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Atmospheric Chemistry and Physics Discussions

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 #1

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General Comments:

This paper presents a new method for retrieving the ice particle number concentration Ni for glaciated clouds, which should be useful for understanding aerosol interactions with ice clouds and the contribution of homogeneous vs. heterogeneous ice nucleation in cirrus clouds. A satellite remote sensing scheme for Ni is needed since field campaigns cannot adequately inform us how Ni varies with latitude and the seasons. The paper is well organized and well written, and usually cites the relevant literature. The quality of the figures is good. The methods developed in Sec. 5 for testing the retrieval are especially creative and effective.

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Discussion paper

A critical limitation of the retrieval algorithm is the use of a normalized universal ice particle size distribution, or PSD (Delanöe et al., 2005), where it is assumed that all PSD in nature conform to this normalized PSD shape. This normalized PSD is based on a four-parameter gamma function (Eq. 4) where parameters No and k can be deduced through their link with other operationally retrieved properties (IWC and No*) while PSD parameters α and β need to be fixed as constants. This is of little consequence regarding β, which affects the largest ice particles having the lowest concentrations. But this is of major consequence regarding α, which strongly influences the smallest ice particles that govern Ni. This is not mentioned in the paper. The small end of the PSD is sensitive to the rate of ice nucleation which is sensitive to the cloud updraft w (with higher w making α more negative, and Ni higher), as well as the aggregation rate that removes smaller ice particles having higher concentration (Herzegh and Hobbs, 1985, QJRMS; Mitchell, 1991, JAS). Thus, some discussion on this topic is warranted, especially on the errors that may result from "non-standard" conditions where atypical updrafts are common (such as over steep orography).

The lead author gave a nice talk about this retrieval at the A-Train Symposium in 2017. Henceforth, Ni refers to Ni for ice particle maximum dimension D > 5 μm. Slide 20 of this presentation, showing global distributions of Ni for 10 °C intervals, appears almost identical to Fig. 9 of this paper for T < -30 °C, except that the Ni legends differ. The Ni values reported in the presentation are higher by a factor of $\sim$ 1.7 relative to the Ni reported in Fig. 9 of this paper. What is the reason for this difference?

A major finding from this study was a strong global temperature dependence for Ni, with Ni increasing with decreasing temperature (e.g. Fig. 9). Ni as observed from many field campaigns are reported in Krämer et al. (2009, ACP) where the middle value for Ni shows little temperature dependence. Although the SPARTICUS Ni measurements shown in Muhlbauer et al. (2014) show a temperature dependence, the study by Krämer et al. is based on many field campaigns. Please comment on this to help readers understand this apparent discrepancy.

Other important issues are discussed below.

Major Comments:

1. Page 8, line 25: The 2DS photodiode array length is 1280 μm, which should be noted. Evidently the "time dimension" is used to size particles up to 3205 μm; please indicate the particle selection criteria used to size and count particles.

2. Figure 5 and Sec. 4.2: For T > -50 °C, by what factor is Ni (Dmin = 5 μm) overestimated, on average? For T $\leq$ -50 °C?

3. Page 21, lines 9-12: The strong temperature dependence of Ni mentioned here appears at variance with the in situ measurements reported in Krämer et al. (2009). Please mention this.

4. Figure 9 and Sec. 6.1: For T > -50 °C, Ni tends to be lower over regions characterized by extensive marine stratus, like off the west coasts of South America and Africa (from equator and southwards). Is this result real, or is it an artefact of the retrieval? If the latter is true, please explain.

5. Page 21, lines 14-19: A similar finding was reported in Mitchell et al. (2016, ACPD), where the highest Ni were associated with mountainous terrain. (Although this paper was rejected since the editor felt the retrieved Ni values were too high, and therefore could not be used to infer nucleation modes, no arguments cast doubt on the spatial and temporal relative differences in Ni, which still appear meaningful.)

6. Page 22, lines 7-9: It is more meaningful to compare model results against observations than vice-versa. Suggest removing this paragraph. For example, in the modeling study by Zhou et al. (2016, ACP), the sensitivity of homo- and heterogeneous ice nucleation to various model parameters and updraft schemes were evaluated. Depending on how these are represented, one can get a broad range of Ni-temperature dependences, including Ni that is relatively insensitive to temperature (similar to the in situ observations of Krämer et al., 2009, ACP), and that modeling result would not support

these DARDAR-LIM findings.

7. Figure 10 and Sec. 6.2: Ni (Dmin = 5 μm) in the tropics appears contrary to the Ni results in Fig. 1 and Fig. 5 of Part 2 of this study by Gryspeerdt et al. (submitted). Fig. 1a of Gryspeerdt et al. show Ni near cloud top while their Fig. 5 shows that Ni does not change appreciably with distance below cloud top (up to 3 km from cloud top) between -50 and -60 °C. Assuming this result extends to other temperatures, the cloud top results in Fig. 1a of Gryspeerdt et al. should also be approximately valid below cloud top.

Regarding Fig. 1a in Gryspeerdt et al., for T $\geq$ -65 °C, Ni is never higher in the tropics relative to the midlatitudes. Between -55 and -40 °C, where the most optically thick cirrus clouds exist (cirrus defined as clouds having T $\leq$ -38 °C), Ni in the tropics is substantially lower than Ni in the midlatitudes. In Fig. 10 of Part 1 (Sourdeval et al.), Ni increases abruptly in the tropics for T < -40 °C (shown by the dashed curve), with Ni here being typically higher than Ni at similar T in the midlatitudes. This result appears opposite to the findings in Fig. 1a of Gryspeerdt et al. (Part 2). In addition, the CALIPSO Ni retrievals of Mitchell et al. (2016, ACPD) qualitatively support the findings of Gryspeerdt et al. (in terms of relative differences), and the in situ measurements from Muhlbauer et al. (2014) show relatively lower "peak Ni" values in anvil cirrus (vs. frontal, jet stream and ridge-crest cirrus). Finally, several studies (e.g. Jensen et al., 2013, PNAS; Spichtinger and Krämer, 2013, ACP), show that tropical tropopause layer (TTL) cirrus tend to have Ni < 30 L-1. Since the areal coverage of TTL cirrus exceeds that of anvil cirrus, and TTL cirrus tend to be higher than anvil cirrus (Gasparini et al., 2017, J. Climate), the Ni of $\sim$ 200 L-1 in the TTL region in Fig. 10 appears at variance with in situ observations. Please comment on, and, if possible, reconcile these issues.

8. Page 23, lines 1-3 and Fig. 10: Fig. 10 and this text indicate that in the midlatitudes for T < -40 °C, Ni is highest during winter and lowest during summer. This same result was found in Mitchell et al. (2016). One of the ACP review criteria questions is "Do the authors give proper credit to related work and clearly indicate their own new/original

contribution?".

Minor Comments:

1. Page 15, line 9: much => slightly?
2. Page 19, line 6: follows => follow?
3. Page 22, line 13: at => as?
4. Figure 10 caption: Mention the meaning of the dashed curve.
5. Page 20, line 1: an => a?

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