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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.

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Interactive Comment

Response to comments from Anonymous Reviewer 3 for "Observations of high droplet number concentrations in Southern Ocean boundary layer clouds"

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1 General comments

I have reviewed the manuscript "Observations of high droplet number concentrations in Southern Ocean boundary layer clouds" by Chubb et al. The work presents results from a small subset of HIPPO flights and examines the microphysical properties of boundary-layer clouds from a small set of observations made near Tasmania. The work highlights that a wide range of cloud droplet number concentrations were observed during these flights. The authors hypothesize that the large number of drops could be related to either anthropogenic emissions or sea-spray aerosol. Based on their analysis of model back trajectories the authors argue that the most likely cause of the large numbers of particles is the generation of sea-spray aerosol associated with very strong winds. It is important to note that this finding is not based on direct observations, but rather on the elimination of a number of other potential sources of the particles. Overall, the manuscript provides a clear and concise description of the results, and I feel that the manuscript would likely be acceptable for publication in Atmospheric Chemistry and Physics with some minor changes. That said, the study would be much more convincing (and useful to the community) if additional data sets were available to give an idea of the importance of the contribution of sea-spray aerosol in a larger sense, if more measurements of aerosol chemistry could be used to help highlight the nature of aerosol that is observed, and application of a more detailed chemistry model to better understand the aerosol sources (including the potential role of mineral dust). I have provided some more detailed comments below.

Thank you for taking the time to review our manuscript. The main points raised in your summary above (underlined by us) are that additional data would have had the potential to make our conclusions much stronger, which we of course completely agree with. This point was also raised by Reviewer 2, who additionally requested that we soften our conclusions as a result. ReviewerÂă1 also requested a softening of our conclusions based on the ambiguities relating to aerosol composition. We have made two several changes to the manuscript as a result of those requests which should address your comment, including a recommendation for aerosol chemical composition measurements on future flights over the Southern Ocean. Please note that there is a version of the revised manuscript with changes tracked since the original submission accompanying this response.

1. Changes to Section 1 (last paragraph) to highlight that we are eliminating alternative hypotheses:

   This paper focuses on ... *(snip)* ... with the approach of

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a strong cold front. Our Direct evidence for this hypothesis—in the form of observations of aerosol composition—is not available, so our objectives are firstly to verify and analyze the available in-flight microphysics observations, which were not intensive due to their secondary importance for the HIPPO missions, and secondly to ... *(snip)*

2. Changes to Section 6.1 (last paragraph) to concede that other aerosol sources cannot be completely ruled out:

   While N_C values of ... *(snip)* ... observed by the UHSAS, which probably includes most of the CCN, was produced locally. We While alternative sources for the CCN cannot be completely ruled out without compositional analysis of the aerosol,we showed through ... *(snip)*

3. Changes to section 7 to soften final conclusion:

   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)*

4. Changes to section 7 to recommend inclusion of additional instrumentation on future flights:

   In this paper, ... *(snip)* ... most likely explanation for these observations. In order to reduce ambiguities such as those discussed in this paper, we strongly recommend the inclusion of aerosol chemical composition measurements for future cloud physics observational missions over the Southern Ocean.

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2 General Comments

Page 25505, line 7. I would suggest adding the word “observed” after “droplet sizes.”

Accepted; please see tracked changes document.

Page 25505, line 13 (and other locations). The use of “high” and “low” to mean “large” and “small” could be confusing to the reader. I would suggest changing the occurrences with large and small to be more accurate.

Thanks for this comment. We would prefer to retain our terminology in this case because N_{C} and N_{U} have units of concentration (cm^{-3}). For this example, if we substituted words for our symbols and used “small” instead of “low”, it would read as follows: “Droplet number concentration was found to be small,” which sounds odd.

Page 25505, line 29. ““Provide” should be “provided”.

Accepted; please see tracked changes document.

Page 25508, line 5. I would suggest adding “associated” after “low-level winds”.

Accepted; please see tracked changes document.

Page 25508, line 16. It would be helpful, at some point in the document, to indicate the airspeed of the aircraft. That would make it easier to understand the impacts of the sampling speed on the results.

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Accepted; this was addressed in response to a comment by Reviewer 1. The changes to the manuscript were:

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

    The aircraft performed four ... *(snip)* ... 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 ... *(snip)*

Page 25509, line 18. Additional detail about the ascent/decent profiles would be helpful. For example, what was the approximate ascent/decent rate of the aircraft? Were these profiles designed to overlap each other?

> The design of the profiles was essentially to sample the atmosphere from about 8000Âăm down to near the surface during a series of flights that constituted a global transect (see Wofsy 2011). The aircraft was either climbing or descending almost continuously so there was no overlap in the profiles. Details of the descent rate were included in response to comments by ReviewerÂă1, and are included in the change for the previous item (see above).

