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Interactive Comment

Interactive comment on "Observations of high droplet number concentrations in Southern Ocean boundary layer clouds" *by* T. Chubb et al.

Anonymous Referee #1

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Review of "Observations of high droplet number concentrations in Southern Ocean boundary layer clouds" by Chubb et al.

Recommendation: Requires minor revision before acceptance in ACP.

This paper presents some interesting observations from microphysical probes and the UHSAS aboard the NSF G-V aircraft during the HIPPO project from over the Southern Ocean (SO). Analyzing data from one case study of boundary layer clouds sampled during the wintertime, the authors show that the observed cloud droplet number concentrations and sub-micron aerosol concentrations observed in the southern most profiles were exceptionally high compared to expectations given background aerosol concentrations in this region. By combining their data with some chemistry observa-

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Interactive Discussion

Discussion Paper

tions and back trajectory analysis, the authors show that although there was some evidence of continental influence for the profiles, the data and trajectories are not consistent with the long range transport of continental aerosols explaining the observed cloud and aerosol concentrations. Thus, they infer that the high surface winds were most likely responsible for the high observed concentrations.

Given the paucity of observations over the Southern Ocean and the contradictions from some previous studies that surface winds were not necessarily correlated with sea salt aerosol production, I certainly believe that this paper should be published. Even though a fairly limited data set is presented in the study, the results are of sufficient merit that they should guide future studies and in fact, should motivate further observations in this region to better explore the relationship between cloud and aerosol properties. Nevertheless, there are a few changes which I suggest should be incorporated into the manuscript to better improve the flow of the manuscript and to better emphasize that the limitations in the data mean that that their results are consistent with the high surface winds causing the observed concentrations rather than proving that the high surface winds cause these concentrations.

I think the paper could be shortened and improved if Section 5 on the evaluation of uncertainties was incorporated into the sections of the manuscript where the relevant results were described earlier. When I was reading through the manuscript for the first time, I was wondering about some of the issues introduced in Section 5 and how they affected the presented analysis. If this material was explained (before or at the same time) as the relevant results, it will be much easier for the reader to interpret the observations and trajectories. Right now, for example, the basis of the calculation of the back trajectories are presented in Section 2.4, the back trajectories themselves described in Section 4.1, and the uncertainties in Section 5.3. There is necessarily some repetition in the manuscript because these calculations are repeated three times. Thus, the paper could be made much more tight if the back trajectories were only discussed in Section 4.1 (with maybe a quick introduction that they will be considered

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in Section 2). Similarly, the uncertainties in the CDP (Section 5.1) and UHSAS (Section 5.2) should be described in Section 2.2 so that the analysis of the flight level data in Section 3 can be better interpreted.

My second major comment can be best described by reviewing the final sentence of the manuscript, namely “we conclude that local production of sea spray aerosol through the high winds in the southernmost regions of the flight is the most likely explanation for these observations.” I think it would be better to state that the observations are consistent with the high winds causing the production of the sea salt aerosol, because this is really inferred from the data rather than establishing a relationship between these variables. I think this change in language is needed because the authors do admit that there is some uncertainties in the trajectory analysis.

One other thing that would be nice to add to the manuscript is a description of how often “the unusual winter-time microphysical conditions in the boundary layer over the Southern Ocean occur”. Other flights are given a cursory inspection to determine how often the pollutants might be present over the Southern Ocean, but can any comments be made about how often the gale force winds might be expected in the boundary layer?

In terms of the microphysics data and the uncertainties, I was surprised that there were no statements about how the bulk water content derived from the size distributions compared to that measured by a bulk water probe. I am assuming there must have been some sort of King or hotwire probe on the G-V. This would be a basic test that could help verify that the CDP is well calibrated (especially since some of the channel boundaries can sometimes be shifted). Can this be done and added to uncertainty analysis section?

Specific comments:

Abstract: “standard cloud physics payload”. Although there may be a standard payload for the G-V, in general there are so many different cloud physics probe that there really

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is no such thing as a standard payload. Recommend listing instruments.

Page 25509, line 14, first word should be clouds rather than cloud

Page 25510, line 9. There are some uncertainties with the depth of field in 2DC probes, especially for particles smaller than 125 micrometers (Baumgardner and Korolev 1997). This should be commented upon when discussing the uncertainties for this probe.

Page 25510, lines 19-21: Given this calibration was done in 2015 and the HIPPO observations were obtained earlier, is this relevant to the presented observations? Was this sample area used in the computation of the microphysical quantities? Make clear.

Page 25514, line 18 or so: How long of a horizontal distance was traveled during the time the profiles were obtained? To what degree could some horizontal in homogeneity in the clouds be affecting the observed profiles?

Page 25516, line 19: Would it be also useful to show/quote more of these maximum values as well as the mean values in the plots?

Page 25516, line 23-25: Could there be any influence (e.g., seeding) of the higher cloud layers on the lower cloud layers that could complicate the observed trends?

Page 25516, line 22: I assume that some of the observations of the UHSAS were obtained at different humidities, resulting in different amounts of growth of particles. Could this be affecting the comparison of concentrations at different flight legs? Were any corrections made for this?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25503, 2015.