Response to Referees' Comments on egusphere-2025-2330, "Altitude-Dependent Formation of Polar Mesospheric Clouds: Charged Nucleation and In Situ Ice Growth on Zonal and Daily Scales"

We sincerely appreciate the time and effort the two Referees have taken to review our work. Their comments are valuable and constructive, and help us to improve the quality of our manuscript. The following are the point-by-point responses.

Response to Referee #1

Dear authors, thank you very much for presenting your work on "Altitude-Dependent Formation of Polar Mesospheric Clouds: Charged Nucleation and In Situ Ice Growth on Zonal and Daily Scales". The proposed new mechanism of a charged meteoric smoke particle nucleation scheme to explain the characteristics of noctilucent clouds is very interesting and complements standard microphysical models. I only have minor suggestions for the text that hopefully increase the readability for the reader and makes your argument easier to follow.

We sincerely thank the reviewer for the positive feedback and thoughtful summary of our work. We are especially grateful for the recognition of the novelty and complementary value of the proposed CMN scheme. We have carefully considered all the suggestions aimed at improving the clarity and readability of the manuscript.

A lot of acronyms such as GS or PMC are introduced in the abstract only. Please consider to reintroduce them in the body of the text again, i.e., when they are first mentioned somewhere else than the abstract.

We thank the reviewer for this helpful suggestion. We have now reintroduced all key acronyms at their first occurrence in the main text.

Moreover, you introduce the CMN scheme in this manuscript, that is in contrast to the conventional GS (growth-sedimentation) scheme. Later in the discussion, you mention the freeze drying effect that, if I understand it correctly, is used nearly synonymous to the GS scheme. Additionally, the cold trap effect is used as a synonym for the GMN scheme. This might be confusing to the reader so please consider to stick to one name per mechanism if it is possible.

We sincerely thank the reviewer for highlighting this potential source of confusion. We agree that clearer terminological distinction is necessary. In the section 4.3 of revised manuscript, we have clarified the relationship between the PMC formation schemes and their associated H_2O redistribution processes: the freeze-drying effect is clearly identified as the H_2O redistribution mechanism resulting from the GS scheme, and the cold-trap effect is explicitly introduced as the H_2O redistribution mechanism resulting from the CMN scheme.

Here are some more technical remarks:
l. 9: please provide the full name of AIM
l.73: the "r" is missing in "wate content"

Done. Thanks.

Figure 7 - 9: you are using a significance criteria of ±0.25 for your correlation coefficients. Could you shortly mention why you chose this threshold? Could you mention your significance criteria in the text?

We thank the reviewer for this question. The threshold of $|r| > 0.25$ corresponds to the 95% confidence level ($p < 0.05$) for a two-tailed test given the sample size of $n = 61$ daily data points per PMC season. This critical value has now been explicitly stated and justified in the main text of the revised manuscript

Thank you very much for preparing the manuscript and presenting as well as discussing your findings so clearly.

We are truly encouraged by the reviewee's generous feedback and grateful for their recognition of our efforts to present and discuss the findings clearly.

Response to Referee #2

GENERAL COMMENTS

This paper proposes an approach to characterizing polar mesospheric cloud (PMC) growth and evolution that differs from the conventional approach. The authors suggest that the behavior of key microphysical parameters such as column-averaged ice particle concentration and particle radius are governed by PMC height, rather than by the background temperature. They analyze a portion of the data record from the SOFIE and CIPS instruments, flown on the AIM satellite, to develop the basis for their approach. This approach also relies on the presence of small charged meteoritic smoke particles to initiate nucleation of ice particles.

We sincerely thank the reviewer for the accurate summary of our manuscript's scope and key findings.

A significant concern with the approach presented in this paper is the assumption that the latitude of SOFIE occultation measurements remains constant throughout a PMC season. This is not correct, and since various microphysical parameters utilized by the authors do have a latitude dependence, there may be biases or errors in season-long calculated averages of SOFIE data that do not consider this variation.

