

Response to Reviewer #1's Comments:

Jiming Li et al. (Author)

We are very grateful for the Review #1's detailed comments and suggestions, which help us improve this paper significantly. Based on two Reviewers' comments, we made a lot of important changing in the revised paper. The detailed information includes:

- (1) Some grammatical errors and unreasonable presentations already were corrected in the revision and the paper also be edited by the nature language editing service to make it more readable.
- (2) We reorganized the introduction section. In addition, some superfluous information in each section was deleted and some interpretations in each section were added in order to make the manuscript more clear. In particular, we rechecked the results of Fig.1, 2 and 3, and added the detailed information about the CALIPSO-GOCCP product (such as, technical details) and method to calculate the SCF in the section 2.1.
- (3) In the section 3 (results part), we also added some physical interpretations of the correlations.

Major Comments:

We appreciated the insightful suggestion and comments made by reviewers. Some responses to the reviewer's comments are listed below:

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 it 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 grid boxes/what is the percent- age 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.

Response: We appreciated the insightful suggestion and comments made by reviewer #1. Indeed, the changes of cloud phase composition include many confounding factors, e.g., ice nuclei (IN) or dynamical processes. We cannot provide certain causation only through the correlation analysis. In this paper, our main purpose is to indicate the aerosols' effect on nucleation cannot fully explain the regional variation and seasonal cycles of supercooled liquid cloud fraction. Although these statistical correlations can't imply certain causation, we expect that these results may provide a unique point of view on the phase change of mixed-phase cloud. For the questions from Reviewer #1, we already added some physical interpretations of the correlations in the section 3.1.

Question:

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.

Response: Yes, Fig1, 2 and 3 all show that the tropics have the lowest SCFs in the world at any isotherm. This result is consistent with previous study from Hu et al.(2010). We consider the possible reasons as: (1) The CALIOP can't penetrate the optically thick

clouds (optical depth>3) to detect the lower clouds, especially over the tropics where cloud overlap is very frequent. (2) This study focuses on the cloud top phase of not only mixed-phase cloud, but also those pure ice or liquid clouds. It means that frequent tropical ice cloud may results in low SCF. (3) Those regions with lowest SCF usually correspond to the typical descending motion. In these moderate or strong subsidence regions, the cloud occurrence is infrequent at the atmospheric layer between -40 °C to 0 °C. However, a little high cloud (or ice phase cloud) still can exist at this layer. The major source of these high clouds is topography-driven gravity wave activity, advection from neighboring tropical convection centers such as the Amazon Basin, the Congo Basin, or ascent associated with mid-latitude fronts (Yuan and Oreopoulos, 2013). Thus, positive correlation between SCF and velocity also exists at subsidence regions with low SCF.

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.

Response: We already revised it.

3. Results, Section 3.1, Lines 384-385: Again, another sentence insinuating that correlation implies causation.

Response: We already revised it.

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?

Response: In the revised paper, we already added the detailed information about the CALIPSO-GOCCP product (such as, technical details) and method to calculate the SCF. For above questions, some interpretations are added in the section 2.1 in order to make

the manuscript more clear.

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.

Response: For the horizontal wind speed at 100 hPa, Noel et al. (2010) found that the frequency of oriented crystal drops severely in areas dominated by stronger horizontal wind speed at 100 hPa. This effect is especially noticeable at latitudes below 40°. But, they have not explained why the correlation between horizontal wind speed and horizontally-oriented ice particle is negative. We speculate that strong horizontal wind possible result in strong vertical wind shear, thus cause shear-gravitational wave motions to induce local updraft circulations (Rauber and Tokay, 1991). As a result, updraft possible perturbs the orientation of ice crystal. In addition, Westbrook et al. (2010) pointed out that supercooled liquid water layers is very important in the formation of planar ice particles susceptible to orientation at midlatitudes (see section 2.1).

