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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 #3

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The article presents a method to be used as an operational retrieval to derive ice crystal number concentrations of pure ice clouds, Ni, (T < -30°C) from combined spaceborne lidar-radar measurements (CALIPSO-CloudSat) and a thorough evaluation using in situ data from five airborne campaigns. An example of application is shown via a case study, including Lagrangian transport modelling. An interesting result is that regions with stronger updraughts show peaks in Ni with particle sizes > 5micron in contrast to regions of mature cloud, as one would expect. At the end, geographical maps and zonal profiles of 10 years of Ni are presented and discussed for particles with sizes > 5micron and >100micron. A follow-up paper will use these data in the framework of aerosol-cloud interactions. Ni is an essential microphysical parameter, which is re-

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cently used as a prognostic variable in climate models, and therefore it is important to have global observational constraints. The variable is also important for process studies. The combination of lidar and radar measurements, being part of the A-Train, allows to determine the vertical structure of clouds such as top and base of the clouds, cloud layering, as well as ice water content and effective ice crystal diameter. The attempt to derive ice crystal number concentration is relatively recent, as its determination depends on several assumptions (in particular a gamma-modified particle size distribution (PSD) and a specific ice crystal mass - maximal diameter relationship). The presented method is based on a direct constraint of the shape of normalized particle size distributions using lidar extinction and radar reflectivity from the operational liDAR-raDAR (DARDAR) products. 40000 in situ PSD's are used for an evaluation, investigating results separately for ice crystal sizes > 5, 25 and 100 micron, first for the prediction of PSD from N0* and Dm and then for retrieved Ni. The article is generally well structured and well written. I strongly recommend the publication of this article, after minor revisions.

Minor Comments

1) The methodology section 2 gains in clarity by integrating the content of section 2.1 into sections 2.3 and section 3.1, in particular as DARDAR products are data and the retrieved variables such as beta-ext, Ze and beta-ext are not defined. In that way the section on the representation of the size distribution gets section 2.1, in which the advantage of using scaled PSD's is described as well as the necessity to assume a certain m-D relationship and a certain shape of PSD. The new section 2.2 (Extracting Ni from DARDAR products) goes then further into detail how to extract Ni from the DARDAR products N0* and IWC. It should be clearly stated in the beginning that from N0* and IWC from DARDAR one deduces Dm and finally Ni. New Section 2.1: Be careful of replacing DARDAR by 'DARDAR retrieval (see section 3.1)'. Then a short description of the DARDAR products (like in initial sect 2.1) should be integrated into section 3.1. P4,l17-18 define beta-ext as (lidar) extinction and Ze as (radar) reflectivity

2) p 6, l22 it is stated that DARDAR retrievals of pure ice clouds for which the iterative retrieval converged too quickly are ignored. How many of these retrievals are these and can you explain which category of cases these are?

3) The evaluation of the prediction of PSD's and Ni (using all field campaigns) and later for retrieved Ni (using coincident SPARTICUS measurements)is shown separately for different temperature intervals, which is important as ice crystal particles shapes differ with temperature. It would be very interesting to separate also anvils and synoptic cirrus, as m-D relations might be different. Is there enough statistics of the collocated SPARTICUS campaign measurements to compare Ni distributions of Fig. 5 for anvils and synoptic cirrus?

4) section 3.2.2: One specific ice crystal mass – maximum diameter (m-D) relationship is used to determine IWC from the PSD. Indeed, Delanoë et al. 2014 show that the uncertainty to the m-D relationship for the normalized PSD is less important when minimizing using lidar extinction and radar reflectivity. The uncertainty seems to increase if only the lidar extinction is used for the minimization (Fig. 9). As both measurements are complementary, there are clouds for which only the first (thin cirrus) or the latter (towards the base of thick cirrus) are available. We also know that the shape of crystals changes with temperature and Heymsfield et al. 2010 showed that the m-D relation for anvil ice clouds yield masses about a factor of 2 larger than for synoptic ice clouds. Erfani and Mitchell 2016 cite this result in their paper and write that their results showing a similarity in m-D expressions between these two cloud types might be an artefact if the ice particle masses for a given projected area are quite different between these types. The L16 m-D relationship was developed for midlatitude cirrus. So for tropical anvils the computed IWC might be biased. Did you test the IWC computed with the L16 m-D relationship with the measured IWC for tropical anvils (using SPARTICUS and ATTREX) ?

5) Figs. 6 c and d of the case study present the trajectories as function of UTC. The relevant variable is the time difference which you show in brackets, and then the posi-

tion on the map in Fig. 6a. If it is not too complicated, it might be clearer to present instead of UTC longitude.

6) concerning Fig. 5, is it possible to get also De from DARDAR for this cloud ?

7) The long descriptive text of the case study is sometimes difficult to follow. I suggest for example to move the analysis of the collocated air track comparison (Fig. 8) to a supplement.

8) I would rename section 6 'Presentation of global Ni climatologies' and 6.1 'Geographical distributions'. P21l5: 'considered with caution' instead of 'cautiously considered'

Additional references

p 2, l 8: an IR spectral approach should also be mentioned: Guignard, A. C. J. Stubenrauch, A. J. Baran, and R. Armante, 2012: Bulk microphysical properties of semi-transparent cirrus from AIRS: a six year global climatology and statistical analysis in synergy with geometrical profiling data from CloudSat-CALIPSO, Atmos. Chem. Phys., 12, 503-525, doi: 10.5194/acp-12-503-2012

p 2, l 25: for liquid clouds one should also cite Han, Q., W. B. Rossow, J. Chou, and R. M. Welch, 1998: Global variation of column droplet concentration in low-level clouds, Geophys. Res. Lett., 25, 9, 1419-1422, doi :10.1029/98GL01095. Aerosol-Cloud Interactions with this dataset have been studied in : Han, Q., W. B. Rossow, J. Zeng, and R. M. Welch, 2002: Three Different Behaviors of Liquid Water Path of Water Clouds in Aerosol–Cloud Interactions, J. Atmos. Sci., 59, 726-735.

Typos: p 2, l 6: based on passive p 3, l 16: 'a suitable' instead of 'an suitable' p 6, l 8 : 'to demonstrate' instead of 'to demonstrated' p 15, l 12-13: 'a statistical comparison' instead of 'a statistical comparisons' p 16, l 2: 'is also indicated' instead of 'also is indicated' p 16, l 7: 'is observed' instead of 'tends is observed'

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Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-20/acp-2018-20-RC1-supplement.pdf

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