We are grateful to the reviewer for raising this critical point regarding the potential influence of SOFIE's latitude variation on our analysis. We fully agree that accounting for latitude dependence is essential in interpreting PMC observations.

In response to this concern, we have thoroughly evaluated the impact of observational latitude drift and applied several safeguards to ensure the robustness of our results:

1. Limited Latitude Drift in Core PMC Season. The latitude variation during our defined core PMC season (–10 to +50 days from solstice) is approximately 9° or less. To quantitatively assess potential bias, we performed a sensitivity test by narrowing the seasonal window to –10 to +30 days, thereby reducing the latitude range to ~4° (as shown later in Figure R1). The key relationships reported in Figs. 5-15 remained statistically unchanged, confirming that our central findings are not an artifact of latitudinal drift.

2. Data Processing Mitigates Systematic Bias: Our use of a 35-day running mean to isolate daily-scale variability inherently removes longer-term trends, including those potentially introduced by the satellite's gradual latitudinal shift. This processing step effectively minimizes any systematic bias from the sampling drift.
3. Negligible Latitudinal Influence in Monthly Comparisons: For the monthly comparisons in Figs. 3-4, the difference in monthly-mean sampling latitude between the months (e.g., December vs. January in the SH: ~3.1°; June vs. July in the NH: ~2.0°) is small (see Fig. R1). The pronounced contrasts in temperature, H_2O, and PMC properties between these months are consistent with known intra-seasonal atmospheric dynamics and are far larger than the variability expected from such minor latitude differences.

Therefore, we conclude that the latitudinal drift of SOFIE does not significantly affect the key relationships reported in this study.

SPECIFIC COMMENTS

Page 2, line 37: The term "inconclusive" is an overstatement regarding long-term trends. The Kirkwood et al. (2008) study only addresses ground-based noctilucent cloud observations from selected Northern Hemisphere stations. The DeLand and Thomas (2019) study uses satellite data to show statistically significant increasing trends at multiple latitude bands in both NH and SH ice water content during 1978-1997, and significant increasing trends in the NH for the period 1998-2018 as well.

We thank the reviewer for this correction and for providing a more precise interpretation of the cited literature. We agree that the describing the evidence for long-term trends as "inconclusive" was an overstatement, and therefore removed this term.

As the reviewer points out, *DeLand and Thomas* (2019) provides clear evidence for statistically significant increases in PMC, particularly in the NH. The revised text now accurately reflects these findings. We have also removed the reference (Kirkwood *et al.*, 2008) of ground-based observations at mid-latitudes.

Page 2, lines 57-59: Vellalassery et al. (2023) presents recent 3-D model results that also support the freeze-drying approach.

Thanks. The work by *Vellalassery et al.* (2023) and their LIMA-MIMAS model results, which provide further support for freeze-drying effect, have been cited in the revised manuscript.

Page 3, lines 66-68: Note that the AIM satellite re-entered the atmosphere in August 2024.

Thanks. The text has been updated to reflect that the AIM satellite has concluded its mission and re-entered the atmosphere in August 2024.

Page 3, line 71: The SOFIE observation latitude is not constant during the PMC season. Figure 1(b) of Hervig et al. (2009a) shows that for the NH 2007 season, the sampling location varies from ~68°N at DFS (days from solstice) = −10 down to ~66°N at DFS = 0, then up to ~72°N by DFS = +50. This variation means that the latitude dependence in key PMC parameters should not be ignored when seasonal averages are created.

We thank the reviewer for raising this important point regarding the variations in SOFIE's observational latitude during the PMC season. We fully agree that this is a critical factor that must be considered in the analysis.

In section 2, we have added a detailed description for the intra-seasonal latitudinal drift of SOFIE/AIM.

In sections 3.3 and 3.4, we have assessed the potential influence of intra-seasonal latitudinal drift on our results by performing sensitivity analyses, and confirmed that the reported relationships in Figs. 3-7 are not significantly affected by the latitudinal variations of SOFIE data.