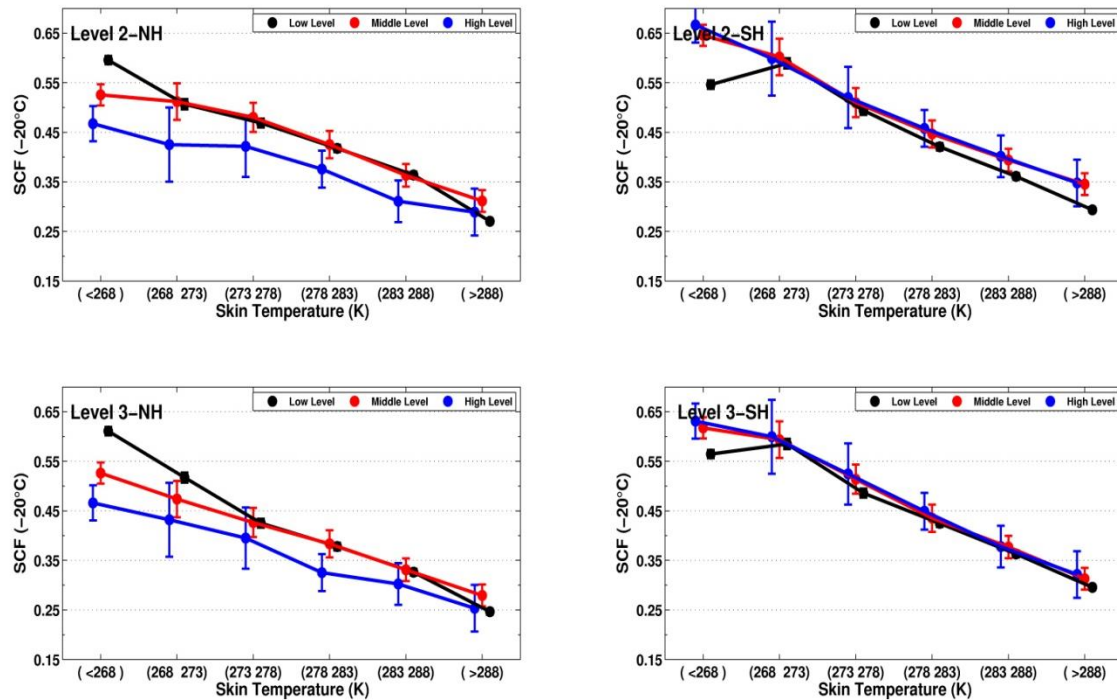
The GOCCP-CALIPSO product doesn't include the horizontally-oriented particles in their ice phase clouds, please see the section 2.1. We have to verify this conclusion in other study by using the VFM product of CALIPSO.

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.

Response: We already added some interpretations in the section 3.2.

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.

Response: We agreed with reviewer. Indeed, the Level 3 product was further improved compared with Level 2 product. However, by performing same correlation analysis with Level 2 aerosol product, we found the results are similar. As a sample, the figure below showed that the patterns of spatial correlations between skin temperature and SCF are similar. Thus, we still kept the results from Level 2 aerosol product in the revised paper.



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.

Response: Following the suggestions from Reviewer #1, we separated the extratropics into the northern and southern hemispheres to perform similar spatial analysis by using different RAFs thresholds. Related changes already were added in the section 3.3.

Minor Comments:

1. Abstract, Line 40: Please specify the months of the year. There were some changes to CALIPSO prior to November 2007.

Response: We already revised it.

2. Abstract, Line 55: Insert “more” after “relatively”.

Response: We already added it.

3. Abstract, Line 60: There is a word missing in between “regional” and “of”. Perhaps “influence”?

Response: We already added it.

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.

Response: We appreciated the insightful suggestions and comment made by reviewer. The introduction was reorganized in the revised paper. Some confused sentences and wrong quotations were corrected.

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.

Response: We already revised it.

6. Dataset and methods, line 185: Mlmenstadt → Mulmenstadt

Response: We already revised it.

7. Dataset and methods, line 201-202: Please provide a reference here.

Response: We already added the reference.

8. Results, Section 3.1, line 312: 30 → -30

Response: We already revised it.

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.

Response:

Response: Yes, we already confirm this conclusion by using Level 2 and Level 3 aerosol products. Dust and polluted dust indeed have higher RAFs than smoke in the selected grid.

