COMMENTS RECEIVED FROM REVIEWER #1

General comments

This manuscript uses aircraft in situ observations of developing cumulus clouds over the Amazon to explore the connection between control variables (here the concentration N_a of aerosol particles entering clouds from below, the height *H* above cloud base, and the updraft speed, *w*) and cloud microphysical properties (cloud droplet concentration, effective radius and spectral shape) at levels above cloud base. The connections are determined by an approach that attempts to isolate the effect of a particular control variable by holding the other control variables fixed (partial derivative approach). This is carried out in practice by binning the data into three dimensional bins of N_a, *H* and *w*. The findings indicate that the primary controls on effective radius are *H* and N_a, and that the primary control on N_d is N_a with *w* also positively influencing N_d. *LWC* is mainly controlled by *H* and *w*. These results seem to make physical sense. There are new findings about factors controlling the drop size shape that will be of interest to the community. The manuscript is relevant to the cloud community, although it is not clear what modelers would do with the results. I find the manuscript suitable for publication in Atmospheric Chemistry and Physics, and offer some suggestions for revision.

Authors answers

We would like to express our gratitude for the anonymous Reviewer #1 for taking the time to review this manuscript. Your suggestions are invaluable and very helpful. We will try our best to address all of them.

Regarding your concerns on how the results can be of use to modelers, here are our suggestions (which we will try to make clearer in the text). Our sensitivity results (i.e. Tables 2-3) can be used for direct comparisons with model results. Tropical clouds in general, or Amazonian clouds in our case, are still poorly represented by models. One of the common issues is the representation of the precipitation daily cycle, where rainfall tends to occur earlier in models compared to observations. We believe one reason for that is the misrepresentation of the DSDs that can lead to artificially high efficiency in rain formation. Therefore, model runs can be performed in order to assess the factors that control DSD formation and comparisons can be made with our results as benchmark. The analysis of the ε and Λ parameters can be especially useful in that regard. In that case, however, it should be beneficial to consider the more detailed results shown in Figure 3. This figure shows that aerosols can induce DSD broadening only close to cloud base, preferably under high *w* conditions. Higher in the clouds, increased aerosol loading leads to DSD narrowing. The variability of DSD shape with altitude is pronounced in cleaner clouds, decreasing with increasing N_a. The updraft effect on DSD shape is secondary, but enhanced updrafts may lead to narrower DSD in clean clouds given the limited aerosol availability. Good models should be able to reproduce such details in order to generate better forecasts. Therefore, we believe our results can be of use in that direction, by providing specificities of Amazonian clouds that models should aim to reproduce.

The last paragraph (right before the acknowledgements) was changed to reflect this feedback:

"The results presented here can potentially be used to validate and derive new parameterizations in numerical models, which usually fail to correctly represent Amazonian convective clouds. One common issue of the models is the representation of the precipitation daily cycle, where the modelled rainfall tends to occur earlier than in the observations. One possible reason for that is the misrepresentation of the cloud DSDs that can lead to artificially high efficiency in rain formation. Therefore, model runs can be performed in order to assess the factors that control DSD formation and comparisons can be made with our results as benchmark. The analysis of the ε and Λ parameters can be especially useful in that regard. The results presented here detail several aspects of the Amazonian clouds and their relation to aerosol and thermodynamic conditions. For instance, it was shown that aerosols can induce DSD broadening only close to cloud base, preferably under high w conditions. Higher in the clouds, increased aerosol loading leads to DSD narrowing. Additionally, DSD broadening with altitude is pronounced only in clean clouds, where the collection processes are efficient. The result is growing ε with altitude, while this parameter remains relatively constant with H in polluted clouds. Good models should be able to reproduce such details in order to generate better forecasts. Therefore, we believe the results presented here can be of use in that direction, by providing specificities of Amazonian clouds that models should aim to reproduce".

Specific comments from Reviewer #1

1.
    a. **(Question)** The finding that N_d is strongly correlated with N_a differs from studies in shallow broken cumulus (e.g. Vogelmann et al. 2011, BAMS, Fig. 10) where N_d does not appear to be strongly correlated with N_a and LWP decreases with N_a. Some discussion of contrasts with prior work would put this work into the context of previous work.
    b. **(Answer)** Thank you for the reference. We specifically address comparison with other studies between P8 Line 29 and P9 Line 11. Nevertheless, your reference prompts an interesting question regarding the sensitivity results and meteorological conditions. We added the following sentences starting at P9 Line 17:

       "The meteorological and cloud morphology conditions in the Amazon also seem to enable the high sensitivity values found. A previous study by Vogelmann et al. (2012) found relatively invariant N_d as function of N_a. Beyond instrumental and methodological differences, this study also focused on shallow (200 m to 500 m thickness), broken clouds with weak updrafts over Oklahoma. This type of cloud favors the entrainment mixing feedback, where polluted clouds tend to have lower LWC because of enhanced droplet evaporation. The differences between the results shown here and the study of Vogelmann et al. (2012) suggest that the entrainment mixing process is not dominant over the Amazon. Possible reasons include abundant water vapor, thicker clouds, stronger convection and updrafts, and low vertical wind shear. High humidity of the surrounding air induces weaker LWC and N_d depletion by the entrainment mixing process (see, for instance, Korolev et al. 2016) because of slower evaporation. Stroger convection induces deeper clouds that have a relatively low area-to-volume ratio as compared to the clouds reported in Vogelmann et al. (2012). Therefore, the entrainment at cloud edges are not as dominant. Low area-to-volume ratios are also favored by the weak vertical wind shear typical of tropical regions. This mechanism was studied in Fan et al. (2009) that concluded that convection invigoration is favored under low vertical wind shear conditions, while the opposite happens with high vertical wind shear".

