Anonymous Referee #1

General Comments: This paper explores important relationships between aerosol properties (specifically mineral dust loading), cloud phase, and other dynamic and thermodynamic variables. I commend the authors for attempting to provide a global perspective on this challenging to measure and understand phenomenon. The satellite-based perspective on aerosol cloud interactions can provide more insight into the global extent of aerosol influences than obtainable from measurements from a single observatory. In general, I think that the analysis completed is interesting and potentially sheds some light onto the relationships between mineral dust and cloud properties. Having said that, I do have some reservations and questions about the manuscript, and therefore would like to see some additional work completed before this paper can be accepted for publication. Additionally, I found the manuscript to be very dense, and found myself having to reread sections on a regular basis. I'm not sure whether this was the result of the frequent use of abbreviations, or the writing style, or something else. However, it was a challenge to read, which is unfortunate given the science that is in the paper.

We sincerely thank Anonymous Referee #1 for pointing out several potential sources of uncertainty in our methods. We have modified several parts of the paper to address these points.

Specific Comments:
1.a) I wonder to what extent the different products used in this evaluation are consistent with one another. This is particularly a large question for the reanalysis derived estimates of temperature, humidity and vertical velocity. Since the reanalyses rely on models to provide input on clouds and dynamics, the values represented in these products must be internally consistent — however, this may not always match the real atmosphere. This is particularly true in areas of cloud cover, where the model clouds and real clouds may not match in time, location and phase.

We are aware of this issue and addressed the problem in lines 371-373 referring to the simulated dust mixing-ratio. Similarly, we assume that *statistically* the derived estimates of temperature, humidity and vertical velocity are accurate enough to evaluate their behaviour at different day-to-day loadings of mineral dust.

We agree that the comparison between the cloud phase from reanalyses and satellite data would produce a significant source of uncertainty. Therefore, we had already excluded the estimates of ice occurrence from the reanalysis to avoid issues resulting from such miss-colocations.

Nevertheless, we have now extended lines 371-373 to include this issue: "Similarly, the atmospheric parameters from the reanalysis may not match the exact position of the clouds in the satellite retrievals. However, we expect the atmospheric parameters to match in average the large-scale conditions influencing the observed clouds."

1.b) Therefore, I wonder whether there may be instances where the model (reanalysis)-produced thermodynamic state is inconsistent with the clouds detected from satellite measurements, potentially biasing the evaluation of observed cloud phase into different temperature regimes from reality. Some discussion on the potential for this to occur would be helpful.

It is possible to find cases where reanalysis and detected clouds have different temperatures. Although this may indeed contribute to the overall errors, we do not believe that this may present a major bias towards a certain cloud phase. This is because the temperature profiles used to bin the measurements and reanalysis into the different temperature regimes (3 K bins) are independent to each other. Therefore, a systematic bias would have been noticed in the comparison between both cloud phase products.

We have now added this point to the Discussion.

2.a) There is a substantial question related to the ability to truly detect relationships between dust and cloud phase in the absence of sufficient constraints on environmental state and dynamics. The co-variability that is demonstrated is interesting, but how can one be sure that this is the result of the aerosol, and not of the dynamical forcing on the cloud?

We agree that the constrains on the data are not enough to prove a causal relationship between dust aerosol and cloud phase. Given the limited data available, we found that additional temporal constrains on the dataset (e.g. dynamic regimes) leaded to insufficient sample sizes in the individual regimes. Therefore, we focused in analysing the co-variability between dust aerosol and some key dynamical parameters in Fig 8 (RH, cloud height and updraft) to rule out the possibility of some of these parameters driving the co-variability observed in Fig 6. From this analysis, we concluded that the co-variability between dust mixing-ratio variability and dynamics appears to be too low to support that the observed increases of ice occurrence in Fig. 6 are controlled by dynamics.

However, we agree that a methodology to isolate the effect of aerosols from the correlated effect of dynamics is still lacking. Therefore, we have now emphasized the need for such a methodology in our outlook: "Additionally, the further development of a methodology to isolate aerosol-cloud interactions from atmospheric dynamics has the potential to reduce much of the uncertainty found in this study."

See also the response to Anonymous Referee #4: "We agree that stability is a useful parameter for separating between aerosol-cloud interaction regimes.
However, #4.1a shows that we found no significant difference in the day-to-day correlation between dust and ice occurrence for different stability regimes (defined as "unstable", "neutral" and "stable"). Nor were the day-to-day changes in stability associated with changes in ice occurrence.

