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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In situ measurements of cloud microphysics and aerosol over coastal Antarctica during the MAC campaign" *by* Sebastian J. O'Shea et al.

Anonymous Referee #1

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Overview

This manuscript presents an analysis of cloud and aerosol measurements collected during the Microphysics of Antarctic Clouds field campaign. The main focus of the analysis presented is on extensive airborne observations from 24 flights, primarily in conditions containing stratiform cloud layers, and these are supplemented by measurements made at a surface site. The observations show that the clouds are dominated by liquid water with variable (and typically low) concentrations of ice particles, suggesting that there are limited sources of primary ice nuclei that are active in the temperature range of these clouds. The ice particle concentrations tended to increase in the H-M temperature zone, suggesting that secondary ice production can play a role in these

clouds.

The main strength of the paper lies in the fact that there is a scarcity of in-situ observations of aerosol and clouds in the Antarctic, resulting in a very limited number of observational constraints that can be used to evaluate NWP and climate models in this region. The novel observations in this paper certainly have the potential to be useful for model evaluation studies and increase our knowledge of some cloud and aerosol microphysics parameters in the region. I do however think that some additional analysis of the data is required before the manuscript can be published in ACP (see comments below).

Main comments

1. Introduction: The authors give some background information on previous Antarctic INP measurements, but there is an absence of information on previous CCN measurements. The CCN are key to the liquid dominated clouds studied in this paper. The introduction should be expanded to include additional information on past results on Antarctic CCN data to put these new observations into context.

2. Section 2.1: The information on the meteorological and cloud conditions that were present during the observation period needs to be strengthened significantly. I realise that there were a lot of flights and that it is not straightforward to summarise this information in a paper, but the very short bit of text on page 6 (lines 1 to 5) is inadequate. Perhaps including additional information in a supplement would be worthwhile, such as a surface analysis chart, a satellite image and the back-trajectories calculated in section 3.5 for each case.

3. Vertical profiles of thermodynamic and cloud data for each aircraft flight would also be extremely useful to include, which again perhaps could be included in a supplement. This would enable the reader to put the microphysical measurements into better context with the cloud and meteorological conditions for each case. It would also be extremely useful for model evaluation purposes.

4. Data from all flights are composited and summarised as a function of altitude (Figures 4, 11, 14), yet presumably there is significant day-to-day variability in the cloud top and cloud base heights. The main problem with this approach is that it is difficult to disentangle changes in the in-cloud, above cloud and below cloud measurements (e.g. location of ice and aerosol particles) with variability in the vertical location of the clouds. Have the authors considered normalising the data relative to the position in the cloud (at least for single-layer clouds), which would then be more comparable to previous studies e.g. McFarquhar et al. (2007)? This, in combination with vertical profile plots (comment 3) would give the reader a much clearer picture of the cases sampled by the aircraft.

5. Page 9 line 22: The calculation of ice mass fraction does not include all particles. The ice mass is taken from the sum of the 2DS MI and HI categories, which cover particles larger than about 80 microns. The LWC is calculated from the CAS, which covers particles sizes less than about 50 microns. Furthermore, as discussed earlier in the manuscript the MI category may include large drizzle drops. The CAS would also be expected to measure small ice particles. Can the authors give some measure of the uncertainty in the calculated IMF that would result from defining liquid and ice water content in this way?

6. Figure 4: It would be better if the authors showed the IMF data as a box and whisker plot rather than plot a mean profile which is pretty meaningless, especially above about 1 km altitude. At these altitudes the mean profile shows values between 0.2 and 0.8, yet the majority of the data points look to have values close to 0 or 1 i.e. the clouds appear to be dominated by liquid drops or they are almost completely glaciated at these altitudes.

7. All figures where you include box and whisker diagrams: I would encourage the authors to ensure that there are the same number of data points included in each bin, otherwise the plots can be misleading. For example, when referring to Figure 9 on page 18 line 6, the authors state that “there was a trend to higher ice concentrations in

both updrafts and downdrafts”. It is unclear to me if the apparent increasing trend in the updrafts is simply the result of poor statistics. There appear to be very few data points with w > 2 ms-1, which are what I think the authors are using to justify the statement. If the bins were adjusted to include the same number of data points then I suspect that the trend may not be evident.

8. Page 13 line 16: Can the authors use the CIP data for the flights where the 2DS was not operating? This would be of interest as it would extend the analysis to colder temperatures, where there is arguably larger differences in the INP parameterizations.

9. Page 13 line 22: Again no clear trend is evident. There are very few data points at temperatures lower than -15 C and there is a lot of scatter in the data.

10. Page 13 line 24: Is the figure showing the histogram missing? This is key to demonstrate that the ice occurs in small patches in the liquid clouds.

11. Page 15: Why do the authors only sub-select data in the H-M temperature range where secondary ice production might be expected to be enhanced? It would be beneficial to also look to see if there was a relationship outside of this temperature zone e.g. to see if there is any evidence of primary ice being formed from the freezing of drizzle drops (e.g. Rango and Hobbs 1991).