Page 3, line 72: Why are no SOFIE data after 2014 considered? While orbit drift of the AIM satellite does have a more significant impact on SOFIE sampling in later years, extending coverage to 2016-2017 would provide continuity with the choice of CIPS data record coverage.

We thank the reviewer for this question regarding the SOFIE data after 2014.

The orbit of the AIM satellite drifted over time. The latitude coverage of SOFIE data has shifted to ~55°N during 2015 and ~52°S during 2014-2015, where the PMC frequency is very low and not suitable for the statistical analysis in this paper. The SOFIE instrument did not observe usable PMC data in 2016 and 2017. As a result, the SOFIE data before 2014 were applied.

The latitude coverages of CIPS data are not affected by satellite orbit shift, and available until 2017.

Page 3, line 79: The PMC height H (calculated by averaging Z_{bot} and Z_{top}) may be approximately equal to the Z_{max} value reported by SOFIE, but the latter term should be a more accurate representation of the largest portion of cloud particles.

Our choice to use the mean PMC height *h* was based on the specific focus of our study, which is to understand the variability of the cloud layer boundaries as a whole and their relationship with environmental drivers.

As shown in Table 1, the averaged PMC height *h* is approximately equal to Z_{max}. More importantly, we conducted a sensitivity analysis and confirmed that the results and conclusion remain unchanged if Z_{max} is used instead of *h*. This demonstrates that the observed physical relationships are not sensitive to the specific choice of PMC altitude metric.

Page 3, lines 80-81: Simple averaging of ice concentration at all altitudes between Z_{bot} and Z_{top} is not necessarily appropriate. Figure 3(e) of Hervig et al (2009a) shows that the altitude dependence of the concentration throughout the NH 2007 season is closer to exponential, with values of ~20 cm^{-3} near Z_{bot}, increasing to ~500-1000 cm^{-3} near Z_{max}, with no useful data for 1-2 km below Z_{top}.

We appreciate the reviewer's comment regarding the vertical profile of ice concentration and its influence on column averages.

It should be clarified that the exponential scaling in the Fig. 3(e) of *Hervig et al.* (2009) is used primarily to visualized the much lower values of ice concentration at altitudes below PMC bottom.

In fact, the profile of ice concentration across the PMC layer, as shown in the Fig. 4(d) of *Hervig et al.* (2009), exhibits an approximately linear increase with altitude.

In addition, the profiles of ice concentration in our Figs. 3-4 also vary linearly rather than exponentially with altitude.

Therefore, a simple average across the PMC layer is an appropriate method for deriving a column-mean value for microphysical parameters within the cloud.

Page 3, lines 81-83: It would be helpful to see plots of the inter-season variation in Z_{max} or H. The SOFIE sampling latitude drifts Equatorward in both hemispheres by 2014, particularly in the SH (Hervig et al. (2016), Figures 5(b) and 10(a)). This will impact the sampling of latitude-dependent quantities.

We thank the reviewer for this valuable suggestion.

To address the concern regarding inter-seasonal sampling drift, we have plotted a new figure (provided below as Figure R1) showing the inter-annual variations of PMC height Z_{max} (5-day smoothed) together with the corresponding SOFIE observational latitude coverage from 2007 to 2014.

For the SH, the intra-season variability of Z_{max} driven by atmospheric dynamics (e.g., gravity or planetary waves) is highly pronounced, making it difficult to identify a clear trend that could be attributed to the latitude drift of SOFIE data over the 7-year SOFIE.

For the NH, both the inter-season and intra-season variations in Z_{max} are comparatively weaker. More importantly, no significant influence from the modest latitude drift is discernible.

We fully agree that sampling latitude is an important factor in PMC climatology. However, the primary focus of this study is to understand intra-seasonal PMC variability. The interannual PMC variability and the long-term orbital drift, while acknowledged, are beyond the scope of this investigation.

