

1 General comments

The authors have made substantial changes to the original manuscript that have eliminated much of the questionable material pertaining to the evaluation of cloud thermodynamic phase temperature ramp schemes in models. They have also expanded the latter part of the manuscript pertaining to the statistical relationships between meteorological parameters and cloud thermodynamic phase, focusing on particular regions of interest. The results are intriguing, however, my main concern is that the physical interpretations of the many correlations in the manuscript are lacking. Moreover, many parts of the analysis appear to be speculative rather than rigorously verified. I also question the robustness of some of the results, which need additional clarification. Finally, many statements in the manuscript read as though correlation implies causation and care must be taken to avoid such statements, as there could clearly be other confounding factors involved. My recommendation is: publish with major revisions. Specific comments follow.

2 Major Comments

1. **Results, Section 3.1, Lines 354-356:** The authors write that the negative correlations between skin temperature and SCF in the mid- and high latitudes “mean” that high skin temperature promotes the glaciation of supercooled droplets, and that in the positive correlations “mean” that high skin temperature inhibits glaciation in the tropics. Firstly, correlation does not imply causation. These correlations may be caused by many different confounding factors that lead to mechanisms that are completely unrelated to skin temperature. What is the physical mechanism for why high skin temperature promotes glaciation in the mid- and high latitudes? This was never mentioned in the manuscript. The authors explain that high skin temperature in the tropics inhibits glaciation because it triggers deep convection, which lofts the liquid droplets to the colder isotherms, but what is the mechanism for the mid- and high latitudes? Secondly, the area of the tropics that the authors are referring to appears to be only a small part of the entire tropics (how many gridboxes/what is the percentage of the area of the tropics that is positively correlated?). Thirdly, if high skin temperature is indeed inhibiting glaciation due to a triggering of deep convection in the tropics, then shouldn't the coldest isotherm, i.e. -30°C also show positive correlations in the tropics as well? The correlations between relative humidity and SCF and vertical velocity and SCF appear to support your argument in the tropics, but please clarify the effect of skin temperature.

Next, there is an apparent contradiction between the authors' explanation of the correlations found in the tropics and Figures 1, 2 and 3. The authors explain that vigorous convective activity, high relative humidity as well as high vertical velocities in this region cause supercooled liquid to loft to

higher altitudes too quickly to allow for glaciation of supercooled liquid, which would suggest that SCFs should be higher in this region for the aforementioned reasons. Yet, Figs. 1, 2 and 3 all show that the tropics contain mixed-phase clouds with some of the lowest SCFs in the world. Please clarify.

2. **Abstract, Lines 57-59:** This is another example of a sentence that reads as if correlations between SCF and U imply that U is the cause for high SCFs in the mid- and high latitudes.
3. **Results, Section 3.1, Lines 384-385:** Again, another sentence insinuating that correlation implies causation.
4. **Results, Section 3.2, Lines 396-398 and 405-408:** These are yet more statements that read as though correlation implies causation. One cannot conclude from these correlations that the meteorological parameters examined in the study impact the SCF, but only that these results provide further evidence to support previous studies that have established a causal effect. Too many of such statements occur in the manuscript to list. Please revise the manuscript bearing in mind that correlation does not imply causation and eliminate all statements that imply a causal effect strictly from only a correlation analysis.
5. **Dataset and methods, Section 2.1:** Please provide more details on the GOCCP-CALIPSO product. How exactly is this product more consistent with the CALIPSO simulator within COSP (line 172: “fully-consistent” is too general)? Technical details starting with how the Level 1 CALIPSO data is processed is recommended. How are horizontally-oriented ice particles treated in the product? Were daytime or nighttime data used? Were there quality checks to minimize misclassification of thin cloud layers with aerosols?
6. **Results, Section 3.2, Lines 396-406:** The mechanism of how horizontally-oriented crystals are eliminated by strong horizontal winds is not clearly explained. Please clarify. Next, as mentioned in the previous comment, does your data include horizontally-oriented particles in the first place? And if so, did you look into whether the frequency of occurrence of these particles actually decreased before making this conclusion? Please clarify.
7. **Results, Section 3.2, Lines 362-370:** Please provide a clear physical explanation for why positive (negative) correlations exist between LTSS and SCF over land (ocean) in the mid- and high latitudes.
8. **Dataset and Methods, Section 2.3:** Why wasn’t the Level 3 product used? The Level 3 product was further processed and screened to remove some misclassifications between thin clouds and aerosols. The screening affected mostly cirrus clouds, but it may be worth repeating the analysis with this product to check whether the results are consistent.

9. **Results, Section 3.3, Fig. 11:** Have the authors tried grouping the high, medium and low RAFs into bins with different thresholds? Would the results be robust to having bins with different thresholds? I recommend the authors to redo the analysis using different threshold values for their high, medium and low RAF bins to check how robust their results are to the choice of RAF bins.

The extratropics of both the northern and southern hemispheres are grouped into a single category even though the aerosol loadings are very different for these two regions. I suggest that the authors separate their analysis of the extratropics into the northern and southern hemispheres and/or over ocean and land. This may help clarify the contributions from the various regions and enable more generalizations of the statistical patterns seen. For example, this could help to better interpret the “U”-shaped pattern seen in Fig. 11d.

3 Minor Comments

1. **Abstract, Line 40:** Please specify the months of the year. There were some changes to CALIPSO prior to November 2007.
2. **Abstract, Line 55:** Insert “more” after “relatively”.
3. **Abstract, Line 60:** There is a word missing in between “regional” and “of”. Perhaps “influence”?
4. **Introduction, lines 72-74:** It is misleading to discuss cirrus clouds in this context. The authors are referring to a layer of mixed-phase clouds at the same isotherm, not two different layers of clouds at two different altitudes and temperatures. The difference in the optical thickness of the mixed-phase clouds due to the liquid and ice partitioning is what is important, and not the greenhouse effect. The authors may also mention the difference in the lifetime effect depending on the liquid and ice partitioning. There should also be a reference here.
5. **Introduction, lines 84-85:** It would be clearer if the term “mixed-phase cloud” was mentioned here. The terminology is used later, but never defined upfront. Here would be a good place to introduce it.
6. **Dataset and methods, line 185:** Mlmenstadt → Mulmenstadt
7. **Dataset and methods, line 201-202:** Please provide a reference here.
8. **Results, Section 3.1, line 312:** 30 → −30
9. **Results, Section 3.2, Lines 419-421:** Have the authors checked that the high RAFs here are indeed due to dust and polluted dust? It may seem intuitive to make such an assumption, but the RAFs also include

smoke and it is not clear that it does not dominate the RAFs rather than dust and polluted dust without verification.