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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In-situ aircraft observations of ice concentrations within clouds over the Antarctic Peninsula and Larsen Ice Shelf" *by* D. P. Grosvenor et al.

Anonymous Referee #2

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General Comment

This manuscript, describing a unique data set, is a welcome addition to the literature since there are few or no cloud observations reported for the Antarctica region. The measurements and analyses are all quite good. I recommend some modest re-organization that would improve readability, namely to revise section 2 as a broader "Methods" section so that it can include not only discussion of such issues as aircraft produced ice particles and ice particle data analysis, but to introduce the ice nucleation parameterizations that data are compared to later in the paper. Additionally, I think that some explicit discussion of expected Hallett-Mossop ice enhancement factors would be useful, to know if these are consistent with the measured ice concentrations attributed to primary nucleation and the peak values inferred to be caused by secondary ice formation. Otherwise, many improvements in organization were made already from the pre-press version of the paper, and it is reasonably well written, if a little long.

Specific Comments

Page 17300, lines 23-24: The statement here gives the impression that ice nucleation activity in bacteria is a common trait. This is not so, as it is quite rare. Please modify, easily done by removing the "contrary" part of the statement.

Page 17300, end of Section 1.1: It seems appropriate here to mention other recent studies to investigate biological ice nuclei from oceans, such as diatoms (e.g., Alpert et al., 2011, Phys. Chem. Chem. Phys., 13, 19882–19894; Knopf et al., 2011, Nature Geoscience, 4, 88–90)

Page 17303, line 6: Aircraft exhaust acting as IN, at modestly supercooled temperatures? I think one needs a reference to suggest such a possibility as in the realm of believability. There has never been any laboratory or observational evidence for IN produced by aircraft exhaust at modest supercoolings that I am aware of. I have only ever previously seen the alternate hypothesis involving the cooling around propeller tips, which seems clearly justified.

Page 17307, line 10: Is there a way to know if clouds were above the flight level at any time? I gather no, but this should be stated somewhere as a potential weakness in clearly identifying the source of ice crystals at different levels.

Page 17307, end of Section 3.2.1: There is mention here of the existence of supercooled rain in these clouds. This is a highly unusual observation that seems to beg better evidence than shown in Fig. 6c. Can it be pointed out which images are supercooled rain drops? Are there better examples available to support this claim?

Page 17310, lines 17-20: Is there any evidence for the seeds of the HM process? In

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other words, what was the initiation mechanism? Graupel? Frozen drops? Or can this not be determined?

Page 17310, line 21: Please take care to be clear as to what the “two regions” are. The two HM regions perhaps? The word “region” is used liberally throughout the paper, so sometimes it is necessary to be more explicit.

Page 17311, line 24: Suggest this sentence should end with “once primary ice has formed.”

Page 17312, end of Section 3.2: Is there a better word than “complicated” to describe the ice ultimate formation process? It seems somewhat chaotic or at least heterogeneous, driven by the availability of ice nuclei and the conditions for secondary ice formation, neither of which are always assured. Furthermore, it is at this point of the paper that one wishes for some discussion of the likelihood of producing the observed ice concentrations in the HM regime on the basis of the inferred primary ice crystal concentrations potentially triggering the process, and the cloud droplet spectra. Can any kind of quantitative statement be made absent a complete modeling treatment of the clouds, such as performed by Phillips et al. (2003, Q. J. R. Meteorol. Soc., 129, 1351–1371) for convective clouds?

Page 17313, lines15-16: In this case where no ice was observed, were the drop sizes requisite for HM? This seems relevant to document explicitly considering the discussion previous to this point. That is, primary ice is needed and appropriate cloud droplet conditions.

Page 17313-17314, Section 3.2.2: Here a cloud case is presented with stated top temperatures of only -$6\,^{\circ}\mathrm{C}$. Is this case an exception to the stated likelihood of primary ice sedimenting from above (temperatures below -$12\,^{\circ}\mathrm{C}$) in order to trigger the HM process in local regions, a point that is reiterated in the last few lines of the paper?

Page 17316, lines 10-11: The parenthetical statement could use “activated” before “ice

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residues," as one could confuse the fact that the noted study examined the ice nuclei from freshly activated tiny ice crystals versus the residues of larger cloud ice particles.

Page 17316, lines 25-26: Can it be clarified here if these measurements do or do not include periods in the marine boundary layer? It is unclear on the basis of discussion. It should also be made clear that the total CAS concentrations are used as the >0.5 micron surrogate concentrations. This discussion spreads over two pages, but could be greatly simplified by stating the assumptions used and then justifying them.

Page 17317-17318: I think it should be stated at the point of introducing the Cooper, Meyers, and Fletcher schemes that these have no inherent dependence on aerosol concentrations. Also, Figure 10 begs the question as to whether the D10 values are given for STP conditions (requires aerosol concentrations at STP as well), and if conversions for this factor have been made in all of the tabulated data as well. Has this been accounted for? As stated in my overview comments, these parameterizations could be introduced earlier in the paper.

Page 17321, lines 11-13: I suggest revising to note that not only IN profiles and cloud microphysical data are needed. Aerosol profiles and thermodynamic characterization of the cloud environment would be useful so that numerical simulations could be performed using IN parameterizations and consideration of mixing processes to better determine if there is consistency or not between predicted and observed ice formation in these clouds.

Page 17321-17322: The sentence straddling these two pages is the only quantitative statement regarding the efficiency of the Hallett-Mosspop process made in this paper. Just wondering if there is any way to determine the consistency of observations made in these flights with quantitative expectations for the HM process?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17295, 2012.