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Interactive comment

Interactive comment on "Ice Particle Production in Mid-level Stratiform Mixed-phase Clouds Observed with Collocated A-Train Measurements" *by* Damao Zhang et al.

P. Seifert (Referee)

seifert@tropos.de

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The underlying manuscript of Zhang et al. is a follow-up publication in a series from the first author using combined CALIPSO and Cloudsat observations to investigate the structure of stratiform mixed-phase clouds on a global scale. The approach has been extended with respect to the previous studies by taking MODIS-retrieved LWP as additional constraint of microphysical properties of the observed clouds.

The presented results are of substantial value for the atmospheric science community, given that the study suggests the presence of a considerable hemispheric contrast in the ice formation efficiency in stratiform mixed-phase clouds. In principle, the dataset

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Discussion paper

seems to be well characterized and the data analysis methods can be considered mature, considering that several related studies were published by Zhang et al. since 2010.

Consequently, there are only a few critics points with respect to the technical implementation of the study. Nevertheless, the implied relevance of the results and the conclusions drawn are way beyond the data basis provided by the authors. This is a major issue. Basically, the message of the dataset is clear –> There is much less ice observed in stratiform mixed-phase clouds in the southern hemisphere. However, the conclusions are too linearly pointing toward aerosol effects. More efforts should be put by the authors on either supporting their strong conclusion, or, on providing a more brought discussion that includes other effects besides the aerosols. More details will follow below in the itemized review comments.

Major comments:

Technically:

1) What is the equation for the relationship between N_ice and Z? It should be presented because this relationship is discussed quite often. Which other parameters go into this equation? What is their role in the determination of Z? E.g., particle size. How much would particle size need to vary in order to explain the observed reflectivity differences? This is likely only a few percent due to the Dˆ6 relationship. May there be any ice growth processes that could explain such a slight hemispheric difference in the crystal size? What if the cloud height and thus the pressure level of cloud formation varies regionally and seasonally? See, for example, Chapter 13.3. of the book of Pruppacher&Klett, 1997 (Fig. 13-29) that presents that the diffusional growth rate of ice crystals varies by up to 100% between pressures of 1000 mb and 500mb.

2) Why is layer maximum ZL used as reference value? Why not mean or, as done by Bühl et al., 2016, the value closest to the liquid layer?

3) Figure 5: When doing hemispheric studies it is not straightforward to use seasons. Better is to use month ranges and then refer to boreal and austral seasons in the text.

4) P 3, L 14: “usually decoupled” is a very vague statement. How often are clouds coupled to the surface or to the planetary boundary layer? This could be easily checked by using global model datasets such as GDAS1 which also provide an estimate of the mixing layer height. It would be a good test to investigate surface effects by excluding/including cloud layers touching the atmosphere-ground mixing layer from/into the statistics. Is there a hemispheric/seasonal variability of potential surface effects?

5) P3, L24-25: Sassen et al. 2012 showed strong effects of specular reflection on the CALIPSO measurements before it was tilted to 3° off-zenith. The authors argue on P4, L21ff that this does not affect the lidar-based liquid cloud determination. Was there an actual check performed to evaluate this assumption? The signal of CALIOP is known to attenuate quickly, also under compact cirrus conditions. Figure 3 in Sassen et al, 2012 shows a dramatic change in the relationship between LDR and temperature between nadir and off-nadir pointing, especially at T>-30°C.

Comments regarding the argumentation:

1) How do the different data analysis methods of Zhang 2010, 2012 and the current one differ? In the current version it is only argued that the current study differs from the 2010 study by considering only single-layer clouds. But can this explain why the results are so different? I would be happy to see some more text dealing with the cross-evaluation of the different studies. Perhaps a table would help to clarify methodological differences.

2) It should be noted that strong hemispheric differences in het. ice formation efficiency were already presented by Kanitz et al., 2011. Since the way of data analysis is similar to the presented study it would be worth mentioning it.

3) P7, L4-13: There are a lot of statements given in this paragraph. “crystals reside

longer", "grow larger by the WBF process", "larger ice growth rate by accretion". Are there references available supporting these statements?

4) I personally strongly support the conclusion given on P8, L16ff. However, are there really no other effects beside aerosol properties that should be discussed? I strongly recommend to at least point to the possibility that the difference in the reflectivities can be either attributed to large changes in the number OR to very small changes in the size of the ice crystals. The evolution of ice crystals depends on a multitude of constraints. . .just take a look into Pruppacher&Klett, 1997 or into modeling studies of mixed-phase clouds, such as the ones of Ann Fridlind or Morrisson et a. 2012. Also the studies of Korolev and/or Field show that cloud dynamics can have a strong effect on the evolution of the ice crystals. Constraining LWP is already a great leap forward, but other environmental parameters such as cloud pressure or the relationship between the clouds and the planetary boundary layer are just a few examples of possible additional factors. Consider also, as another example: Average CCN concentrations in the atmosphere over the Southern-hemispheric (SH) Oceans are only one fifth of the northern-hemispheric values (Yum et al. 2004). Assuming constant cloud depth and liquid water path, much fewer but much larger droplets can be expected in the SH clouds. Heterogeneous freezing parameterizations (especially immersion freezing) rely mainly on temperature and aerosol properties but not on droplet size (See, e.g., Demott et al., 2010). Thus, having much less droplets available for ice nucleation will result in correspondingly lower ice crystal concentrations, even in the absence of any aerosol effect. This pathway could also contribute to the apparently less efficient ice formation over the SH. There are still a lot of unknowns that need to be resolved before we can actually pin down the observations solely to aerosol effects. I'm looking forward to a lot of future studies dedicated to this key question of current atmospheric research.

Minor comments:

P2, L 16: An impressive demonstration of the lifetime effect as a function of temperature is also given by Bühl et al., 2016.

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