Page 25513, line 20-25. The text argues that the potential temperature profile shown for profile 2 is more complex than that for profile 1, but that isn't clear to me from the figure unless the authors are referring to the buffer layer. Perhaps the small spatial extent of the figure hides the relative complexity? The text also states that large values of CDP liquid water content are consistent with a cumulus cell. Given the aircraft speed, how large would this cell have to be to provide the continuous profile shown in the figure? Is that reasonable?

Fig. 1. *Time series data for profile 2. Blue line: altitude (left axis). Green lines: 1Hz and 11-point smoothed LWC.*

Thank you for this comment. The buffer layer is precisely the complexity to which we refer. We agree that there is a lot of information in figure 4, but the main point is that the change in specific humidity (and also NU, but this discussion follows later) occurs at a different level to the θv increase (temperature inversion). It seems that we neglected to include this point in that paragraph, so we have added it in the revised manuscript (see below).

The second point was about the size of the putative cumulus cell. We mentioned that there was a relatively consistent ρ_L in a layer about 250 m, which is about right based on our Figure 4. When viewed as a time series and smoothed, as in Figure 2 of this document, there is a local maximum near $t = 18865$. The slope of this feature drops of sharply after about $t = 18870$, even though the descent rate decreased dramatically at about this time, suggesting that the aircraft is indeed exiting a convective feature through the side (nearly horizontally) rather than through the base. A reasonable estimate for the traversal time of the convective core might be 10 s (1300 m), which is not unreasonable for this type of feature.

The 1 Hz data appears to have some periodicity in this region with 6 peaks spaced by roughly 5 s (650 m), but they are not clearly separated by clear

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air. There may well be some complexity to the feature (entrainment and/or multiple cores) that we do not attempt to explain.

1. Changes to section 3.1 (paragraph 2). Description of change in humidity with altitude.

   Profile 2 shares a number of ... *(snip)* ... intermediate layer of about 200 m, with in which the specific humidity (q_v) remained similar to the in-cloud values. There was a weaker θ_v increase at 1320 m a.s.l., which was coincident with a sharp drop in q_v to nearly zero. At cloud top ... *(snip)*

2. Changed section 3.1 (paragraph 2) to improve our description of the convective feature:

   Profile 2 shares a number of ... *(snip)* ... cloudy layer. Below thisthere was a layer about 250 m deep with relatively consistent values of $\rho_\mathrm{L} \simeq 0.25\,\mathrm{g\,m^{-3}}$, which we interpret , the 10-second smoothed ρ_L (not shown) reached a minimum of $0.25\,\mathrm{g\,m^{-3}}$ before increasing briefly to $0.35\,\mathrm{g\,m^{-3}}$ and then dropping rapidly as the aircraft continued to descend. We interpret this feature as a cumulus cell rising into ... *(snip)*

Page 25516, line 7. It would be good to add a note to the caption of Figure 5 about the offset applied to some profiles.

Accepted; this was done in the response to a comment by Reviewer 2. We have also removed the offsets from Figures 9 and 10 to improve clarity.

Page 25517, line 3. The text states that there is a peak in N_U near cloud top, but that isn't clear to me from the figures.

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Apologies; the dashed line (representing in-cloud values, which are absolutely not to be trusted due to droplets splashing on the inlet) disappeared from this panel somehow. This has been rectified in figure 5 of the new manuscript.

Page 25517, line 27. You might want to add "thermodynamically" before the word "stable".

Accepted; please see tracked changes document.

Page 25522, line 11. The text in this paragraph states that HIPPO-4 RF06 is not a good example of a pristine flight nor a polluted one, but early in the section, Profile 1 is descried as "very clean". This description appears to be a bit inconsistent.

Thank you for this comment. It was somewhat paradoxical that the profile closest to the continent was in many ways the cleanest. Upon reflection, we find the sentence describing profile 1 as "very clean" is somewhat redundant so we have removed it from the revised manuscript. Please see the tracked changes document.

Page 25523, line 9. The text states that the trajectories from 500 and $1500\,\mathrm{m}$ are very similar, and if dust is a major issue than the aerosol loading should be the same (or at least close in value). Is this due to deeper boundary layers (and associated vertical mixing) over Australia?

We showed that trajectories arriving at 500 and $1500\,\mathrm{m}$ for profile 3 actually had similar histories both spatially and vertically (at least for those that originated over land). Many of the members for the $1500\,\mathrm{m}$ ensemble were close

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to the surface when over land. Therefore, as we mentioned, the aerosol loading should be similar for these levels at the location of profile 3, but it was not (suggesting, again, a local aerosol source). Presumably the mixed layer would have been deeper over the continent than over the ocean, but this is speculative and not necessary for our argument. We have not made any specific changes to the manuscript in response to this comment.

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