

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 #1

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General:

The paper is well written, with a good review of past literature. The data presented is new and important. I have a few suggestions for improvement.

1) While the DeMott et al. (2010) parameterization gives better agreement with the data than the older, non-aerosol based parameterizations, it should be noted that DeMott et al had very few IN data points $< 0.1 \text{ lit-1}$, and those were at temperatures between -23 and -35 (DeMott Fig. 2). This is substantially colder than the temperatures considered in this work. In fact, DeMott Fig. 3B shows that these points are outliers if using the parameterization developed, predicting higher IN concentrations than were actually

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observed. At any rate, the fact that there isn't matching data for comparison should be discussed. This could be true of the other, older, parameterizations as well.

2) p. 297, lines 8-10: It's an interesting question how radiatively important clouds are over Antarctica, due to its already high albedo. Since the authors invoke radiative effects for why their measurements are important, more systematic detail in this section would be useful. Included should be a summary of relative importance of longwave and shortwave forcing of liquid vs. ice clouds in this region, and likely net effects.

3) Most of the rest of the paper could be shortened and tightened up overall. There seems to be many meteorological details without discussing their significance, and statement of cloud locations, altitudes and LW and ice concentrations for each case, which might be better specified in the tables. Perhaps things could be reorganized into ice only (heterogeneous nucleation vs. Hallett Mossop), mixed-phase and liquid-only cases for contrast and comparison. At any rate, the detailed discussion of all the cases seemed somewhat repetitive and the authors should consider if things could be condensed, without losing important points.

Specific:

1) p. (17)299, line 11: What is the detection limit for soot by this satellite (in terms of concentration)? 2) p. 299, line 13: Was this “aerosol” the ice nucleating aerosol particles, or just general aerosol particles? 3) p.302, lines 16-17: For which probe(s) were shattered particles removed in the software, the CAS, CIP or both? 4) p.303, line 3-4: “very high concentrations” of what? Presume it's ice, but it should be specified. 5) p.304, lines 18-25: After reading twice, I think I understand what the authors are getting at, but this paragraph is confusing. Please rewrite more clearly. 6) p.305: Based on the figures, I assume the 2D images were examined to confirm that the very low concentrations of large particles were, in fact, ice crystals and not ultra-giant aerosol particles, but this should be specified. 7) p. 307-308 discussion: Was there any effort made to sample throughout the depth of the cloud so cloud depth and nucleation regions near

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cloud top might be observed? Knowing the location of the samples relative to cloud top and cloud base would be useful, but it seems this was only available sometimes. 8) p.308, lines 8-9: "significantly lower" than what? 9) p.309, line 12: What was the temperature of the low layer where the HM process was observed? I know it's discussed later, but it should be here for consistency with the rest of the discussion. 10) p.311, line 26-28: This doesn't seem to be stated correctly. How can a "lack of IN" "create the primary ice particles"? 11) p.312, line 14: I would delete the phrase "but also demonstrate that the process is complicated". Ice nucleation does have many aspects, but it doesn't really seem that complicated in this case, which you have documented well. 12) p.312, bottom: I find it odd that a cloud with no liquid water and ice conc of 0.04 lit-1 (40 m-3) would be optically dense enough to be detected by MODIS, particularly if the cloud was over an ice surface (unclear if this was the case). Am I wrong about that, or perhaps this is a case where most of the ice is smaller than the 112 μm detection limit used for the CIP? Or the MODIS data is from a different time period? Please explain or discuss. 13) p.316, line 25: Which channels were used for the CAS aerosol totals? The DeMott parameterization has an upper size limit. Did the smaller particles dominate the concentrations here?— in which case the upper cutoff wouldn't be important, but worth mentioning. 14) p.317, line 14: Upon investigation of the tables, it appears that the 0.1-0.4 cm-3 was the range of the actual aerosol concentration observations, but this should be stated. Using a range of concentrations to assess effects of measurement uncertainty is a good approach. 15) Table 1, mean temp column seems to have an error: -413.5

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