2.

a. **(Question)** The justification for why N_a is used rather than CCN does not make sense to me. The CCN measurements include supersaturations up to 0.55%, so why not just choose a fixed supersaturation (interpolate if needed) and use CCN. The authors should use both CCN and N_a and compare the results.

b. **(Answer)** We understand the concern because, at first glance, CCN concentrations seems to be the way to go. However, it is known that clean and polluted clouds can present different supersaturation conditions, making the use of a constant-supersaturation CCN measurement not representative of the cloud variability we observed. In this case, it is hard to obtain a common benchmark to make comparisons. Nevertheless, the averaged CCN and N_a below clouds proved to be linearly correlated and N_a does not change with supersaturation. Calculating partial derivatives to N_a or to the proportional CCN would not change the results. Added the following text at P5 Line 29:

   "It is known that polluted clouds tend to have lower supersaturations given the enhanced condensation. Therefore, the use of constant-supersaturation CCN concentrations does not provide a common benchmark between the clouds probed here. Conversely, it is difficult to obtain the supersaturation within the clouds and the consequent CCN concentration modulation. In that regard, N_a proved to be most adequate for providing a framework to compare polluted and clean clouds".

3.

a. **(Question)** The authors should investigate the impacts of the rather large bin sizes they need to compute the partial derivatives on the values of the derivatives.

b. **(Answer)** This is a tricky process. We believe the bin sizes are as large as they can possibly be. Therefore, the only option would be to assess the effects of narrower bins on the results. However, even if there are differences between the narrow-bin and broad-bin cases, it is difficult to assess the significance because of the lower statistical confidence of the former case. Nonetheless, we performed a test by increasing the number of w and H bins. We maintained the same overall interval (0 ms^{-1} < w < 8 ms^{-1} and 0 km < H < ~4 km) and added one w bin and three H bins. By recalculating the sensitivities and comparing to the results shown in Tables 2-3, we obtained a maximum difference of 0.064. Therefore, narrower bin sizes would not result in significantly different results.

Added the following sentence on the end of Section 2.2: "Different bin configurations were tested and the results proved to be relatively insensitive to the bin number and width".

4.

a. **(Question)** P1, Line 32. I disagree that height above cloud base is a good proxy for time in cloud. A much better estimate would be *H*/*w*, which actually has the units of time and would be the exact time in cloud if w is constant with height. The authors should re-evaluate their conclusions in the light of this error.

b. **(Answer)** We understand your concern given that *H* may seem an arbitrary choice. However, we believe there is no real gain by analyzing the results in terms of *H/w* instead of *H* only. If we were to directly estimate the cloud lifetime from the *H/w* ratio, we would have to prescribe the *w* vertical profile. Even though we have some measurements in the clouds, this would be very difficult given that *w* also varies greatly in the horizontal direction (contrast between cloud core and edges). On the other hand, if we consider a constant *w*, then *H* would be directly proportional to *H/w*, meaning that *H* is an effective proxy for cloud lifetime as calculated from this ratio. Taking into account the measurement strategy and that we are measuring almost exclusively growing convective elements, the choice of *H* seems to be the most direct one. We made this point clearer in the text by adding the following discussion in the 4th paragraph of Section 2.2: "It could be argued that the ratio *H*/*w* would be a more direct estimate of the cloud lifetime, given that it is the time that it took for the cloud to reach *H*. However, this approach would need prescribed *w* profiles below each measurement, which is not feasible in this study given that different clouds can be measured in the same profiling mission. Additionally, there is high *w* variability horizontally between the clouds edges and cores, adding extra complexity. Therefore, we will use *H* as the proxy for cloud evolution even though it does not represent cloud lifetime directly (i.e. does not have units of time). The profiling strategy of measuring growing convective clouds favors this interpretation".

5.

a. **(Question)** McFiggans et al. (2006, ACP) have a good review paper exploring factors controlling cloud microphysics. The results here could be put into the context of the findings in that paper.

b. **(Answer)** We cite this paper in the first sentence of Section 2.2. We do provide discussions considering the overall context of the sensitivity approach. We present the papers that performed sensitivity calculations, McFiggans et al. (2006) being one of them, and highlight their limitations and how we aim to improve such analysis. Therefore, we believe we are already putting our results into perspective.

6.

a. **(Question)** P5, line 27. What are linear and angular coefficients?

b. **(Answer)** Those are the parameters we obtained from the regression curve between N_a and *CCN*. The sentence has been changed slightly following this feedback.

7.

a. **(Question)** Was there any correlation between *w* and N_a? How might this change the results?

b. **(Answer)** Firstly, we consider only a constant value for N_a for each flight (the averaged measurement around the cloud base altitude). Therefore, for cloud base, there would be several *w* values for only one N_a and we cannot calculate the correlation between them. Nonetheless, any correlations between *w* and N_a (or *H*) are minimized by the binning procedure. When we calculate the sensitivities by fixing two bins and varying the third, the interdependences between N_a, *w*, and *H* are eliminated.

8.

a. **(Question)** P11, Line 24-26. I couldn't follow this argument at all. Others will probably have difficulty with it. Sensitivity in N_d to what?

b. **(Answer)** It is sensitivity of N_d to N_a, as can be inferred from context. Changed the sentence slightly to: "By comparing to the expression $\overline{S_{D_{eff}}(N_a)} = -\frac{1}{3}\overline{S_{N_d}(N_a)}$ often found in the literature, we can conclude that the value of the sensitivity of N_d to N_a is offset by some effect on DSD shape. In other words, two thirds of the N_d sensitivity is allocated into DSD narrowing or broadening, while the remainder is effectively altering D_{eff}".

9.

a. **(Question)** P12, Line 14. Insensitive rather than insensible.

b. **(Answer)** Thanks.