Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-1074-RC4, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of mineral dust on the day-to-day variability of stratiform cloud glaciation occurrence" *by* Diego Villanueva et al.

Anonymous Referee #4

Received and published: 12 December 2018

This paper shows how satellite observations can be associated with aerosol model reanalysis to infer the impact of dust on cloud thermodynamic phase transition. The authors used LIDAR and RADAR measurements from the A-train to retrieve information on the cloud thermodynamic phase using different products (DARDAR, GOCP) and the reanalysis MACC to co-locate dust mixing ratio and updraft. Therefore, the study retrieves the frequency phase ratio as a function of dust mixing ratio constrained for different regimes of latitude, temperature, season, etc. The aerosol-cloud interaction problem is a difficult subject to study with observations because aerosols and clouds properties cannot be spatially and temporally co-located. The use of satellite and reanlysis circumvents the problem. The results show that the cloud ice fraction increases

with the concentration of dust, suggesting that dust plumes contain ice nuclei which is line with previous studies as mentioned by the authors. Each effects are quantified, the manuscript is well written and well structured in my opinion. The objectives of the study are clearly mentioned in the introduction and the results associated with their significances are stated in the conclusion.

The topic of this study matches very well with the journal topics and is of great interest for the atmosphere community, in particular people studying aerosol-cloud interaction. However, I consider that the article needs important revision that I estimate necessary to be published: the discussion on meteorological parameter impacts is too short, the data section is too short... (see below for detailed descriptions).

Major revisions: 1. Meteorological parameters have a larger impact on cloud properties than aerosols (Gryspeerdt et al., 2016). Different meteorological regimes can change the aerosol-cloud interaction by an order of magnitude. Even if you mention in your paper the meteorological parameters (sections 4 and 5), it is missing in the paper. You refer to humidity, but the stability is also an important parameter in the aerosol-cloud interaction. The spatial resolution of ERA can seem coarse but it could constrain your situation and could avoid any correlation you are referring to (line. 400): You might not have the atmospheric state at the cloud but it refers to general atmospheric processes which are important as well.

Also, the boxes you considered based on latitudes-longitudes contain both land and ocean which are in different regimes of aerosols and meteorological parameters, I would like to see a differentiation between land and ocean. Moreover, you based your study on dust aerosols, but other parameters can have an impact on the ice fraction (soot, sea salt, sulphate...), the low correlation you observe in the hemisphere north could also be due to the fact that there are more different aerosol types which can act as IN as well.

2. You refer to the maximum number of points you can retrieve in line 126-127, but the

actual number of points never appears in the article, I suggest to add the number of data points in the Figures 6, 7, 8 for each point. The work is based on statistical analysis so the number of points is important, especially if you compare different regimes. I am particularly concerned by Figure 4-b and the increase of ice occurrence for latitudes lower than 73S (maybe the results are not statistically significant because you do not have enough data points).

Also I am concern by Equation 3.6: If I understood, you average for each dust mixing ratio bin to have constant number of 10 data points. This method artificially increases the correlation coefficient. Could you measure the Pearson correlation based on the 2-D histograms. For example, in Figure 6-a, what would the correlation coefficient be if you consider all the couples (iceOccurence-FineDustMixingRation) without averaging for Fine dustmixing-ration bins first.

3. The data section lacks necessary information. The satellite needs to be described more precisely with information about the performance of the algorithms: When they are compared to in-situ or ground-based measurements, how do they perform? What are the methods to derived cloud properties? The same goes for MACC, in line 120 you refer to "good results", can you develop and quantify.

4. You plotted the uncertainties in your figure but you do not refer to them in your text. For example in line 293, you use "notably higher", but if you consider the uncertainties in the figure, the difference is not that high. Can you comment on that?

Minor revisions:

- Introduction: There is plenty of different methods to study the aerosol-cloud interactions. The method you are using present fair advantages. A paragraph is needed to highlight this.

- The use of "e.g." needs a coma, example in line 28: (e.g., Patagonia, South Africa, and Australia)

- Method section: Are clouds vertically co-located with the dust mixing ratio?

- Figure 1 is not described, you need to introduce it and describe it.

- Figure 8-a and 8-b are not presented, can you describe them before referencing to them?

- Line 179: Why do you use 30 degree? Is there any specific reason? Did you try with boxes of 20 degree for example? You refer to "optimize the number of different satellite swaths", I do not understand, can you develop?

- Equation 3.1: Has it been used in a previous study?

- Line 226: "2,5" should be "2.5"

- Line 278: How do you explain that you have a larger correlation in the southern hemisphere compare to the northern hemisphere? Can you speculate?

- Line 309 - 315: This paragraph is not clear, can you reformulate?

- Line 345 : "In contrast ..." : For the other cases where you have a lower correlation, can it mean that the glaciation happened before, and therefore you do not find a good correlation but dust plumes still contain IN, can you comment on that?

- Line 437: you mention in the paper that you are substituting the m2/3 to do a linear regression, but not in the conclusion, so it is confusing when you refer to m2/3 as linear. Can you clarify this in the conclusion?

- Figure 2 caption: "— are reclassified a ice", I think you mean liquid.

- Figure 2: You put arrows on the colorbars but it cannot be greater than 100% ice, or 100% liquid.

- Table1: It took me a while to understand Table 1, there is a lot of information, and some of them are never mentioned in the text. Can you simplify it? I have the feeling that it could actually be two different tables.

References: Gryspeerdt, E., Quaas, J., & Bellouin, N. (2016). Constraining the aerosol influence on cloud fraction. Journal of Geophysical Research: Atmospheres, 121(7), 3566-3583.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1074, 2018.