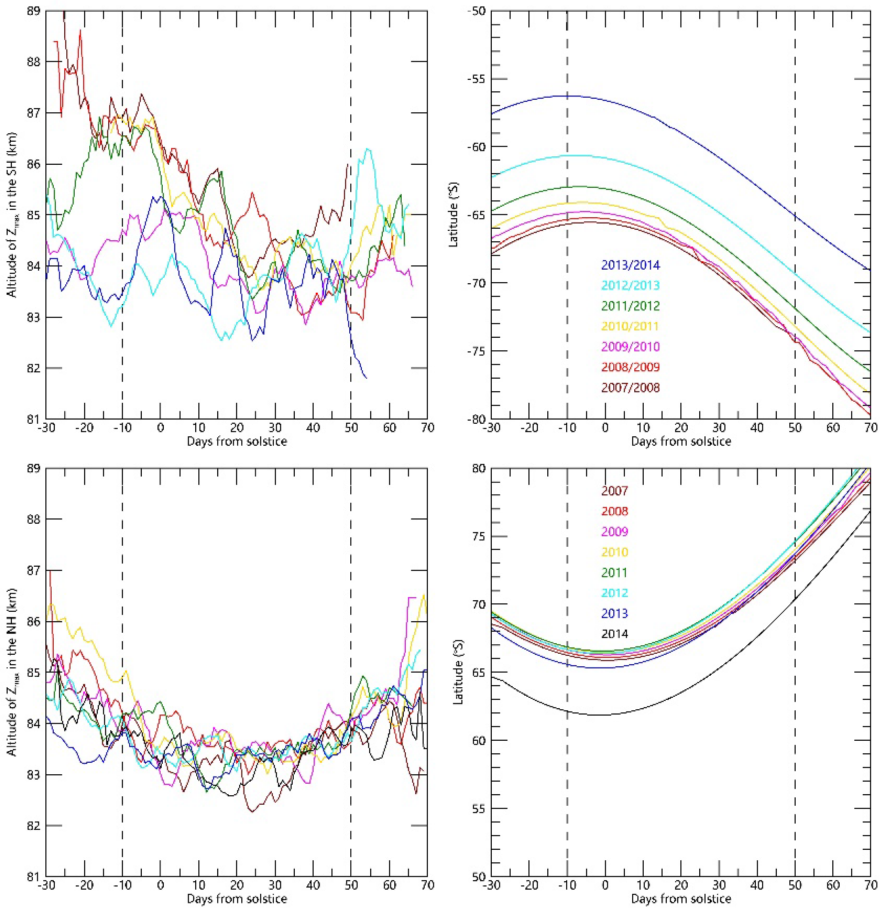

Figure R1. (Top) The variations of Z_{max} for PMC seasons from 2007/2008 to 2013/2014 in the SH, and the equatorward shift of SOFIE latitude coverage. The Z_{max} has been 5-day smoothed. (Bottom) The variations of Z_{max} and the SOFIE latitude coverage in the NH.

Page 4, lines 92-93: It is difficult to believe that the average of 10 season-long zonal averages of PMC properties such as IWC and radius can frequently have a standard deviation that is less than 2% of the original quantity. It would be helpful to show the yearly values of IWC for a few latitude bands for comparison with other published papers that show such time series.

We sincerely thank the reviewer for the meticulous reading, which allowed us to identify and correct a significant error in our original description. The values originally described as "standard deviation" were in fact the "standard error of the mean".

The text has been corrected accordingly. The standard deviation of the interannual variability is indeed larger than the displayed error bars by a factor of sqrt(10).

The mean IWC values for each latitude band, averaged over the 10 PMC seasons, are displayed in the figure.

Page 4, lines 101-103: Decreasing PMC altitudes in the SH during the core of the season have also been shown by Bailey et al. (2005) using SNOE data, and by DeLand and Gorkavyi (2020) using OMPS LP data.

We thank the reviewer for pointing out these highly relevant and important references. The findings of (Bailey *et al.*, 2005) and (DeLand and Gorkavyi, 2021), which document the characteristic seasonal descent of PMC heights in the SH using independent satellite datasets (SNOE and OMPS LP respectively), provide valuable validation for our SOFIE observations. These citations have been added to the revised manuscript.

