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Discussions

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

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Response to comments from Anonymous Reviewer #1 for "Observations of high droplet number concentrations in Southern Ocean boundary layer clouds"

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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.

1 General Comments

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 observations 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.

> Thank you for taking the time to provide a thoughtful review of our paper. We will address the comments below point by point. There is a version of the revised manuscript with changes tracked since the original submission accompanying this response, and we indicate changes to the manuscript, with deletions indicated by red and additions indicated by blue.

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 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.

> Thank you for this comment. We respectfully disagree with your suggestion of working the discussion of the uncertainties into the results section. We have confidence in the data and were forthright with the discussion of the uncertainties in section 5, which we referred to in the description of the instrumentation and in the results section itself. Discussing the uncertainties alongside the results would be distracting, and the argument that we have presented is already complex enough. However, we agree that the discussion about the uncertainties in the back trajectories in particular was somewhat repetitive and there was an opportunity to make the manuscript more concise.
>
> In response, section 5.3 (Uncertainties in back trajectories), which was a general discussion of the uncertainties in the use of back trajectories, has been removed and the content from there was worked in to section 2.4 (Calculation of back trajectories). Overall this resulted in a reduction of about 250 words, so it was clearly worthwhile.
>
> We have left sections 5.1 and 5.2 in place, with minor modifications to ad-

dress various reviewer comments. There was no scope to significantly reduce the overall length of the manuscript by moving these elsewhere. In addition, both of these sections included details specific analysis that we wanted to keep separate from our results.

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.

> Thank you for this comment. Even though there are some uncertainties in our analysis, in part due to missing data and in part to the absence of instrumentation that would make the argument unequivocal, we believe that we have presented a strong case for our the hypothesis that the elevated aerosol and droplet concentrations are due to sea spray aerosol. However, comments from both Reviewer #1 and Reviewer #2 requested a dilution of the language, so we have changed section 7 (the only paragraph):
>
>> In this paper, ... *(snip)* ... . We conclude that these observations are consistent with the local production of sea spray aerosol through the due to high winds in the southernmost regions of the flightis the most likely explanation for these observations. ... *(snip)*

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?

> Thank you for this comment. The occurrence of strong winds over the Southern Ocean has received some attention in the literature, which we do address (e.g. references to Korhonen et al., 2010; Hande et al., 2012a). In addition, we specifically commented in the original manuscript (P. 25515, lines 8–10) on how often gale force winds occurred at Macquarie Island, where there is a weather station operated by the Australia Bureau of Meteorology. Upon reflection, this sentence would be better placed in the discussion, so we have made the following changes to the manuscript:
>
> 1. Edited section 3.1 (final sentences of this section):
>
>   The boundary layer wind speeds ... *(snip)* ... Using a log scaling law to translate this to surface conditions, the ten meter winds would have been in the range of 17 to $20\,\mathrm{m\,s^{-1}}$. Gale force winds speed gale force ($\geq 17\,\mathrm{m\,s^{-1}}$) occur regularly over the SO; weather station data from Macquarie Island, which is nearby in the storm track region, had half hourly average surface wind speeds greater than this on about 15% of days between 2008 and 2011 .
>
> 2. Edited section 6.2 (moved the discussion from 3.1 to here):
>
>   This result is of interest ...*(snip)* ... over the SO. Strong boundary layer Gale force winds, such as those encountered in HIPPO-4 RF06, are a regular occurrence occur regularly over the SO. ; weather station data from Macquarie Island, which

> is nearby in the storm track region, had half hourly average surface wind speeds greater than this on about 15 $\%$ of days between 2008 and 2011. Moreover, ...*(snip)*

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?

> Thank you for this comment. There was indeed a PMS-King type hot wire probe installed on the GV, and it was operational during HIPPO-4 RF06, and naturally compared the values derived from this instrument with those from the CDP. The two values were highly correlated ($R = 0.98$) but initially the CDP values were approximately twice that of the King probe. This was in spite of the standard calibration using glass beads during the HIPPO-4 campaign and subsequent post-processing Romashkin (2012). This was highly concerning for us and the resultant investigation led to the beam mapping of CDP #16, which was the one used in HIPPO-4. The beam mapping is a relatively new technique which evaluates the true sample area of the specific instrument (as opposed to the "theoretical" sample area which had been used previously) with water droplets. This was performed by DMT, the instrument manufacturer, who were intentionally kept unaware of the discrepancy that we had identified.

The original data were processed using a theoretical sample area of $0.240\,\mathrm{mm}^2$, but the subsequent beam mapping of CDP #16 showed that the sample area was $0.309\,\mathrm{mm}^2$.