12. Page 18 and figure 9: What is the mechanism by which glaciation would occur preferentially in downdrafts?

13. Figure 9: From the figure, it looks like the frequency of downdrafts measured by the aircraft is much larger than the updrafts. This was surprising to me and I would like the authors to confirm that there is no instrumental bias in the vertical wind measurement. Assuming that there is no bias, can the authors explain the higher frequency of downdrafts in the measurements?

14. Page 19: Is the implication that the columns observed at warmer temperatures are generated by secondary ice processes? If so make that clear to the reader and link to

figure 5 perhaps.

15. Figure 11: Can you include CPC data from the ground site? Also, how is number concentration derived from the aerosol scattering cross-section?

16. Page 21 line 13: Presumably any surface generated aerosol is inhibited from being transported above the boundary layer as there are likely to be strong thermodynamic gradients co-incident with the top of single-layer clouds? If the data were normalised and plotted relative to cloud/boundary layer top (see point 4), you may expect to see sharper vertical changes in the profile of aerosol. These would be smeared out in figure 11 as all flight data are simply plotted relative to altitude and the data are therefore averaged over different boundary layer depths. Also, if the boundary layer was not well-mixed, then this could also result in a drop-off in the concentration of surface generated aerosol with altitude. Can you examine the aircraft thermodynamic data to examine if this was important?

17. Page 23 line 8: Is there any evidence that the enhanced aerosol concentrations observed above cloud are being entrained into the cloud top? The suggested linkage between high aerosol concentrations above cloud top and the presence of clouds is rather tenuous.

18. Page 24 lines 6 to 10: This short paragraph on the WIBS data (which is not very informative) should either be expanded to link to the aircraft measurements or other ground based data, or removed given that a subsequent paper on this data is planned.

19. Page 26 line 1: Can you use back-trajectories to see if there is a different source region for the case where the aerosol hygroscopicity increased between 28 and 29 Nov?

20. Page 27 line 15: What about using the below-cloud aerosol concentration? This is more likely to be relevant, especially for cases were no elevated aerosol layers were observed to be in contact with cloud top.

21. Figure 14: Is the data in the top right panel suggesting that the source of the elevated aerosol concentrations are from the southern Ocean? Are these above cloud?

22. Figure 15: It is somewhat surprising that given that there is evidence of a correlation between aerosol concentrations and source region (Fig 14) that this is not apparent in cloud drop concentration. I would have expected increases in the aerosol concentrations to result in elevated cloud drop concentrations in these liquid dominated layer clouds.

23. The discussion section focuses solely on ice production in the clouds. Given that these are liquid dominated clouds the authors should also include some discussion on the liquid phase.

24. Page 25 line 27: Can this not be estimated given that the aircraft was doing vertical profiles?

Minor comments and technical corrections

1. Page 1 line 23: Additional clarification on what “key processes” means is required.

2. Page 1 line 27: Clarify that the size quoted is particle diameter.

3. Methods: It would seem more appropriate to introduce the NAME model in the methods section instead of in the results section.

4. Page 6 line 22: I don’t think the CAS part of a CAPS probe has anti-shatter tips. Please check. Also, was the CDP fitted with anti-shatter tips?

5. Are the aircraft altitudes relative to mean sea level?

6. Page 6 line 28: Can you put a measure of uncertainty on your IWC estimate that results from assuming the Brown and Francis mass-diameter relation?

7. Page 8 line 5: The MI data is only shown in one small subset of data (Fig 8).

8. Figure 2: Is this a flight average or an in-cloud average PSD? Rather than showing

one PSD, can you give a more quantitative measure of the agreement between instruments over all flights e.g. compare drop number and LWC from CDP/CAS, IWC from 2DS/CIP, aerosol concentration from GRIMM/CAS. It is also worth describing why you choose certain probes in your analysis e.g. CAS appears to be used preferentially for LWC over the CDP.

9. Page 9 line 10 to 14: Suggest removing if data is not shown/discussed.

10. Figure 4: What is the point of figure 4b? Is it just to zoom in on the data closer to the surface? If so, why cut-off the x-axis at 0.3 when there are many data points above this as shown in figure 4a. Consider removing the lower panel.

11. Figure 5: The continuous colorbar used does not adequately discriminate different flights. Suggest using an alternative way of plotting each flight or remove the colorbar.

12. Page 12 line 10: The authors state the reason is not clear but then go on to say that higher aerosol concentrations were observed which could explain the higher drop numbers. Do you see any clear correlation between sub-cloud aerosol and cloud drop number across the different flights?

13. Figure 10: The figure caption needs to include a lot more information.

14. Page 18 line 10: Replace 3:58 with 15:58 and 4:04 with 16:04 to be consistent with figure axis label.

15. Throughout the manuscript the authors use the abbreviation “ca”. Why not simply write “circa”? Or consider replacing with “approximately” or “about”, both of which are used in this context more extensively in the English language.

16. The authors switch between using cm-3 and scm-3 when referring to number concentrations of cloud and aerosol particles. Cloud drop number concentration data are shown in cm-3 (Figs 5, 6, 8, 12, 15), whereas aerosol number concentration data are mostly shown in scm-3 (Figs 11, 12, 14), except for CCN concentrations in Fig 13 which use cm-3. I would suggest being consistent in the use of units throughout and

using cm-3. It is the ambient aerosol number concentration on which the cloud drops form (not the value at STP) that is important for cloud drop number concentration for example.

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