Page 4, lines 110-118: Why do you disregard the effect of significant cooling between December and January in the SH as a mechanism for changes? You say that profiles are stable, but show a decrease in concentration and an increase in radius. The latter effect (at the bottom of the profile) is consistent with larger particles sedimenting and sublimating (consistent with H_2O changes at 83 km and below).

We thank the reviewer for this insightful comment, which allow us to clarify the role of temperature in our interpretation.

We fully agree that the substantial cooling between December and January is the fundamental driver of the changes observed in Fig. 3. As discussed in section 4.4, we propose that the influence of temperature operates through two interconnected processes:

1. Cloud Boundary Adjustment: The lower temperatures (result from the adiabatic cooling of upwelling) in January expand the supersaturated zone, depressing the cloud base. Simultaneously, enhanced dehydration via cold-trap effect lowers the cloud top, collectively reducing the overall PMC height *h*.
2. Microphysical Response to Altitude Change: Due to the relative stable vertical profiles of ice particle concentration and size, the column-mean value of N_c and r_c are highly sensitive to the altitude of the cloud boundaries. In January, the cloud height is lower, resulting in the observed decrease in N_c and increase in r_c.

In addition, as noted by the reviewer, the radius profiles diverge below ~83 km, with January showing larger particles. This could be explained by the abundant H_2O transported upward by upwellings in January, which enhance the growth of ice particles at PMC bottom.

Page 5, line 129: Why is the duration of the PMC season for the CIPS averages different than the PMC season defined for the SOFIE analysis?

We thank the reviewer for identifying this inconsistency in analysis period definitions.

To ensure a direct and consistent comparison between the CIPS and SOFIE dataset, we have refined the core PMC season for the CIPS analysis to match that used for SOFIE, now spanning from -10 to 50 days relative to solstice for both instruments.

The results presented in the revised Figure 1, which uses the (-10, 50) time span, are consistent with previous results of the (0, 40) period of PMC season. This demonstrates that the key findings are not sensitive to this slight seasonal adjustment.

Page 9, lines 146-148: The correlation analysis used here uses "anomaly" data from which a 35-day running mean has been subtracted for each season. This step incorrectly removes true variations in Z_{max} during a season (see Hervig et al. (2009a), Bailey et al. (2005)). NOTE: This information is only presented in the caption for Figures 5-6. This is an important feature of the data analysis that should be stated (and justified) in the text as well.

We thank the reviewer for emphasizing the importance of clearly describing our data processing methodology. The use of a 35-day running mean was applied to remove the long-term intra-seasonal disturbances driven by gravity or planetary waves. This method also effectively diminishes potential biases from the gradual equatorward drift of SOFIE's sampling latitude during the season.

Furthermore, a sensitivity test confirms that the core correlations presented in Figs. 5-6 remain robust and are not affected by the removal of the running mean, underscoring the stability of our findings.

We acknowledge that this information was initially only in the figure captions, and we have now added a comprehensive description of this processing step in the revised manuscript.

For completeness, all relevant figure captions have explicitly stated that the data were processed with the 35-day running mean.

Page 9, lines 148-149: Positive correlations < 0.3 do not seem to be very strong.

We agree with the reviewer that correlation coefficients below |0.3| typically indicate a weak linear relationship. However, for our effective sample size of n = 61 daily data points, a correlation of $|r| > 0.25$ is statistically significant at the 95% confidence level.

Page 9, lines 156-161: Extending the region for averaging T_{env} down to 78 km includes a significant altitude region that does not impact PMC microphysical properties, because PMCs are not observed at such warm temperatures (see average Z_{bot} in Figures 3-4). Why not limit the lowest altitude of the T_{env} calculation to 81 or 82 km to be more representative of only the PMC region?

We agree that the altitude range of T_{env} should not down to 78 km, and the lowest altitude is set at 80 km. Figs. 8&9 were replot for the altitude range from 80 km to 88 km, and the results and correlations remain virtually unchanged.

