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Interactive comment

Interactive comment on "The Microphysics of Clouds over the Antarctic Peninsula – Part 1: Observations" *by* Tom Lachlan-Cope et al.

Anonymous Referee #1

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Review of "The microphysics of clouds over the Antarctic peninsula- Part I Observations" by Lachlan-Cope et al.

Recommendation: Requires major revision before acceptance for publication

This paper presents observations of clouds over the Antarctic peninsula for two seasons, and notes that there were substantial differences in the cloud properties between the two years because air masses in one of the years were more likely to have passed close to the surface over the sea ice in the Weddell Sea. Consequently, the authors claim that the sea ice covered Weddell Sea can act as a source of both cloud condensation nuclei and ice nuclei. I am definitely of the opinion that this paper should be published because there are very few observations in this region, and such observations are sorely needed in order to improve climate and numerical weather prediction

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Discussion paper

models in this area. However, I did not find that all of the analysis presented in the paper to be sufficiently thorough, and recommend some additional analysis and dependencies that should be examined before the paper should be accepted for final publication in ACP. My concerns are more thoroughly documented below.

Major Comments

1. There should be a better description of the limitations and uncertainties of the measurements. For example, is there any potential problem with shattering on the tube of the CAPS artificially amplifying concentrations of particles? Was any effort made to identify and remove shattered artifacts? These can be important even up to 500 microns. The authors state that they ignore particles smaller than 200 micrometer equivalent diameter because they make minimal contributions compared to the number measured by the CAS. This seems a bit counterintuitive because it suggests that there is a range between 50 and 200 microns where there were few particles. Because of the dependence of the CIP probe sample volume on diameter, especially for smaller particles, even if there are very few counts the concentrations of such particles can be large because of a small and poorly defined sample volume. Thus, I am not sure how robust the analysis of particles over this size range actually is. The authors stated that the phase identification scheme was tested by examining sorted images by eye. But, how well can you determine the phases of the smaller particles by eye, especially from the relatively coarse resolution of the photodiodes of the CIP? Information about the shape of the size distribution measured by the CAS (flatter indicative of ice, peaked indicative of supercooled liquid) could also have been used. Was there any Rosemount icing detector that could also have helped identify the phase.

2. It is mentioned that the CAS is used to estimate aerosol concentrations outside of clouds, and that all flights were used even when no clouds were present. But, it should be noted that if there is going to be any interpretation on how aerosols affect cloud properties, probably only the data from when clouds were detected should be used. The other flights might have very different meteorological conditions and hence

their analysis is not relevant. Secondly, has any efforts been made to determine if the humidity varied between flights? The humidity may have an impact on the aerosol concentration as the aerosols can expand as they are humidified. This may have an impact on the interpretation of the results. And, finally it is assumed that the measured aerosol concentration will "bear some relation to the number of CCN and IN available." However, there is no simple 1:1 relation because the CCN also depend on the supersaturation in cloud, which in turn will depend on the vertical velocity. Further, since only about 1 in 1 million aerosol particles is an IN, there is no guarantee there will be a strong relation between aerosol number and IN. Therefore, these assertions about aerosols should be better justified or the appropriate caveats should be added into the text.

3. Averaging cloud properties over one-degree longitude bands represents a very coarse average of quantities that can vary over much smaller scales. How much variability of the cloud properties were there within that one degree band? The authors state that "it would be expected that the variability observed within each individual cloud would be less than the variability between different clouds measured on different occasions." This might not necessarily be true. Can some analysis be performed or presented to show that this is actually the fact?

4. I think there needs to be a better description of the meteorological context of the observations. Given some of my comments above, I think there could be a strong dependence of both the cloud and aerosol properties on the wind direction and speed. How much variation in longitude bands exists between flights? It would be interesting to see if there was also dependence on these wind speeds, vertical velocities, lower atmospheric stability, etc. It may also be possible that the background concentrations of aerosols in the free troposphere could also be affecting the cloud properties. The dependence on meteorological conditions may be much greater than the differences between 2010 and 2011 that the authors are highlighting. Similarly, Table 2 that gives the significance of aerosol differences between years and regions does not imply cau-

sation. Could some of these differences be caused by varying meteorological conditions, varying relative humidity or cloud vertical velocities, varying horizontal velocities and wind directions, etc.? It may be possible to investigate some of these relationships better with the data available. It might be possible to get this information if case-by-case studies were performed in addition to such coarse averaging.

