Envisat" *by* Maya García-Comas et al.

Anonymous Referee #1

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Overall recommendation: minor revisions

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.

Specific comments: individual scientific questions/issues 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, . . .? 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? 3. Do you have plans for making this dataset publicly available? 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. 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 Mice∼0.06 ngm3. - It is important to note that particle size is only determined when the extinction β(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. 6. P3 L30: It also operated with a high sensitivity – How do you define high sensitivity? 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? 8. Regarding all figures plotted vs. altitude: shouldn't the vertical axis be tangent altitude, not altitude? 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 background 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. 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. 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. 12. P11 L12-13: reformulate sentence, e.g.: Using the NLC mode data at similar NH latitudes, we derive a mean bottom altitude of $\sim$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 lati-

tude value, or a daily changing latitude value based on the changing SOFIE latitudes? Please describe your method better. 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)? 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. 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? 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. 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. 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. 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. 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". 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. 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. 23. Section 7 (Diurnal variation of ice volume density, Figure 12) is lacking clarity and not convincing: 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? 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. o P21 L3-4: The negative am-pm difference OF WHAT at 80-85 km at latitudes below (DO YOU MEAN EQUATORWARD?) 80âŮęN is well anti-correlated to the am-pm ice differences OF WHAT. – Don't understand this sentence. 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 on? Don't understand this sentence. 24. P22 L3: I wouldn't call Figure 5 showing a "very good agreement" overall 25. P22 L4: slightly larger – please quantify

technical corrections 1. Throughout the text, you mix up the present and the past tense, especially when it comes to reporting results from previous papers. I think that the paper would read better if the present tense was used consistently throughout the paper. Examples: P1, L6, P1 L14, P2 L18-19, P2 L22-35, P3 L90-94, P4 L11 – P5 L8, P5 L16-17, . . . 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. 3. P1 L1: we have analyzed MIPAS IR measurements . . . 4. P1 L3: coverage of the PMC total ice volume 5. P1 L13: caused by sequestration and sublimation 6. P1 L14: latitudes POLEWARD of 70°. Or do you only mean northern latitudes only? Then I would write latitudes poleward of 70°N 7. P1 L15: the PMC volume ice density 8. P1 L17-18: occur IN the coldest region of the atmosphere near the summer POLAR mesopause 9. P2 L7: (e.g., Baumgarten and Fiedler, 2008; 10. P2 L9: (e.g., Berger and Zahn, 2002 11. P2 L12: global climate change in the middle atmosphere 12. P2 L14: Since enhanced CO2 amounts (see, e.g., Yue et al., 2015) ARE EXPECTED TO lead to 13. P2 L14: and higher CH4 amounts to enhanced 14. P2 L15: they MAY both lead to 15. P2 L16: in the MIDDLE atmosphere (middle atmosphere is defined as the stratosphere and mesosphere) 16. P3 L4: A different technique, however, has been DEVELOPED by the AIM/SOFIE (Aeronomy of Ice in the Mesosphere/SolarOccultation for Ice Experiment) instrument. You should also name this different technique in this paragraph. 17. P3 L12-13: write out instrument abbreviations, at least MIPAS! 18. P3 L14: better spatial AND TEMPORAL coverage 19. P3 L16: In A previous paper (López-Puertas et al., 2009) 20. P3 L17: remove MIPAS complete name since you have to put it earlier 21. P3 L20: global distribution 22. P3 L20: of ice volume density 23. P3 L22: and ITS hemispheric dependence . . . Since you refer to the layer, not PMCs 24. P3 L22-23:

