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

Anonymous Referee #2

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The paper presents data from one HIPPO flight over the Southern Ocean where high aerosol number and cloud droplet concentrations were observed in the boundary layer. Based on 3-day back trajectories, concentrations of CO and BC, and high wind speeds, the authors conclude that the enhanced concentrations were likely due to sea spray aerosol. However, direct evidence for this conclusion, e.g., aerosol composition measurements or thermal analysis, is lacking. In addition, as pointed out below, the CO and BC supporting data are missing at some altitudes making the case for non-combustion sources less certain. The only direct aerosol available for assessing the contribution of sea spray aerosol to the high number and cloud droplet concentrations is the number size distribution measured with the UHSAS. It should be possible to apply a lognormal fit to these data and estimate the number concentration of sea spray aerosol as

was done by Modini et al., JGR, vol. 120, doi:10.1002/2014JD022963, 2015. Based on size distributions generated in a wave tank (Prather et al., PNAS, 2013) and the canonical number size distribution of sea spray aerosol defined by Lewis and Schwartz (2004), Modini et al. fit a sea spray aerosol mode with the constraint of a 200 nm +/- 30% mean diameter and a geometric standard deviation between 2.5 and 3. They then integrated the number concentration within that mode to estimate the number concentration of sea spray aerosol. For comparison to that analysis, the UHSAS data in this paper would have to be shifted to the same RH. I assume the data shown in Figure 6 are at ambient RH. If they are at ambient RH and they are shifted to a dry diameter, the peak diameter of the mode would be smaller than previously reported SSA size distributions. This analysis would help to assess whether the measured aerosols were primary marine aerosol. The paper should be published because there is a lack of in-situ aerosol and cloud data over the Southern Ocean. That said, the above size distribution analysis should be performed to assess the potential contribution of sea spray aerosol to the total number concentration. In addition, given the lack of direct evidence and the uncertainties in the analysis (e.g., back trajectory calculations, UHSAS data, missing CO and BC data), the conclusion should be softened to "sea spray aerosol is a POSSIBLE explanation for these observations". It would also benefit the community if a strong recommendation for aerosol chemical composition measurements on future flights over the Southern Ocean were included in the conclusion section. Additional issues to be addressed are listed below.

p. 25511, Section 2.3. These measurements quantify refractory BC and CO. It is mentioned that they do not measure mineral dust. Long range transport of anthropogenic sulfate and organic carbon would also be missed.

p. 25525, Lines 11 – 14: It is stated in the text that during profile 4, Nc was in the range of 6 – 10 cmˆ-3 with particles of mean diameter 6 – 7 um. Based on Figure 5, below cloud Nc for profile 4 was around 450 cmˆ-3 with a mean diameter of 40 um. Am I reading the Figure incorrectly?

ACPD
15, C8645–C8647, 2015

p. 25527, lines 18 – 25: It is stated that “For profiles 3 and 4. . .the most likely signature of anthropogenic influence was well above and decoupled from the boundary layer. . .” But there are no BC data for these flights below ∼1200 m (at least according to Figure 9). And CO is not significantly lower (65 and 70 ppbv for flights 3 and 4 at altitudes < 1000 m) than the air masses tagged as anthropogenic during Flight 2 (∼65 ppbv at 4000 – 5000 m).

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