After the first review stage, the manuscript by Villanueva et al. has been improved mainly clarifying in the text a few parts which were unclear and adding a few aspects which were missing and related to sources of uncertainty and limitations within the proposed approach to quantify the impact of mineral dust on the day-to-day variability of stratiform cloud glaciation.
The main manuscript goal is to introduce a new “metric” to quantity the indirect radiative impact of aerosol-cloud interactions.
Therefore, they have the unique opportunity to discuss they method on a rigorous quantitative basis, despite of the intrinsic limitations of the utilized datasets and the contingencies in their interpretation.
I am still convinced that the value of using the day-to-day variability to quantify the impact of mineral dust is not fully demonstrated given the number of unquantified factors and uncertainty contributions.
Nevertheless, the manuscript is interesting though it must report an analysis which will results incomplete, per its nature, being an innovative idea proposed to the community but based on not “tailored” data. This would not be an issue if all the assessable factors playing a role would be properly quantified. This can guarantee the validity of the results within the uncertainty margins.
It must be stressed that the presented analysis may strongly depend on the utilized dataset, thus loosing of generality, for example because of missing daytime data.
In summary, I could say that I am not fully satisfied by the changes in the updated version of the manuscript. I think some additional work was requested and this was not done. Several aspects touched by the referees have been solved by the authors with the typical statement “we expect that…..” without any quantification.
Trying to be concrete in the benefit of the paper and considering the conclusions of the manuscript, I’ll try to provide final recommendations of the minimum work which to my opinion must be added in order to provide a convincing message which can really stimulate future studies.

Anyhow, I will not contest the editor final decision if his overall opinion on the review process is satisfying.

1. *Between –36°C and –9°C, day-to-day increases in fine-mode dust mixing-ratio (from lowest to highest decile) were mostly associated with increases in the day-to-day cloud ice occurrence-frequency (FPR) of about 5% to 10% in the mid- and high- latitudes.*

This conclusion relates to the night time data only, this is due to intrinsic dataset limitations (sensor issue and the related period). The authors claim that the presented method can be applied in general but I think they miss in their data the cloud diurnal cycle which is not averaged out by the monthly mean and must be considered in the quantification of the aerosol-cloud-radiation effects.
Nevertheless, to requires the use of more recent CALIPSO data not compromised by spurious effects is too demanding.
For other cycles, possibly present in the data, I acknowledge that, as the authors states, it might be still possible to distinguish between dusty and non-dusty conditions at each point of the weather cycle but the uncertainty affecting their conclusion is not quantified.

2. The response of cloud ice occurrence-frequency to variations in the fine-mode dust mixing-ratio was similar between the mid- and high- latitudes and between Southern and Northern Hemispheres. Moreover, increases in FPR from first to last dust decile were also present in the northern and southern high-latitudes, even though dust aerosol is believed to play a minor role in cloud glaciation in the Antarctic region.

The dataset is quite heterogeneous in terms of samples in the different zonal regions and in particular at the South Pole. I suggested a re-gridding of the data in an irregular way which can enlarge the sampling where it is poorer, reducing the related uncertainties. Though this could be not the best method, a new way to make the authors’ investigation more robust is needed. Otherwise the presented results are too driven by the dataset limitations. I saw the authors added a paragraph to stress the limitations in the general validity of their results; this can be considered sufficient.
3. Using constraints on atmospheric humidity and static stability we could partly remove the confounding effects due to meteorological changes associated with dust aerosol.

This is a point where to my opinion an addition effort is required.
Here, a multivariate analysis (or anything similar) could tell us more and this should be done to give more value to the manuscript. in order to quantify the influence of static stability and humidity on the dust-cloud-phase relationship, a different and organic statistical approach is needed. Same applies to the correlation between dust mixing-ratio and the large-scale vertical velocity, where the authors provide in their answer the calculation or the Pearson's coefficient which reveal a faint correlation.
To support the authors' speculations, often interesting, a broader statistical analysis should be performed to strengthen the final message.

4. The results also suggest the existence of different sensitivities to mineral dust for different latitude bands. The north-south differences in ice occurrence-frequency for similar mineral dust mixing-ratios agree with previous studies on the mineralogical differences between Southern and Northern Hemisphere. A larger fraction of feldspar in the Southern Hemisphere could explain the differences at –15°C, and the higher freezing efficiency of Illite and Smectite (more abundant in the Northern Hemisphere) over Kaolinite (more abundant in the Southern Hemisphere) could explain the differences at –30°C.

This is a very interesting speculation and I think, even though it would be valuable, no additional effort is needed for this part of the discussion.

Finally, I ask the authors to more clearly mention in the paper the ongoing debate on the relative contribution of homogeneous and heterogeneous freezing, using also the reference mentioned in their answer (Barahona et al., 2017; Dietlicher et al., 2018).