We recalculated the droplet concentration and liquid water content using the measured beam area for this paper.

As mentioned in the original manuscript, we evaluated the CDP liquid water content against theoretical values for profile 1, which we believe to be close to truly adiabatic (see figure). We used a parcel model (conserved θ_e) initialized near cloud base to calculate a theoretical LWC profile. If the values in the shaded region—where there appears to be entrainment of dry air from above—are excluded, a very good match with the CDP data is obtained ($\rho_{L,CDP} = 1.01\rho_{L,\mathrm{Adiabatic}}$). The King probe appears to be measuring about 68% of the adiabatic amount according to this analysis, although this is not outside the range of possibilities for stratocumulus clouds (see Boers et al., 1996).

Finally, we note the comments of Romashkin (2012) pertaining to the use of the PMS-King probe data on the GV during HIPPO-4:

> Significant improvements have been made to the King probe processing code to better quantify changes in the heat transfer related to the changes in the airspeed. However, rapid fluctuations in the PLWCC baseline are still observed in the PLWCC that are not realistic. Please compare the PLWCC data with PLWCD_* that is calculated from the cloud droplet probe to assess the quality of the liquid water data from the King probe.

In conclusion, there are some uncertainties about the LWC data from both the CDP and the PMS-King probe on the GV and the true value was probably somewhere between the two. Assuming for a moment that the PMS-King probe were accurate, there could be two hypotheses to explain the difference in the CDP observations:

1. The CDP sample area was even larger than the beam mapping analysis suggested, in which case then the cloud droplet number concentrations would have

been affected too, resulting in a mean CDNC for profile 1 of about $32\,\mathrm{cm}^{-3}$. This is quite low but not impossible given previous observations.

2. There was a binning error resulting in systematic mis-sizing of the droplets. This could affect $\rho_{L,CDP}$ without affecting the CDNC. Neither of these possibilities change our conclusions in any way, because in the case of (1.), we would still need to explain the factor of five (or more) difference in the CDNC for profiles 1 and 4, and in the case of (2.), the CDNC data are unaffected. Following our intensive quality control of the CDP data we elected to present these in order that our results could be fully reproducible. However, we agree that it is worth mentioning that these analyses have been performed in the manuscript, in such a way that it does not distract from our message.

We have made the following changes to the manuscript:

1. Item added to section 2.2: to describe bulk water measurements:

    - *PMS-King "hot-wire" probe.* Total cloud liquid water content can be directly measured by exposing a temperature-controlled element to the flow outside the aircraft (King et al., 1978). Within cloud, the power required to maintain a constant temperature is compared to that required in clear air to derive $\rho_{\mathrm{L,King}}$.

2. Sentences added to section 5.1:

    The accuracy of the CDP is typically stated as $\pm 10\,\%$ due to uncertainties in the true sample volume and in the sizing of small particles through Mie scattering. However, the PMS-King probe consistently showed about 0.68 of ρ_L from the CDP. Using a parcel ascent ... *(snip)* Even if the CDP did significantly overestimate ρ_L—which we believe to be unlikely—it may have also overestimated N_{C} by the same fraction,

    depending on the cause of the error. However, this would ultimately have little impact on our conclusions, because it would still be necessary to explain a factor of five increase in N_{C} between profiles 1 and 4.

3. New bibliography item added for King et al. (1978).

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

> We'd prefer to leave the instrument list to section 2.2, but accept your comment about the terminology. The leading sentence of the abstract has been changed as follows:
>
> Data from the standard cloud physics payload Cloud physics data collected during the NSF/NCAR High-performance Instrumented Airborne Platform for Environmental Research (HIAPER) Pole-to-Pole Observations (HIPPO) campaigns provide ... *(snip)*

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

> Accepted.

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.

Accepted. Please note the following changes in the manuscript.

1. Changes to section 2.2:

   - *Particle Measurement Systems (PMS) 2-D Cloud Imaging Probe (2DC).* Precipitation particles larger than about ... *(snip)* ... individual particle images. Here, as for most applications of the 2DC, we only use This type of probe is susceptible to uncertainties in depth-of-field for particles with diameters less than $200\,\mu\mathrm{m}$ (Baumgardner and Korolev, 1997), but we made no specific correction for this other than only using particles with diameters ... *(snip)*

2. New bibliography entry for (Baumgardner and Korolev, 1997).

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.

