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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 #2

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The manuscript summarizes the characteristics of the Microphysics of Antarctic Clouds, or MAC campaign conducted in 2015 over coastal Antarctica and the nearby ocean regions. The measurements, comprising cloud and aerosol retrievals from both ground-based and airborne measurements, provide a compelling source of information for this region, that will most likely be of high interest to the atmospheric modelling community for example.

The Authors go further into analyzing the key features in the cloud and aerosol retrievals and the processes affecting them. Interesting features are revealed about the cloud ice mass fraction, ice particle types and the aerosol, even though conclusive explanations for many of the observed cloud features seem elusive. However, much of

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this can be rightfully attributed to the extremely challenging conditions as well as the so far quite low number of observations in this region. The descriptions of the instruments and retrieval techniques, as well as the presentation of data are for the most part adequate and very good.

My main concerns about the manuscript are related to the description of the surrounding conditions during these measurements. I do think the manuscript would greatly benefit from a bit more systematic description of the meteorological conditions as well as the structure of the clouds sampled during the campaign. This could potentially help with the interpretation of the results, many of which are now based simply on microphysical retrievals put together from all available flights.

Below, I will try to summarize these points with more specific comments, followed by minor and technical comments.

1. The manuscript does not provide very detailed information about the height and depth of the sampled cloud layers. If there are considerable differences in the altitude and depth of the cloud layer, this would imply differences in the cloud dynamics as well as in the large-scale meteorological setting from one flight to the next, and would therefore be worth a look to support the subsequent analysis. It is also rather difficult to follow whether the analyzed data (in general but also in the few flight specific examples) represent the cloud base, in-cloud or cloud top conditions. For example, the vertical profile of ice mass fraction given in Fig 4 is interesting information, but it would make it far more interesting, if that data could be put in the context of sampling level with respect to the vertical extent of the sampled cloud.

2. Would it be possible to consider the existence of multiple cloud layers for all of the flights? It is commented in Section 4.1 that seeding effects were not detected apart from the frontal cloud case. However, could the possibly overlapping cloud layers affect the radiation budget of the sampled clouds, that might impact mixing and perhaps entrainment and thus the cloud properties?

3. While the manuscript does consider the impact of airmass history on particle number concentrations in Section 3.5, it does not clearly outline how these shifts affect other aspects, such as large-scale meteorological forcing, which could impact the cloud properties. Generally, I think it would add great value to put these observations into context by including some more detailed information about the meteorological conditions during the campaign.

4. Related to the above, it would be interesting to couple meteorological data (e.g. wind speed) with estimates of the cloud altitude (as per the comments no. 1) in order to estimate the potential of blowing snow to affect the measured cloud properties. This possibility is considered in Section 4.1 but explicitly only for one flight. A more detailed evaluation of the prevailing conditions would allow a broader consideration of at least the possibility of blowing snow contribution also for other flights.

Other minor and technical comments

1. The notation scm-3 is used throughout the paper for the units of aerosol concentration, apparently referring to concentration defined in STP conditions, yet for cloud droplets the more commonplace cm-3 is used. I think the STP-related notation should be clearly defined and it should be made very clear where each of the notations are used to avoid confusion. However, I do think the best option by far would be to just use the same units (cm-3) for all concentrations.

2. Page 7, line 9: Please add a short definition for the circularity.

3. Fig 4, ”Radar altitude” vs ”altitude”, are the panels from different datasets? Please explain.

4. Page 13 lines 24-27: Histogram of the spatial extent of ice: which figure should I be looking at here? If it is missing, please consider adding a figure showing this.

5. Page 15, and fig 6: Why do you limit the analysis to the temperature range -8...-3 in this particular case? Please elaborate.

6. The terminology regarding Fig 7 is confusing: page 16, line 9 ”particle size distributions”, page 17, line 2 ”droplet spectrum” and just ”size distributions” in the caption. Please try to use more consistent terminology here so it’s easier to read and to know exactly what you refer to.

7. Fig 7: You don’t mention the dashed lines in the caption or elsewhere in the text. Please add an explanation for these or remove them from the figure.

8. Page 17, line 5: Could you please add some numbers to make a more clear case for the droplet depletion?

9. Page 19: Please consider adding a dedicated subsection for the analysis of the cloud particle images. Coming directly after the quantitative results on ice fractions and number concentrations it feels a little out of place to me.

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