the correlation of ice volume density 25. P3 L23: and the water vapour concentration 26. P4 L6: during the summer seasons 27. P4 L10-11: described BY López-Puertas et al. (2009). 28. P5 L8: The more recent version 5 (5.02/5.06) of MIPAS L1b spectra has been used – What are the differences to the previous version? Is there a reference for the new version? 29. P5 L12-13: were retrieved only for scans with converged pressure-temperature profiles 30. P5 L18 –P6 L1: are described BY García-Comas et al. (2012). 31. P6 L7: and SHOW, in general, a remarkable agreement 32. P6 L7-8: is there a reference to this statement? 33. P6 L8-10: is there a reference for this statement? 34. P6 L11: Since MIPAS measures PMCs in IR emission 35. P6 L12: particularly ABOUT WHETHER they are warmer or colder 36. P6 L13-14: 5-20 K cooler than the ambient ATMOSPHERE 37. P6 L20: will be heated by absorption 38. P6 L22: warmer than the ambient gas BY about 1 K 39. P6 L26: affected by the problem pointed out by Petelina and Zasetsky (2009) – I think you should briefly describe that problem. If you don't want to do that, please write "affected by A problem pointed out by Petelina and Zasetsky (2009)" 40. P6 L27-28: If we assume that ice particles are cooler than the retrieved gas temperature we obtain 41. P6 L30: The vertical resolution of the ice density profiles 42. P6 L30-31: averaging kernel matrix, depends 43. P6 L32: For the middle and upper atmosphere modes (MA and UA, or together MUA) it is coarser 44. P6/P7: I would combine the information about the vertical resolution and the averaging kernels into one paragraph. 45. Figure 1 caption: Zonal mean ice volume density during four days, two in the SH and two in the NH, . . . 46. Figure 1 caption: is the mesopause as derived from MIPAS 47. Figure 1 caption: explain what #sc means in subplot titles. 48. Figure 2 caption: Polar maps of ice volume density . . . 49. Figure 2 caption: explain what D+N means. Is this information necessary, since it is the same in all four subplots? 50. P8 L5: Note that MIPAS IS sensitive 51. P8 L2 – P9 L5: If you don't comment on the +3K line in the text – why put it in the figure? 52. P8 L6: Noise errors in these plots are about $0.3 \times 10{-}14$ cm3/cm3. – How do you calculate this noise error? 53. P8 L6-7: The PMCs are generally located at regions colder than the frost point temperature for almost all conditions: - Please comment on

the low latitude clouds detected outside regions colder than the frost point temperature: why there? Measurement error, false detections? Due to transport? 54. Figure 1: would it be worth including another contour for 0.3 10-14 cm3/cm3, so one can see which detections may be false clouds? This may be interesting for clouds detected at low latitudes. Also true for many following plots. 55. P8 L10 - P9 L1: Emission from isolated clouds located in the LOS far away from the tangent point, and hence at higher altitudes, can be measured and thus attributed to these lower tangent heights (see . . .) – Replace with: Emission from isolated clouds located in the LOS far away from the tangent point is reported at abnormally low tangent altitudes (see . . .). 56. P9 L2: the FOV can affect the HEIGHT OF THE lower and upper boundaries 57. P9 L5: see bottom-left panel of Fig. 1 around 70âŮęN. – The bottom left panel shows PMCs in the SH. ? 58. P9 L9: they are sometimes far away – reformulate. 59. Figure 3 caption: Zonal mean ice volume density 60. Figure 3 caption: weighted with the ice volume density 61. Figure 3 caption: for the NLC (top panels) and the MUA (lower panels) modes (since you should have explained what MUA means in the text before this figure caption) 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). 63. Figure 4 caption: Zonal mean ice mass density 64. Figure 4 caption: weighted with the ice mass density 65. P10 L6: at slightly lower altitudes in the NH 66. P10 L10-11: because they were taken in the summer on different days, with those of the NLC mode closer to the peak of the PMCs season. Reformulate, e.g., because observations in different modes occurred on different days, with observations in the NLC mode generally occurring closer in time to the peak of the PMC season than observations in the other modes. 67. P10 L14: found in the SOFIE IR extinction measurements - SOFIE also measures scattered radiation at UV wavelengths! See Hervig et al., Interpretation of SOFIE PMC measurements, 2009. 68. P10 L15: measured by lidars - please also add reference for this statement. 69. P10 L15: Hervig et al. (2009b) have shown that 70. P10 L15-16: that the vertical smoothing DUE TO limb view geometry 71. P11 L1: are then more sensitive than lidars to clouds at higher altitudes. 72. P11 L8: and SOFIE measurements 73. P11 L10-11: at a latitude close to 70° - this is imprecise, especially since you further below state how important the correct latitude is for the bottom altitude. 74. P11 L11-12: SOFIE obtained a slightly lower altitude of 81.6 km, which is within their mutual standard deviations. – In this sentence you write just about SOFIE, so what does "their" and "mutual" refer to? 75. P11 L13-14: whereas in SOFIE those found below 79 km were excluded - reformulate, e.g., whereas SOFIE measurements that had an altitude of peak extinction below 79 km were excluded (Hervig et al., 2009b). 76. P11 L14-15: hence A DIFFERENCE OF a few degrees in latitude 77. P11 L15: might induce 78. P11 L15: in bottom altitude 79. P11 L20: Replace your title "Concentration" with "Ice mass density". I connect the word concentration more to number density, whereas you're writing about the MIPAS ice mass density. 80. P11 L26-27: These SOFIE measurements OCCURRED 81. P11 L27: at latitudes between 66âŮęN, in the early season, to 74âŮęN, towards the end of the season - this range does not include any information about the min/max SOFIE latitude at mid-summer. 82. P11 L29: Figure 4 is not introduced properly. 83. P11 L 32-33: that present an average value of 47.4 ngm^{-3}, and the lidar results reported by von Cossart et al. (1999), with ice mass ranging from 36 to 102 ngm^{-3} – hard to understand, please rewrite. 84. P11 L33 – P12 L1: MIPAS, as well as SOFIE, also measure thinner ice clouds than other IR instruments measuring the PMCs from space, e.g., HALOE (Hervig et al., 2003). – This sentence somehow interrupts the logical flow of comparing MIPAS/SOFIE to lidar, doesn't fit in. Where is this result from, could you refer to specific plots that you used to come up with this conclusion? 85. P12 L6: reported by Hervig et al. (2009b) 86. P12 L7: at least for the 65-75° latitude RANGE 87. P12 L9-10: for the coincident days and latitudes - state your coincidence criterion. 88. P13 L1: large variability of the ice mass density 89. P13 L3: the mean MIPAS ice mass density 90. Figure 5: why do you only show results from the MA and UA modes (MUA), and not the NLC mode? 91. Figure 5: Why do you only show NH data, not SH data? If you don't want to show, at least provide some statement about how they look like. 92. Figure 5 caption: The mean IWC values for both MIPAS and SOFIE are also