Thanks for this comment. The beam area is not expected to have changed in the interval between HIPPO-4 and the subsequent beam mapping. The beam mapping technique is relatively new and providesthe best esttimate of the true sample area that is available. As for the ρ_L and N_C data, we recalculated these ourselves using the new sample area. We have made this clear in the manuscript by adding a sentence to the item in section 2.2:

- *Droplet Measurement Technologies (DMT) Cloud Droplet Probe (CDP).* The CDP operates by ... *(snip)* ... beam mapping by the manufacturer in June 2015. We recalculated ρ_L and N_C using the updated

sample area. Further discussion of the uncertainties associated with this instrument is provided in Sect. 5.1.

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 inhomogeneity in the clouds be affecting the observed profiles?

This is always a problem with aircraft data, and there is a trade-off to be made between artefacts due to high vertical speeds and inhomogeneities due to horizontal distance travelled. For HIPPO-4 RF06, the profiles were performed with a vertical speed of $7.5\,\mathrm{ms}^{-1}$ at altitudes above $600\,\mathrm{m}$, and $2.5\,\mathrm{ms}^{-1}$ below this, at a true air speed of $130\,\mathrm{ms}^{-1}$. The distance covered was about $38\,\mathrm{km}$ between $1500\,\mathrm{m}$ and $160\,\mathrm{m}$ (the lowest altitude reached) for each profile. However, as mentioned in the original manuscript, there weren't significant differences between the ascending and descending profile data, except in profile 2 where there was no cloud sampled in the ascending profile. We do not anticipate major effects from this factor, but have highlighted these details in the revised manuscript. Changes:

1. Inserted sentences in section 2.1 (final paragraph) to describe air speeds and vertical motion:

   The aircraft performed four descent/ascent profiles ... *(snip)* ... cloud top conditions are provided in Fig. 3. The mean true air speed varied during the profiles, but it was consistently about $130\,\mathrm{m\,s}^{-1}$ at altitudes below $1500\,\mathrm{m}$ a.s.l. (well above the boundary layer). The vertical speed of the HIAPER was about $7.5\,\mathrm{m\,s}^{-1}$ for altitudes above $600\,\mathrm{m}$ a.s.l., and $2.5\,\mathrm{m\,s}^{-1}$ below this. A total distance of about $38\,\mathrm{km}$ was covered between the lowest level and $1500\,\mathrm{m}$ a.s.l. for each profile. Conditions were quite varied between ... *(snip)*

2. Edited first paragraph of section 3.1 to highlight typical concerns with aircraft profiles:

   Figure 4 shows thermodynamic observations from each of the descending profiles from the $1\,\mathrm{Hz}$ dataset. The slantwise nature of aircraft profiles leaves open the possibility of horizontal inhomogeneity limiting the analysis, but the values for the ascending profiles were not ... *(snip)*

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?

The maximum values for N_C were included for profiles 3 and 4 but not for profiles 1 and 2 simply because there was much more variability in profiles 3 and 4. The maximum values are particularly important for profile 4 because N_C was correlated with ρ_L for this profile only, indicating that entrainment was important. We are reluctant to introduce more values in to the descriptions for profiles 1 and 2 because they could be distracting, and the values could be read from the graphs if readers are interested.

We have made no specific changes to the manuscript in response to this comment.

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?

If you are referring to the cloud layer above $2400\,\mathrm{m}$ a.s.l. in profile 3, we think that this is highly unlikely. There was no evidence of any precipita-

tion particles above the boundary layer cloud top, and there was a vertical displacement of nearly $1500\,\mathrm{m}$ between the cloud layers in this instance.

We have made no specific changes to the manuscript in response to this comment.

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?

This issue was also raised by Reviewer #2 and is addressed more thoroughly in our response to their comments. In summary, due to the combined effect of decelerating the air, anti-ice heating and optics block heating, it is fairly safe to assume that the observed particle sizes are close to dry sizes. There is precedence for this in the literature, and we have made this more clear in the manuscript with the following changes:

1. Added sentence to UHSAS item in section 2.2:
   - *DMT Ultra High Sensitivity Aerosol Spectrometer (UHSAS).* The UHSAS measures sizes of aerosol ... *(snip)*. Due to the combined effect of electrical anti-ice and internal heating, and adiabatic heating of decelerated inlet air, we assumed that the measured particle diameters were close to their dry diameters (e.g. Blot et al., 2013; Kassianov et al., 2015). ... *(snip)*
2. Bibliography item added for Kassianov et al. (2015).

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figure-1.png

Fig. 1. *Data from profile 1 plotted against altitude (m a.s.l.). Left: LWC derived from various instruments (colored lines), with adiabatic (solid gray) and 0.68 times adiabatic (dashed gray). The shaded region indicates where the cloud was sub-adiabatic, probably due to entrainment from above at the boundary between two overturning cells. Right: temperature and dew-point temperature (colors) and lifted parcel (gray lines).*