We used lower-tropospheric static stability (LTSS) following Li et al. 2017 (defined in (Klein and Hartmann, 1993))."

2.b) Particularly at high latitudes, where figure 8 appears to show a relationship between dust mixing ratios and vertical velocities, it could be challenging to discern the impacts of the dynamics from those of the aerosols. I will say that it seems to me that the relationship is such that you would not necessarily expect that the dynamics and aerosols would work in the same direction – with increasing mineral dust loading you have increasing vertical velocities (upward), which would support enhanced supersaturation. Therefore, the cloud liquid would increase in response to the updraft but decrease in response to the mineral dust loading. Assuming I have that correct, this could help to support the idea that the changes are the result of aerosols and not dynamics, but I think that a good amount of discussion on this topic is warranted.

We thank Anonymous Referee #1 for this helpful argument. The correlation between dust mixing-ratio and updraft suggest indeed that (everything else held constant), the ice occurrence should decrease at higher dust mixing-ratios. In fact, larger updrafts favour supersaturation and therefore CCN activation, droplet growth and inhibition of the WBF (Wegener–Bergeron–Findeisen) process. All three processes would lead to a lower cloud ice occurrence. One could then argue that the increase in ice occurrence for higher dust mixing-ratio should be even larger if the effect of updrafts is considered. We agree however, that this would raise a larger discussion. Immersion freezing, for example, requires a saturation over liquid water. This could result in updrafts promoting heterogeneous nucleation.

The spatial correlations in the study of J. Li et al. 2017 show actually an increase in cloud ice occurrence for higher large-scale updrafts. Although not included in the paper, we also found a day-to-day increase in cloud ice occurrence for higher updrafts. Therefore, the in-depth analysis of the dust-updraft-iceOccurrence co-variability would need a new evaluation of the relationship between large-scale updraft and ice cloud occurrence. As mentioned by Anonymous Referee #1, this would lead to a large (and necessary) discussion but this is outside the scope of this study.

This issue has been now commented in the Discussion.

• Line 129: What, if any, sensitivity is there to the order of the averaging?

Temperature is the dimension along which the cloud phase variability is highest. If temperature were the first ordering dimension, the information about each column would be biased towards the temperatures with the largest cloud cover. For example, a larger cloud fraction at higher temperatures would lead to a lower average ice cloud occurrence. This is because many columns would only contain data for higher temperatures (lower ice cloud occurrence).

Figure #1. 2.b shows the effect of reversing the averaging order. In the northern high latitudes (cyan box), the cloud volume fraction is significantly higher at higher temperatures (See S8 in the supplement). This produces a bias towards lower ice occurrences when averaging temperature at first.

From sect 4.2 and 4.3 we established that temperature, followed by latitude are the dimension with the largest variability of ice cloud occurrence. By averaging such dimensions last, we minimize the bias produced while averaging a dataset containing missing values.

