Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-20-RC3, 2018

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Ice crystal number concentration estimates from lidar-radar satellite remote sensing. Part 1: Method and evaluation" *by* Odran Sourdeval et al.

Anonymous Referee #2

Received and published: 15 March 2018

Review of "Ice crystal number concentration estimates from lidar-radar satellite remote sensing. Part 1: Method and evaluation" by O. Sourdeval et al.

This paper describes an ice concentration retrieval based on the DARDAR Cloud-Sat/CALIPSO combined lidar-radar retrieval. The extension of DARDAR to retrieve ice concentrations, evaluation by comparison with in situ aircraft measurements, and global distributions are discussed. Although the ice concentration retrieval seems reasonable and potentially useful, I have significant concerns with the paper in its current version. In particular, I think the validity of the retrieval in regions without both lidar extinction and radar reflectivity needs much more discussion and evaluation. Also,

the use of 2D-S measurements for determining concentrations of small ice crystals is suspect at best. These issues (and others) are discussed in detail below.

1. The discussion of the retrieval algorithm in section 2 implicitly assumes that both extinction from the lidar and radar reflectivity are available. The authors should make clear early in the paper that the ice concentration retrieval is dubious in cirrus that are not detected by both radar and lidar (i.e., either too optically thin for detection by the CloudSat radar or below optically thick layers where the CALIOP lidar is blocked). When only lidar backscatter or radar reflectivity are available, the ice concentration is entirely dependent on the assumed size distribution. Mean PSDs are shown in the paper, but aircraft data shows that enormous PSD temporal and spatial variability is typically prevalent in cirrus. With only lidar or radar data available, this variability cannot be captured by the retrieval.

2. Page 6, lines 24-28: It would be helpful if some formal estimate of the uncertainties in N_i retrievals associated with measurement uncertainties could be provided.

3. Page 6, lines 26-27: Further discussion of the the uncertainty in N_i retrieval associated with PSD shape assumption should be included. Perhaps examples could be provided as a guide.

4. As noted in the manuscript, only 2D-S data was available from SPARTICUS. The 2D-S ice concentrations are overwhelmingly dominated by the 1st size bin (5–15 μm). Artifacts and uncertainties render the first bin or two of 2D-S measurements relatively useless. Most 2D-S data users do not use concentrations in the first two bins in their analyses because of the large uncertainties. I would recommend excluding the first two bins in the PSD comparisons shown in Figure 1 for temperature bins for which little or no ATTREX data is available. Also, I think it is inappropriate to use $N_i^{D>5\mu m}$ data from the SPARTICUS 2D-S-only dataset for evaluation of the satellite retrievals. The MACPEX 2D-S data should only be used for $N_i^{D>25\mu m}$ and $N_i^{D>100\mu m}$.

5. Figure 1: Indicate in figure or caption which temperature ranges correspond to

ATTREX data (mostly < -70°C) and SPARTICUS data (warmer temperature ranges).

6. Figure 2: The authors should note and discuss the D05 overestimate (by factor of 2–3) for small (D <10 μm) particles in -80 to -70°C bin compared to ATTREX measurements.

7. Figure 3: The small sample volume of the FCDP instrument results in discretization of the ice number concentration in steps of about 12 L^{-1}bin^{-1}. In other words, the FCDP instrument cannot effectively measure ice concentrations smaller than about 10–20 L^{-1} if the data is used at 1 Hz (as in this study). The CAS data has a similar sample volume issue. Since ice concentrations are often dominated by the small crystals sampled by FCDP and CAS, I would recommend not showing the in situ vs D05 comparisons for concentrations less than 10 L^{-1}.

In some of the temperature bins, the data extends to ice concentrations greater than 1000 L^{-1}. Extending the upper limit on the Figure 3 axes would be helpful to show how well the retrieval compares with in situ measurements at higher ice concentrations.

8. Figure 3: The authors should note that discrepancies up to factors of 2–3 occur but are difficult to see with the log-log axis scales.

9. Will the N_i data be made publicly available? If so, data quality flags should be included to indicate when both radar and lidar signals are available as well as when the retrieval is questionable based on in situ comparisons?

