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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observed aerosol suppression of cloud ice in low-level Arctic mixed-phase clouds" *by* Matthew S. Norgren et al.

Anonymous Referee #1

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This is a review of the manuscript "Observed aerosol suppression of cloud ice in low-level Arctic mixed-phase clouds" by M. Norgren et al., submitted to ACPD. The authors analyze a 9-year long record of cloud and aerosol observations to describe in-cloud microphysical processes and the dependencies on the degree of pollution.

The results are interesting and highlight the further need to pin down the role of aerosols in determining the properties of cloud ice particles. The paper is well written in a mostly clear and organized way. In the following, I am raising a number of concerns which should be addressed prior to publication.

1) Throughout the manuscript, it is questionable to draw direct conclusions on the process of ice nucleation because nucleation alone may not be the only mechanism of initiating cloud ice. In particular when riming is assumed to be non-negligible, the po-

tential roles of rime splintering should be pointed out. In addition to that, mechanisms like droplet shattering upon freezing and upon ice-ice collisions have been under discussion recently. Overall, each of the ice-initiating mechanisms seems to be far from well-described or understood. Therefore, it feels more appropriate to refer to modified ice number concentrations only, rather than linking the indirect observations described here to exactly one of these processes. In fact, a recurring finding in studies of ice nucleation is that the concentrations of ice nuclei cannot explain ice number concentrations. Nevertheless, a discussion of potential mechanisms will be valuable at some point of the paper.

2) A point that remains unclear to me is related to the subsampling of data. The focus of this study are mixed-phase clouds, so the threshold for humidity is chosen to be 100% saturation with respect to ice. However, mixed-phase implies the presence of liquid droplets. In a mixed-phase cloud, humidity is usually close to water saturation (or higher in strong updrafts), otherwise the cloud droplets would evaporate quickly. Therefore it would be straightforward to choose water saturation, or a value close to that. Instead, the choice of RHi=100% explicitly includes humidities well below water saturation since temperatures are constrained to T<6°C. The authors indicate there may be also clouds with tiny or without any liquid in the LWP0 category, sometimes explicitly called ice clouds (e.g. page 11, line7). I suspect that such definitions may have a big impact on the results within this LWP bin, and indeed the properties seem to behave distinct in some ways. Therefore I strongly suggest to explore the impact of the threshold for humidity. Nevertheless, this kind of threshold may be problematic generally, since the saturation in clouds can be highly variable and is strongly tied to the structure of turbulent eddies, in particular with a small content of cloud droplets. So how meaningful is the profile of a single sounding which is supposed to represent intervals of 6 to 12 hours? Based on the manuscript I also cannot get an idea of how helpful the Mergesonde product is in addressing the problem of variability. Generally, to improve the clarity of the overall picture of mixed-phase, it might be beneficial to exclude ice-only clouds and introduce a lower threshold for LWP in the LWP0 category,

e.g., to exclude effects like sublimating small ice (see also below).

3)The discussion of results would benefit a lot by outlining the strategy of how the profiles of reflectivity, ice water content and fall speed will be interpreted. At this point it will be also helpful for the reader to explain what it means to use a reflectivity-weighted fall speed which actually represents the tail of the largest particles of the size distribution. For example (as on page 11, line 8), assume we compare two situations with the same IWC, but different Vf, where the latter difference would be caused only by the size distribution width. What is the measure of mean size and what would it mean in terms of the number difference? Otherwise, is the general assumption that the width of the size distribution is the same for both polluted and clean clouds and is it a good assumption?

Minor points

Page 2, line 25: suggest Barrow, Alaska

Page 3, line 12; Page 17 line 11: I recommend to rephrase “secondary ice mass growth” because it may be misleading and imply some connection to secondary ice production such as rime splintering. Personally I don’t see a need to call growth secondary, assuming that any initiating process would not be "growth".

Page 5, line 1: Does the analysis ensure that only cloud decks are analyzed with a cloud fraction of 100% for a period considerably longer than the 120 min window? The statement on page 9, last line, seems to imply there would be lateral entrainment at the cloud boundaries.

Page 7, paragraph 1: To get a sense for the analyzed data, the total amount or fraction of analyzed days would be interesting.

Page 12, line 13: This is one of the points when I stumble over ice clouds rather than mixed-phase clouds while bothe the title and abstract make different statements. The effect of ice sublimation would be a clear indicator of lacking water, so why include such

situations?

Page 16, line 5: With droplet freezing being the primary nucleation mechanism, the saturation as such is not the relevant variable, but temperature is.

Page 16, line 7: How reliable are such conclusions about ice number when the estimate of IWC may have a relative error of 100%?

Page 16, line 16: Hoffer 1961 may be an appropriate reference for the freezing behavior of drizzle or rain drops in which the aerosol content would scale, more or less, with drop mass due to collisional growth. However, cloud droplets mainly grow by condensation, and thus will contain the same amount of aerosol during growth. This is different from Hoffer’s method of producing particle-containing drops, while we expect that more aerosol surface area per drop would yield a higher chance of freezing.

Page 16, line 29: Due to the low temperatures investigated by Eastwood et al. 2009, it seems that this publication is hardly relevant for very most of the clouds summarized in Fig. 5. Also their humidities were mostly well below water saturation, while I am still assuming that the manuscript focusses on mixed-phase clouds.

Page 17, line 1: The statement on CCN is hard to understand, please rephrase.

Page 17, lin 14: typo: Yao

Figure 6: “December is assigned the value 0” might be showing up inadvertently, otherwise I do not understand.

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