Page 9, lines 161-163: It is easy to understand that reducing T_{env} will lower Z_{bot}, since that level is defined by the existence of PMCs. It is not as obvious that Z_{top} will be raised by a corresponding amount to maintain a constant value of H. You have already shown in Figure 2 that H has a clear decrease during the PMC season in the SH, and temperatures at PMC formation altitudes are also decreasing (Figure 3). It seems simpler to assume that the lower temperature enables PMC formation at lower altitudes.

We thank the reviewer for this insightful comment, which allow us to clarify the complexity in PMC boundary variability.

1. Our analysis in Figure 10 reveals that PMC layer boundaries respond independently to temperature changes at different altitudes: Z_{bot} is positively correlated with temperatures near 80 km, while Z_{top} is negatively correlated with temperatures near 86 km. This decoupling indicates that the mean PMC height *h* does not respond to a bulk temperature change but instead sensitive to the vertical structure of cooling.
   The daily/zonal mean T_{env} is an average derived from ~15 SOFIE orbital profiles per day. A decrease in this mean value can arise from various scenarios: some orbital temperature profiles may exhibit stronger cooling near 86 km (which would act to raise Z_{top}), while others show stronger cooling near 80 km (which would lower Z_{bot}). The net effect is that the daily-mean PMC height *h* remains largely insensitive to the daily-mean T_{env}, as the opposing boundary adjustments compensate for one another.
2. The observed seasonal descent of both Z_{top} and Z_{bot} in the SH (Fig. 2) represents a specific case where dynamical and microphysical processes override these simple thermal responses. We attribute this coordinated descent to the cold-trap effect (section 4.4): intensified upwelling simultaneously 1) cools the entire layer (contributing to Z_{bot} descent), 2) enhances ice growth that consumes available H_2O, and 3) dehydrates the upper atmosphere, thereby depressing Z_{top}. In this framework, the descent of Z_{top} in Fig. 2 is primarily driven by H_2O depletion (a microphysical consequence of upwelling) rather than by the direct thermal effect at PMC top altitudes (the cooling at PMC top would raise Z_{top}).

Crucially, the apparent role of temperature depends on the timescale of analysis: 1) When the cold-trap effect dominants (e.g., seasonal trend in Fig. 2 where the 35-running mean is not removed), intensified upwelling drives coordinated cooling and dehydration, resulting in a lower PMC height *h*. In this scenario, both PMC boundaries are positively correlated to environment temperatures. 2) When high-frequency variability is isolated (e.g., daily anomalies in Figs. 8-10 after removing the 35-day running mean), the compensating thermal adjustments at different altitudes dominate, decoupling PMC height *h* from T_{env}.

Page 9, line 165: Larger particles and higher concentration (more nucleation) do not necessarily occur simultaneously in the same altitude region.

We agree with the reviewer's clarification that the processes of nucleation (governing concentration) and growth (governing size) are often vertically separated and do not necessarily occur in the same altitude region.

In fact, our analysis is specifically focused on the column-integrated microphysical properties (column-averaged concentration N_c and radius r_c), which represent the bulk characteristics of the entire cloud layer.

Page 16, lines 211-213: It is not obvious how there can be “negligible sedimentation” without requiring increasing vertical winds to maintain a constant particle altitude. As the ice particles grow, gravitational effects become more important. If the PMC reaches local equilibrium, what mechanism then dissipates a stable PMC?

We thank the reviewer for this critical question regarding the role of sedimentation and PMC dissipation.

We agree that vertical winds are necessary to balance gravitational settling if individual particles were to remain at a constant altitude for an extended period. The CMN scheme does not require ice particles to be maintained at a fixed altitude indefinitely. Instead, it describes a statistical equilibrium on daily and zonal scales, where the continuous in-situ nucleation and growth of ice particles in a sustained supersaturated environment creates a stable cloud layer. The ~2-day lifetime of ice particles in PMC is consistent with the timescales over which this equilibrium is maintained before the large-scale environment changes.

Regarding dissipation: In the GS scheme, a cloud dissipates over ~2 days primarily through the natural lifecycle of sedimenting particles. In the CMN scheme, dissipation occurs more rapidly when atmospheric waves or other disturbances disrupt the local thermodynamic equilibrium, causing temperatures to rise above the saturation threshold and leading to instantaneous sublimation throughout the cloud layer.

