Comment on angeo-2022-22
Anonymous Referee #1

Referee comment on "The solar induced 27-day modulation on polar mesospheric cloud (PMC), based on the combined observations from SOFIE and MLS" by Shican Qiu et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2022-22-RC1, 2022

Review of “The solar induced 27-day modulation on polar mesospheric cloud (PMC), based on the combined observations from SOFIE and MLS” by Shican et al.

General Comments

This paper investigates how the 27-day solar rotation impacts polar mesospheric clouds (PMC), using observations from the SOFIE and MLS satellite instruments.

The paper reproduces previous investigations by several different authors, who used SOFIE observations to study how the 27-day solar rotation affects PMCs, temperature, and water vapor. The Authors need to better motivate their work, especially since they are repeating previous studies. Specifically, they should indicate what advancements they are after, and/or if there are issues with the previous studies that they hope to resolve.

Implementation of the English language could be greatly improved. I cite a few instances below, but certainly not all of them. This makes the paper difficult to read, and could certainly be used as grounds for rejection. Rather, I ask the Authors to revise the paper, and to possibly seek assistance with the writing before sending it back for review. The scientific community needs to uphold a high standard of writing and grammar, and this paper does not meet such a standard.

Specific Comments

Line 12: It is certainly not true that temperature is considered by all to be the controlling
factor. Some studies indicate that water vapor is the major factor determining PMC variability [e.g., Lubken et al., 2021]. You should make a fair statement here.

Lines 12-17: This sentence is too long.

Line 30: What do you mean by “an increment of water”?

Line 37: Eliminate “the” before PMC and mesospheric.

Line 40-43: This sentence is too long.

Line 72: SOFIE and AIM were defined above.

Line 82: 1.5 degrees in what context? Is this the FOV width? Longitude?

Line 84: The MLS vertical resolution is roughly a scale height (~8 km) in the upper mesosphere. This makes it somewhat difficult for use in PMC studies, because PMCs have much finer vertical structure.

Line 85: Correlation between what parameters? What seasonal components are referred to? T & H2O I assume?

Line 88: A 35-day window will not remove the seasonal variation in polar summer. The seasonal variation in temperature for example, is more on the order of 100 days (Figure 1). 35 days rather removes natural variability and noise. Please clarify this discussion.

Figure 1b: You do not state where the temperatures come from.

Line 100: Add a reference here.

Line 103: “serial numbers”? Perhaps you mean “sequential”
Line 110-112: The nomenclature T_anomalies is awkward, consider using $T_a$ or DT (same comment for Y10).

Lines 115-120: This discussion is hard to follow, and should be rewritten.

Line 156: Temperature is spelled wrong. Is it true that $T$ drives the 27-day response in PMCs? What about water vapor? SOFIE measures H2O, and you should look at that here as well. Perhaps there is a relationship that others have missed?

Equation 6: It would be more appropriate to write $S$ as the ratio of the H2O partial pressure over the saturation vapor pressure. You should then state that the denominator in Eqn. 6 is the saturation vapor pressure according to Marti and Mauersberger [1993].

Line 161: The accepted unit for pressure is hPa.

Line 163: This does not mean that you can ignore water vapor. There are numerous papers that document a strong dependence of PMCs on water vapor, and you should certainly consider it as important here.

Line 164: Are you looking at $T$ from MLS or SOFIE or both? The pressure levels you select are below typical PMC heights (0.05 hPa or ~84 km). Is there a reason for this?

Line 165 (and elsewhere): I think you mean hPa, not pa, for the unit of pressure.

Lines 171-177: This discussion is poorly written and hard to understand.

Figure 2: “SOFIF” -> “SOFIE”

Line 184: visibility -? variability?

Line 187: “exits” -> exists
Line 192: I think you have confused the MLS and SOFIE results here. Furthermore, the anti-correlation is not significant for the SH using SOFIE T, and barely significant when using MLS T. This may indicate another pathway, perhaps water vapor.

Lines 203-205: What you offer as “an explanation” is simply a restatement of what is observed, and not an explanation of what may be causing it.

Figure 6 & Line 207: These results are puzzling. Panels a, c, and d show what is expected, with a positive correlation near zero time lag. Panel b seems to be completely opposite, with a negative correlation near zero time lag. This certainly warrants discussion, and perhaps some investigation to see if it is an error in the analysis, or something unique about that season.

Figure 7: There are similar discrepancies here as were mentioned concerning Figure 6.

Section 4.2: You show the correlation results vs. latitude in Figures 5-7 for 13 of the 16 PMC seasons examined. The presentation not in any logical order (i.e., by time...), and furthermore switches between NH and SH. I suggest that you rather show the SEA analysis vs. latitude in both hemispheres. Then, you can show one or two individual seasons of interest if they helps illustrate your ideas.

Table 2: Are these results for SOFIE or MLS?

Line 228: Is it also possible that the change with height is a reflection of a dynamical process? For example, vertical winds in the mesosphere are a few cm/s (or roughly 0.2 km/day), which is reminiscent of your change in time lag with height.

Section 5: The conclusions are concise, but are not clearly stated. This sections needs to be rewritten.