Response to Reviewer #2's Comments:

Jiming Li et al. (Author)

We are very grateful for the Review #2's detailed comments and suggestions, which help us improve this paper significantly. Based on two Reviewers' comments, we made a lot of important changing in the revised paper. The detailed information includes:

- (1) Some grammatical errors and unreasonable presentations already were corrected in the revision and the paper also be edited by the nature language editing service to make it more readable.
- (2) We reorganized the introduction section. In addition, some superfluous information in each section was deleted and some interpretations in each section were added in order to make the manuscript more clear. In particular, we rechecked the results of Fig.1, 2 and 3, and added the detailed information about the CALIPSO-GOCCP product (such as, technical details) and method to calculate the SCF in the section 2.1. We hope that the added information can explain the doubt from Review #2.
- (3) In the section 3 (results part), we added some physical interpretations of the correlations.

Specific responses:

We appreciated the insightful suggestion and comments made by reviewers. Some responses to the reviewer's comments are listed below:

- (1) The method the authors used to derive the SCFs is not clear. They need to better explain how they did it. For example, there is no such -10 isotherm in the CALIPSO-GOCCP product. Did they choose one 3deg-temperature bin only for each -10 -20 -30 isotherm? I personally couldn't reproduce their figure 1 2 and 3 using CALIPSO-GOCCP data. Anyhow, I found particularly difficult to believe there are almost no SLF over shallow cumulus region around - 10degC, which is why I doubt the results presented here. Even if the calculations were correct, the result section is far too

descriptive and there are no discussions behind the description. The authors should revisit their results sections to make it shorter more clear and delete all descriptions irrelevant to the discussion.

Response: We are very sorry to mislead reviewers about Fig.1, 2 and 3. We rechecked the results of Fig.1, 2 and 3, and added the detailed information about the CALIPSO-GOCCP product (such as, technical details) and method to calculate the SCF in the section 2.1. In section 3, we also added some physical interpretations of the correlations to make the manuscript more clear.

(2) I don't understand the first paragraph of section 3. The authors seem to pick randomly parameters and try to find a correlation between them. It is very confusing and need to be fully re-written. Again, the authors have to pay attention to the explanations and clearly state why they chose one parameter over another and why did they change with respect to the bands of latitude.

Response: We appreciated the insightful suggestion from reviewer #2. In the revised paper, we reorganized the section3 and added some physical interpretations. Based on the spatial distributions of correlations, we choose different meteorological parameters to analyze the spatial correlations in different regions. For example, the correlations (at the 90% confidence level) between SCF and velocity (or u wind) almost locate at tropics (or middle latitudes).

(3) In general the method to compute the figures is unclear and blurry. For example, how did they get this 10x10 grid from the 2x2 in the correlation figures. What about the results in Fig.11.

Response: In the revised paper (section 3.2), we added the description of method. In addition, we also changed the grid size from 10x10 to 6x6. For the Fig. 11, the results are based on the spatial correlation analysis for those $6^{\circ} \times 6^{\circ}$ grids, which have correlation with high confidence level (90% level).

(4) Line 69 This sentence is wrong. The IPCC statement is as follow: "Cloud feedbacks remain the largest source of uncertainty in climate models" ipcc 2007/2013

Response: We already revised it.

(5) Line 97-103 The second part (However...) is confusing, please reformulate by adding something like may coexist "for a certain time" as opposed to "in equilibrium" found in the first part.

Response: We already reformulated it.

(6) Line 184 It is possible that GOCCP slightly underestimates the ice clouds in the lowest layers in the Arctic but not in the mid-lat cases as demonstrated in Cesana et al ., 2016 (Cesana G.,H. Chepfer, D. Winker, X. Cai, B. Getzewich, H. Okamoto, Y. Hagihara, O. Jourdan, G. Mioche,V. Noel and M. Reverdy, 2016: Using in-situ airborne measurements to evaluate three cloud phase products derived from CALIPSO, J. Geophys. Res. Atmos., 121, doi:10.1002/2015JD024334).

Response: We agreed with reviewer and already revised it in the revised paper.

(7) Line 188: which version? Day / night / day+night?

Response: Please see the section 2.1.

(8) Line 202 Does that mean you use the bin -9 to -12 for -10degC? Please clarify

Response: We added the related information in the section 2.1.

(9) Line 271: 10x10 seems very large. Did you try a sensitivity test at 5x5 that should be enough to gather statistically representative samples over 8 years.

Response: We already changed the grid size from $10^{\circ} \times 10^{\circ}$ to $6^{\circ} \times 6^{\circ}$.