10. Page 14, lines 1-6: I do not understand what the authors are saying here. I was under the impression that Figures 2 and 3 simply showed statistical comparisons between the in-situ-measured and retrieved PSDs and ice concentrations. The first paragraph of section 4.2 suggests the comparisons in section 4.1 were ideal cases. Perhaps this idealization should be explained and emphasized at the beginning of section 4.1.

11. Section 4.2: I am not convinced that the near-coincident in situ/satellite retrieval comparisons are useful given the enormous spatial/temporal variability in cloud proper-

ties and the corresponding need for very close time and space coincidences for meaningful comparisons. Not surprisingly, the scatter in the comparisons shown in Figure 4 is very large, spanning 1–2 orders of magnitude.

12. Page 15, line 9-10: In contrast to the statement here, the DARDAR-LIM retrieval overestimates $N_i^{D>5\mu m}$ and $N_i^{D>25\mu m}$ compared to SPARTICUS data even in the -60 to -50° C temperature bins.

13. Figure 5: The comparisons shown here are very difficult to see, particularly those for lidar-only and radar-only retrievals. The relative agreement between lidar-radar, radar-only, and lidar-only retrievals should be shown in a separate figure, particularly since the lidar-only and radar-only retrievals are suspect.

Also, as discussed above, the SPARTICUS 2D-S-only ice concentrations for D>5 μm are dominated by the first size bin, with enormous associated uncertainties. The comparisons with SPARTICUS 2D-S-only ice concentrations including the first bin are of little value, possibly misleading, and should be removed.

14. Page 19, lines 8-10: the lack of clear transitions in retrieved properties between the lidar-only, lidar-radar, and radar-only regions does not necessarily mean the lidar-only and radar-only N_i retrievals are credible.

15. Figure 8: Scatter plots of $N_i^{D>5\mu m}$ and $N_i^{D>100\mu m}$ versus N$_{2D-S}$ would provide much clearer comparisons between the retrievals and measurements. Further, the points could be color coded to indicate whether they are in the lidar-only, lidar-radar, and radar-only regions. In the discussion of Figure 8, the authors claim good agreement between the in situ and retrieved ice concentrations, and they dismiss glaring discrepancies as being caused by the imperfect time coincidence. This argument seems unjustified. The flight track segment has been chosen for good time/space coincidence.

16. Section 5.3: The authors describe a cloud formation scenario with air parcels ascending across the -40° C isoline, which suggests that freezing of liquid drops could be the main ice formation mechanism. Yet they attribute the differences between the high and low ice concentration regions to differences in vertical wind speed and cite the strong sensitivity of N_i to w (citing Krämer et al. 2016; papers showing this sensitivity decades earlier should be cited). However, the strong sensitivity to w occurs primarily when aqueous aerosols freeze, not so much when liquid droplets freeze. Either the description is not clear, or the physical argument made does not make sense.

17. Figures 9 and 10: The discrepancy between $N_i^{D>5\mu m}$ and ATTREX FCDP ice concentrations noted above is apparent in the coldest temperature bins and the TTL. Typical values of $N_i^{D>5\mu m}$ are 200-300 L^{-1}, whereas ATTREX in situ measurements indicate ice concentrations of about 100 L^{-1} (Jensen et al., 2016). It is also interesting that the ice concentrations are higher over continental and convective regions even in the coldest temperature bins (near the tropical tropopause) where the vast majority of clouds form in situ. Additionally, it might be worth noting that the statistics must be poor in the coldest temperature bin poleward of about 30° latitude since such cold temperatures rarely occur there.

18. Page 22, line 7: Simply stating that the spatial distributions agree with the global model predictions is no doubt too strong. A quick examination shows there are some regions of agreement and some glaring discrepancies. I would either omit this statement or qualify it. Perhaps the comparison really shouldn't be discussed without providing much more detail.

19. Section 6.2: Most of the speculations about the physical causes of the zonal-height distributions in this section are either not justified or would require much more detail to adequately discuss. It does not look to me like there is a particularly sharp transition at -40° C, nor would one be expected given the importance of sedimentation in cirrus. The retrieval probably doesn't work well in the antarctic wintertime stratosphere since PSCs are typically mixtures of ice crystals, NAT particles, and ternary aerosols.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-20,

2018.