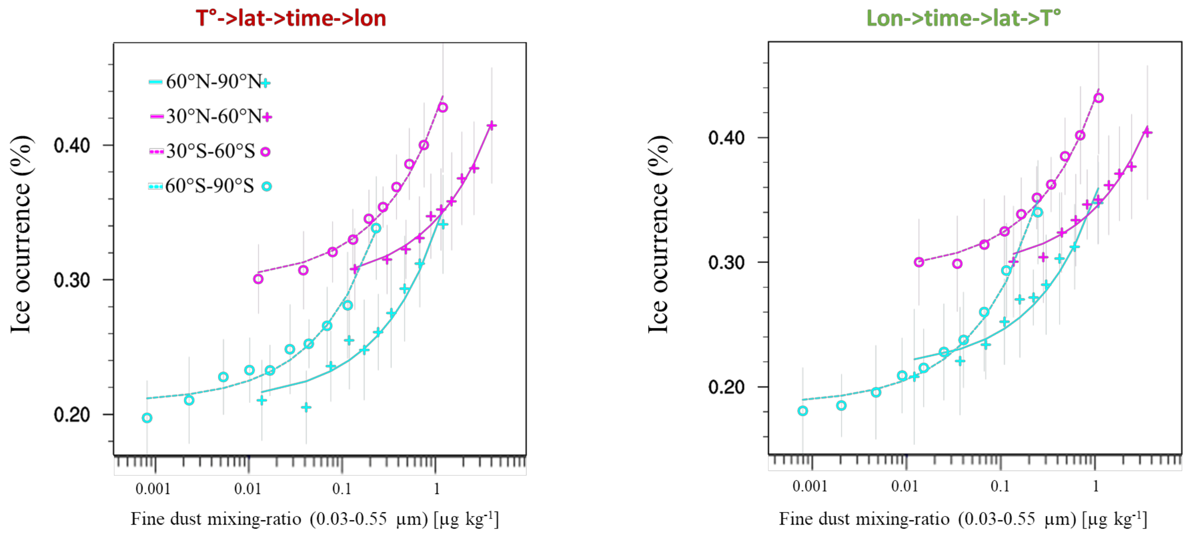
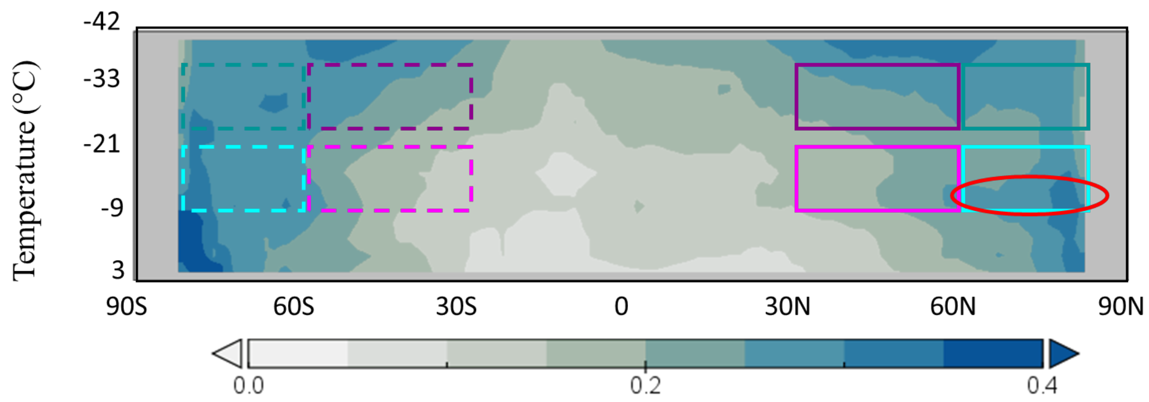


Figure #1.2.b *Effect of averaging order for FPR_{GOCCP}*



S8. Zonal mean of cloud volume fraction [%]. CALIOP-GOCCP 2007-2010.

• Lines 139-140: Please describe what cloud depth constitutes precipitating vs. nonprecipitating in 2B-CLDCLASS

As briefly mentioned in lines 88-89, the 2B-CLDCLASS product uses mainly the radar reflectivity to classify clouds as “precipitating”. The radar is sensitive to large particles (e.g., rain drops) and therefore clouds with a reflectivity larger than a given temperature-dependent threshold are defined as “precipitating”. The fifth range gate (~1.2 km above ground level) is used for the classification. The threshold is defined between -10 and 0 dBZ for temperatures between -10 °C and 0 °C and constant outside this temperature range (Hudak et al., 2009).

Hudak et al. 2009 offers a validation of the 2B-CLDCLASS precipitation product and a brief description (paragraphs 10-11) of the precipitation algorithm.

References: Hudak, D., Rodriguez, P. and Donaldson, N.: Validation of the CloudSat precipitation occurrence algorithm using the Canadian C band radar network, J. Geophys. Res. Atmos., doi:10.1029/2008JD009992, 2009.

These points have been added to the Methods section.

• Line 179: 30x1.825 deg — depending on latitude, this can be a very large amount of area. Therefore, I would be concerned about sub-grid variability, particularly at lower latitudes. For example, dust and cloud could be on different sides of a front within a given grid box. This is discussed a bit starting on line 370. Additionally, there could be significant gradients in phase over such a large horizontal extent, particularly around mid-latitude frontal zones, coastlines, etc. Such gradients may have little to do with the dust, but everything to do with changes in forcing.

As mentioned by Anonymous Referee #1, our approach does not consider such post-/pre-frontal differences in the analysis. Whether the relationship shown in our study is also applicable within mesoscale convection systems is a very interesting question.

Nevertheless, we believe that for studying such mesoscale phenomena another toolset is required, including mesoscale modelling and tracking of frontal systems.

• Figures 3 and 4: Recommend removing the dotted vertical lines to improve clarity.
Removed.

• Figure 3: "Error bars" — this isn't really error, is it? Just the standard deviation and variability?
Renamed.

• Figure 4: Same comment about "error bars"
Renamed.