Review of “Dominant role of charged meteoric smoke particles in the polar mesospheric clouds” by Zhang et al.

This article investigates the dependence of PMCs on various environmental parameters, using observations from the SOFIE and CIPS satellite instruments. The article is generally well written, but the scientific analysis is not rigorous and suffers from major flaws. As a result, the conclusions are unsupported and misleading, and I recommend that this paper is rejected.

The first major flaw is the unsupported conclusion that charged meteoric smoke particles (MSP) explain the observed variability in PMCs (even evoking this in the title). In fact, the first sentence of the conclusion states that “This study demonstrates that the charged-MSPs nucleation scheme can explain a number of puzzles in the growth-sedimentation scheme and is likely to be responsible for the formation of PMCs”. This connection is made without any rigorous analysis, but instead is supported with loose associations and conjecture. Making such a bold statement requires clear evidence from the observations, or clear indications from a theoretical approach. The paper offers neither. The idea of charged MSP as PMC nuclei has been examine by previous authors (referenced in this paper), who performed rigorous model experiments and considered observations. Advancing upon these studies will require much more than the Authors have performed for this paper. The Authors furthermore ignored the SOFIE observations of the MSP content in PMC ice particles, which are reported vs. height. These unique measurements are surely relevant to an investigation of how MSPs affect PMCs, and should be considered here.

The second major flaw is the conclusion that the primary influence on PMC characteristics is altitude. This is somewhat ridiculous, as altitude is simply a coordinate, and not a forcing variable. This idea ignores the fact that PMCs are influenced by a variety of environmental parameters including temperature, water vapor, pressure, vertical wind, MSP, etc…, all of which vary in height, and act together to influence PMCs. It would be irresponsible and damaging to publish such simplistic ideas in the scientific literature.

The third major flaw is that the Authors do not appear to understand the current benchmark PMC models (e.g., WACCM-CARMA, LIMA-MIMAS), although most of these are discussed by papers in the reference list. They fail to recognize that existing models do a very good job of explaining the horizontal, vertical, and time dependence observed in PMCs [e.g., Bardeen et al., 2010; Kuilman et al., 2017; Wilms et al., 2016, and others]. Because the current state of model-observation agreement was not examined, it is difficult to understand the magnitude of the problem at hand (i.e., the effect of nucleation on PMCs).

Despite these flaws, the subject matter is of interest and the data used is of high quality, and I believe this investigation could represent a useful contribution after a major effort and restructuring.

Specific Comments

Placing all of the figures are all at the end of the paper makes the review unnecessarily tedious. It is now common to locate the figures inline, after the associated text.

Line 29: There is a more recent reference on space traffic effects on PMC [Stevens et al., 2022].

Line 41: The debate is not really "intense"

Line 43: It is not fair to state that nucleation is the most significant uncertainty. Rather, there are several references in this paper which suggest that nucleation is not important when describing PMC variability [Megner et al., 2011; Hervig et al., 2009c]. Even more curious is that Wilms et al. [2016] performed detailed model studies and conclude that "low MSP number density (or low nucleation rate per particle) is not a hindrance of NLC development; it is rather a prerequisite." In any case, the present study needs to do a much better job of documenting the current state of our understanding.

Line 59: State the typical PMC altitudes here.

Line 71: "sublimate"

Line 85: This statement is not supported by observations, as there are no global or routine observations of MSP size.

Line 91: A better reference here would be Baumgarten et al. [2012] (there may be a more recent paper, please check).

Line 112: On the SOFIE webpage, it looks like SOFIE reports the amount of MSPs contained in PMC particles. This quantity would be highly relevant to your study, and you should at least take a look at the measurements. Also, all of the SOFIE PMC retrievals are reported vs. height. You should mention these details here.

Line 115: On the SOFIE web page it looks like there are NH PMC observations also in 2015 and 2020-2022.

Line 117: Delete "rectangular"

Line 119: On the CIPS webpage, CIPS PMC data are available through 2022 in the NH and through 2023 in the SH.

Line 129: The section title "Effects of Altitude" is inappropriate, since altitude itself does not modulate PMC properties but rather is simply a coordinate.

Figures 2 - 5: What altitude are these results for? Is it for Zmax or something else?

Lines 135-140: These statements are not supported by the present results in any way, and in fact are somewhat nonsensical. Please see the model-SOFIE comparisons in Bardeen et al. [2010], which indicate that WACCM-CARMA simulations are in very good agreement with the observed height dependence in r and N. This is a strong indication that the microphysics in current (GS) models is fundamentally correct. The statement that r and N do not vary with height in the current GS scheme (models) is incorrect and not supported.

Line 146: Note that CIPS can not determine the PMC height, and only reports IWC which is a vertical integral. Since CIPS does not have the ability to examine height dependence in any way, this statement is nonsense.

Line 188: Your statement here could be easily tested with the thermodynamic equilibrium approach described a few lines later. SOFIE provides everything you would need (T, H_2O, P), and comparing the simulations to the measured Qice would be a tangible indication of microphysical (vs. thermodynamic) influences. This type of analysis could elevate the paper from conjecture to quantification.

Line 200: There are references on the nature of MSP particles this that should be quoted here. The Megner et al. papers are in your reference list, but you should also look at Bardeen et al. [2010]. Note that these are model simulations, and that the only observations are from a few rocket experiments [e.g., Havnes et al., 2019]. These papers will quantify how much the MSP N and r vary with height, and these details should be considered here, rather than just broad speculation.

Line 253, Fig 7: You examine the PMC r and N vs. height, all of which are observed simultaneously (i.e., the PMC properties are measured at each height in a cloud). Why then would you search for a time lag between the height and N (or r)? This seems like nonsense, and it would be surprising if a time lag was actually discovered.

Line 270: To state that this is “predicted by the CMN scheme” is unfair, since the paper does not present observations or simulations of the effect, but rather only makes speculative connections.

Line 295: The conclusions section contains an incredible number of unsupported statements, and publishing this would be irresponsible and damaging to the scientific literature.