provided. (You have already defined IWC in the text, so you don't need to do it here again) 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. 94. P13 L5-6: This MIPAS data feature of large ice MASS densities at high altitudes can (remove commas) 95. P13 L6: Fig. 4, bottom right panel – Figure 4 has only one row of plots, there is no bottom right panel 96. P13 L5-9: The whole discussion of high MIPAS ice mass densities at different latitudes does not add to the discussion of Figure 5, which shows a comparison to SOFIE at the SOFIE latitudes. If your point is that MIPAS ice mass density is high compared to other observations at non-SOFIE latitudes, you have to compare to another instrument that observes at more latitudes than SOFIE. 97. P13 L9: In this region – replace with: At the altitudes of the largest high bias in MIPAS ice mass density 98. P13 L15: The mean IWC of the coincident observations, which is provided in Fig. 5, is generally larger . . . 99. P13 L17: in better agreement with model calculations than SOFIE (Hervig et al. (2009c, see their Fig. 5d). 100. P13 L22-23: As expected Figure 6 shows the same general behaviour as discussed above for the volume density (Figure 3). 101. P13 L23-24: I would rewrite this sentence a little: In NLC mode, which contains observations during the mid-season period, we note that the amount of water vapour in the form of ice ranges from 1 to 3 ppmv at latitudes equatorward of 70-75°, and reaches values up to 5-6 ppmv close to the poles. 102. P13 L25: since, to our knowledge, the water ice content has not been measured at latitudes higher than $\sim$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. 103. P13 L26: Hervig et al. (2015) have shown time series of SOFIE Qice at the altitude of peak extinction for the 2007-2013 period for the Northern and Southern hemispheres (their Figure 2). 104. P13 L29: shown in the left panel of Figure 6 – do you mean the left panels or the top/bottom left panel? 105. P13 L30: Section 6. 106. P14 L2: Please describe how you define the mean altitude of the PMC layer: e.g., do you use the ice volume density, ice mass density, or Qice? 107. Figure 7: replace "mesopause" and "PMC mean altitude" with zmeso and zPMC, respectively. 108. Figure 7: consider using a second y-axis on the right side that shows zmeso – zpmc without the 79km offset. 109. Figure 7 caption: The different colors indicate the results for the NLC (black) and MUA (red) MIPAS observation modes (see Table 2). 110. Figure 8: in the subplot titles, remove "Ice water content" since that information is already visible in y-axis annotation. Replace by IWC in y-axis labels and caption. 111. P15 L2: The MIPAS mean valueS obtained here – what are they? 112. P15 L7: is significantly smaller, in $\sim$1 km - What do you mean here? Did you mean to write: is significantly smaller, by $\sim$1 km – which would mean that the SOFIE mean PMC height is 2.5 $\pm$0.5 km below the mesopause, OR is significantly smaller, only $\sim$1km, - which means that the SOFIE mean PMC height is 1 km below the mesopause. 113. P15 L7 - P16 L1: Please add a citation for this statement. 114. Figure 9 caption: Remove "ice water content", use only IWC 115. Figure 9 caption: describe black line and correlation coefficient 116. Figure 9 caption: Zonal mean ice volume density (left) and H2O concentration anomaly . . . 117. Figure 9: if you don't discuss the +3K frost point temperature in the text, why put it into the plots? 118. Figure 9 caption: the mesopause as derived from MIPAS 119. P16 L4: obtained by Russell et al. (2010) 120. P17 L6-7: I would rewrite this sentence as: Figure 8 is consistent with the zonal mean ice volume density shown in Figure 4, which shows that ice mass density increases towards the poles. 121. P17 L7: The large variability is also clearly visible – I would rewrite this as: Large variability from season to season is also clearly visible 122. P17 L11: The NH/SH ratio of the ice water content varies with latitude (not shown) 123. P17 L17: The correlation is significant and shows that the PMC layers are denser and wider when the frost point temperature occurs at lower altitudes. – This sentence needs to be rewritten: how can the PMC layer be denser and wider at the same time? This sounds like a contradiction to me, what exactly do you mean? And why do you talk about (several) PMC layers – do you mean multiple layers in altitude? 124. P17 L23: the SOFIE vertical resolution is well suited 125. P17 L29 – P19 L3: This text interrupts the discussion of Figure 11 and should be moved to the introduction (Section 1). 126. Figure 11: Remove "D+N" from subplot titles 127. Figure 11 caption: note the different scales 128. Figure 11 caption:

