Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-661-RC2, 2019
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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The day-to-day co-variability between mineral dust and cloud glaciation: A proxy for heterogeneous freezing" *by* Diego Villanueva et al.

Anonymous Referee #1

Received and published: 27 September 2019

The manuscript by Villanueva et al. aims at contributing to the interpretation of the role of mineral dust in the formation of ice clouds. In particular, the authors want to demonstrate that the day-to-day che co-variability between mineral dust concentration and cloud glaciation can be used in future as a proxy to better understand heterogeneous freezing mechanism. First of all I must admit that the manuscript is hard to read in several parts, and it takes more than one reading to make sure that the reported content is fully understood. The assessment of the most suitable datasets utilized for the study of heterogeneous freezing as well as the statistics related to the relationship between ice occurrence, updrafts and dust concentration are very interesting. Nevertheless I am not fully sure that this manuscript demonstrates the value of the day-to-day

co-variability as a proxy of the effect on cloud glaciation.

Below I report my general comments.

First of all, the authors have selected data from many difference sources, MACC and ERA interim meteorological reanalysis CALIPSO-GOCCP, DARDAR products from the A-Train satellite constellation. What are the effects on the final results presented in the manuscript of combining these datasource with different speculation (resolutions, sampling, uncertainties, ..)?

The authors often do assumptions and simplifications (eg. use of night time measurements only, neglecting of ice in the mixed phased clouds,. . .) which can strongly increase the uncertainty of the final results and limit the value of the data interpretation. Cloud phase is mainly regulated by temperature and so it is not clear to me why the authors considered only nighttime measurements. What's the effect of this data selection on the final results?

The scope of the manuscript is to demonstrate that the day-to-day co-variability of dust concentration ice cloud glaciation may be used to quantify the role of dust aerosol on the cloud thermodynamic phase around the globe. I understand that, in order to use the day-to-day variability, the authors removed the seasonal component subtracting the monthly means. I am not sure this is sufficient to remove all the possible variabilities which can affect the selected data. Weather variability for example occurs on a scale of 5-6 days. Can the authors explain how they can assure the data are not affected by any other relevant variability cycle? In addition, it is not clear to me from the reported the description whether the 3K binning can smooth the day-to-day variability (though the binding was needed with respect to the considered dataset) or anyhow mix different observation scenarios, i.e. high dust content and low content but at the same temperature. I think the description in section 3.2 must be clearer.

Many time time the authors state that the presented results can be affected by assumptions or effect not considered but also that several properties, which at regional scale may have significant difference, should be reconciled by the fact that the static are calculated at the global scale. This is not true for all the variables, RH is an example. There might be strong variations of RH at regional scale in one hemisphere only, which can affect the value of the interpretation of results.

Sampling uncertainties are mentioned a few times by the authors themselves, though these are never quantified. For example, if the regions where the data sample is larger and more complete is resampled to reduce the amount and obtain a dataset of homogenous size across the zonal regions what the effect would be?

Below I report some detailed comments sometimes still of general breath.

Line 177-180: Re-gridding operated here generates also a degradation of the horizontal resolution whose effect on the provided analysis is not quantified. It is not clear to me if this is a real advantage or not.

Line 185: replace "in the study" with the "in the study by Huang et al"

Line2 190-192: please clarify that the choice to ignore ice in mixed phase clouds is an advantage according to the approach your are adopting but also that the authors cannot be sure this has not an impact on the final results.

Lines 264-266: the authors limit their investigation to the altocumulus clouds: can they quantify the impact of this choice on the final results?

Line 322: I think this simplification can create confusion only.

Lines 323-325: though concentration of dust is lower at high altitudes, this does not necessarily indicates that this is due to lower temperatures; this sentence create confusion and solve in a few words a more complicated issue which involves also many other factors, such as atmospheric dynamics and radiative budget. For example, there might be a feedback mechanism influencing the top altitude of aerosols. I think the sentence must be rephrased or otherwise removed.

Line 330: the authors should clarify the reason for the supposed correlation between the maxima observed in the NH and at the tropics, and how the transport of ice clouds downward may occur. Can this be related to any wave activity at the synoptic scale?

Lines 342-343: I do not see the steep increase at the Northern Pole. I ask the authors to clarify.

Lines 345-347: I think the authors may remove this lines, too conjectural; digressions are not needed in that part of the manuscript.

Lines 348-350: how much does the number of data influence you conclusions at the South Pole? The authors should discuss this aspect in the paper.

Lines 352 - 355: the correlation mentioned here between the updraft and the FPR looks not so strong, can the authors provide numbers (i.e. regression coefficient or any other statistical tests)? Given that a correlation with two different parameters of the FPR is studied, is it the case to carry out a partial correlation analysis?

Lines 423 - 427: I agree with the statement provided by the authors though they should acknowledge that in the SH with low USST and high RH the positive correlation is much lower than in other conditions. Can the authors comment a bit more on this aspect? Do the authors envisage a larger contribution in the SH of the homogenous nucleation than in other regions?

Lines 435-439: these lines are to speculative, I'd honestly remove them.

Lines 441-443: Can this results be due to the purer nature of the dust in the SH compared to the NH, where it is often mixed to other aerosol types? In the discussion following to these lines, the authors mention the aged aerosol but never the effect of the aerosol mixing.

Line 502: among the significant number of factors contributing the uncertainty affecting the presented analysis I'd add the limitation to consider only a specific type of cloud type, and only night time observations, as well as the effect of the electric charge of mineral dust particles.

Line 535 and following: in this section there are few sentences which are very speculative and though these are able put on the table the pletora of different interpretations to te presented data, at the same time, may be not always helpful to the users, also considering that this is not a research article. I suggest to shorten it or arrange in clearer way.

Line 547: something missing in this sentence.

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