Page 17, lines 247-248: SH PMC altitudes do not show a latitude dependence in multiple satellite data sets: Bailey et al. (2005) using SNOE data (Figure 7), DeLand and Gorkavyi (2020) using OMPS LP data (Figure 11).

We sincerely thank the reviewer for highlighting these important references, which reported that PMC altitudes are latitude independent. Indeed, these limb measurements do not support our assumption that the PMC altitudes are higher at lower latitudes in the SH.

However, upon re-examining the limb measurement techniques used in these studies, the “false” PMC events may obscure a true latitudinal trend:

As discussed in *DeLand and Gorkavyi* (2021), limb sounders can detect “false” PMC signals in the near- or far-field of the tangent point, often at altitudes below 80 km (see Figs. 4&5 therein). While a filter is applied to remove events below 80 km, some false detections above 80 km (but lower than cloud base) likely remain.

Critically, the probability of such false PMC events is latitudinally dependent. At high latitudes (80°–85°S), where PMC occurrence frequency exceeds 90%, a detected off-tangent signal is very likely a "true" PMC from another part of the atmosphere. At lower latitudes (65°–70°S), where occurrence frequency is only ~38%, a similar off-tangent signal has a much higher probability of being a "false" detection from a lower altitude. This systematic bias would artificially depress the measured average PMC altitude, leading to lower PMC heights at lower latitudes.

The false PMC signals may mask an underlying decrease in PMC altitude with increasing latitude; therefore, the constant altitudes reported by limb sounders may represent a convergence between a true negative trend and an opposing observational bias.

We emphasize that above analysis is not intended to challenge the main conclusions of these valuable studies, but rather to highlight that potential retrieval artifacts in limb sounders might complicate the detection of a subtle latitudinal trend in PMC heights, though this remains speculative and requires further investigation.

Page 17, lines 255-259: The CMN scheme does not seem to be required to explain this case. The large influx of H_2O from altitudes above the PMC formation zone enables initial formation and rapid growth at these higher levels. Are charged MSPs really necessary as well?

Thanks. In response to the reviewer's comment, we have removed Section 4.2.3 regarding the rocket exhaust case, as we agree that it could also be explained by the GS framework. Namely, the excessive H_2O enables ice particles to rapidly grow at PMC top, and the larger ice particles may be observed at PMC top if the sedimentation rate is lower than the growth rate.

The purpose of including this case in our original manuscript was to present observational evidence consistent with the in-situ growth hypothesis in the CMN scheme, by showing that larger particles can exist at higher altitudes.

ADDITIONAL REFERENCES

Bailey, S. M., Merkel, A. W., Thomas, G. E., and Carstens, J. N. (2005). Observations of polar mesospheric clouds by the Student Nitric Oxide Explorer. *J. Geophys. Res.*, *110*, D13203, https://doi.org/10.1029/2004JD005422

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We thank the referee for providing these valuable references.

TYPOGRAPHICAL ERRORS

Page 3, line 73: "wate" should be "water".

Page 6, line 133: "could" should be "cloud".

Page 7, Figure 3: "Temperautre" should be "Temperature".

Page 16, line 203: "statistic" should be "statistical".

Page 18, line 262: "grower lager" should be "grow larger".

Thanks. All these errors have been corrected.

References

Bailey, S. M., A. W. Merkel, G. E. Thomas, and J. N. Carstens (2005), Observations of polar mesospheric clouds by the Student Nitric Oxide Explorer, *Journal of Geophysical Research: Atmospheres*, *110*(D13), doi:https://doi.org/10.1029/2004JD005422.

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Kirkwood, S., P. Dalin, and A. Réchou (2008), Noctilucent clouds observed from the UK and Denmark-Trends and variations over 43 years, *Annales Geophysicae*, *26*(5), 1243-1254, doi:https://doi.org/10.5194/angeo-26-1243-2008.

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