explain what white symbols are 129. Figure 12: It would be useful to use the same x and y axis ranges for all four panels since that makes comparison easier. 130. P19 L4: polar region: the region . . . 131. P20 L1-2: by Hervig et al. (2015) using SOFIE observations 132. P20 L4: observed bottom of the ice layer 133. P20 L8-9: could you add a sentence about how the column abundance of gas-phase H2O in the dehydration layer and the hydration layer is defined? 134. P20 L15: Diurnal variation of ice volume density 135. P20 L16: The diurnal variation of PMCs 136. P20 L16: when comparing different PMC datasets. 137. P20 L17: equatorward 138. P20 L18: in temperature and meridional advection 139. P20 L24: but not as large 140. P20 L25: 36% in the IWC (not shown). 141. P20 L25-26: Note however that the IWC am-pm differences are also influenced by the slightly negative volume density difference at altitudes above 86 km – Could you explain better how/where from you get the "slightly negative volume density difference at altitudes above 86 km", e.g. point to a plot that shows this? 142. P20 L29-30: That corresponds . . . That indicates . . . - rewrite more elegantly. 143. P20 L30-32: a significantly narrower and thinner pm cloud which mainly disappears below 84 km, in agreement with findings at sub-polar latitudes from Stevens et al. (2010) and Gerding et al. (2013). 144. P21 L4: northward of 65°N. 145. P21 L18-19: We have analyzed MIPAS IR measurements for the NH and SH summer seasons from 2005 to 2012. 146. P21 L21: It is therefore sensitive – What is sensitive? It sounds like scattering is sensitive to the total ice volume. 147. P21 L21: very small ice particles that UV-VIS scattering observations are generally not sensitive to. 148. P21 L23: The measurements cover only a few days of the PMC season (varying from 3 to 15) but, as opposed to SOFIE, have global polar coverage. 149. P21 L30: from lidar measurements 150. P21 L31: particles in the upper part 151. P22 L1: PMCs are very variable 152. P22 L2: concentration – do you mean ice mass density? 153. P22 L2: water ice content – replace by IWC 154. P22 L4-5: smaller sensitivity – do you mean that MIPAS is less sensitive than SOFIE? Do you mean that the "smaller sensitivity" cause slightly larger MIPAS IWC, or a larger MIPAS variability? Please clarify. 155. P22 L5-6: larger values – of what? 156. P22 L15: It is not clear what "their" refers to - please rewrite more clearly. 157. P22 L15: since the NLC-mode measurements 158. P22 L16: while the MUA-mode observations are 159. P22 L17: as we move away from the polar region – replace with “towards more equatorward latitudes” 160. P22 L18: equatorward 161. P22 L20: from several satellite instruments 162. P22 L25: denser and wider – what do you mean? 163. P22 L27: of MIPAS PMCs and water vapour have shown 164. P22 L27: have confirmed that PMC layers 165. P22 L32: has shown

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