5. I think that the analysis on the cloud properties could be presented more thoroughly. Was there any dependence on how the cloud properties vary with height or temperature or with the distance from cloud top to where the observations were made. In addition to showing the cloud properties, is there any information available on the coverage of clouds or the frequency of occurrence of different phases? Is there any variation in how the cloud particle sizes vary in the different conditions?

6. There are many speculative comments in the manuscript that should be either better justified or removed. For example, on page 6 the authors state that a peak in ice number concentration around -5C "is probably related to a secondary ice production process. . ..". While this statement may be true, it should be much better justified. For example, splinter production in the Hallett-Mossop process typically requires a 0.2 to 5 m/s impaction speed and the presence of droplets greater than 23 micrometer in diameter. Were such conditions present? Similarly, the discussion in the paragraph from lines 13 to 16 on page 6 is entirely speculative given that little information about the number of ice nuclei available at any temperature is available. In addition, for the analysis presented around line 30 on page 8, there needs to be presentation of other conditions (e.g., droplet sizes) to verify that the peak in ice crystal numbers is most likely due to the Hallett-Mossop process. I also feel that a lot of the discussion in the first full paragraph on page 9 is highly speculative and should be redrafted to clarify what is clearly known, and what are speculative comments regarding as to where primary and secondary production processes are occurring. Is it possible to present histograms or probability distribution functions of the different parameters to more clearly see the role of secondary ice crystal production processes? There are also many speculative

comments in the summary and conclusion that should be adjusted accordingly. There are too many words like “seems”, “cloud”, “might”, etc.

7. Another place the speculative comments are present is from lines 9 to 13 on page 8. The authors state that it “is POSSIBLE that sea salt . . . could be lifted into the air as blowing snow. . ..and this has been SUGGESTED as an efficient mechanism for getting sea salt . . ..into the boundary layer. This SUGGESTS that the increase in droplet number concentrations . . . COULD results from the increase in aerosol numbers.” Perhaps looking at the data on a case-by-case basis as well as looking at the averages would allow the authors to remove some of these highly speculative comments.

8. The analysis stratifies the data in a very averaged sense. In addition to this type of analysis, it would be very interesting to also stratify the observed cloud properties by cloud type since the sampling of types and altitudes may have varied on different flights and hence can affect some of the statistical analysis presented.

9. I recommend the use of ice nucleating particles (INPs) rather than ice nuclei (IN) to be consistent with currently excepted terminology.

Detailed Comments

1. Page 1, line 13: I would find the abstract more satisfying if significantly more was replaced with something more quantitative.

2. Page 1, line 25: I recommend labeling the locations of Palmer Station in Figure 1 as not all readers may be familiar with its location

3. Page 1, line 28: Recommend using the more standard terminology of ice nucleating particle (INP) rather than ice nuclei following the terminology adopted by Vali (2011).

4. Page 3, line 13: What does reasonably well mean quantitatively?

5. Page 3, line 22: Was any graupel present? It would seem that the circularity of graupel particles might approach those of liquid drops. In addition, how does the resolution

of the photodiodes affect the application of this scheme for smaller particles? Should the thresholds for circularity be dependent on the sizes of the particles?

6. Page 4, line 7: What is equivalent diameter? Is this an area equivalent diameter?

7. Page 4, line 12: What was the shape of the CAS size distributions? If it was peaked, this would offer more evidence that the particles were indeed supercooled water.

8. Page 5, line 22: suggest changing “are” to “were”

9. Page 5, line 23: suggest changing “are” to “were”

10. Page 5, line 26: Can slightly be quantified?

11. Page 8, line 19: Not enough evidence has been presented to definitively state that this is secondary ice production due to the Hallett-Mossop process.

12. Figures 3 and 4: The black shading is very faint and not be easy to see when the article is produced. Can you make the shading darker?

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