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Comment on acp-2021-677

Anonymous Referee #1

Referee comment on "Factors affecting precipitation formation and precipitation susceptibility of marine stratocumulus with variable above- and below-cloud aerosol concentrations over the Southeast Atlantic" by Siddhant Gupta et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-677-RC1, 2021

Review of "Precipitation Susceptibility of Marine Stratocumulus with Variable Above and Below-Cloud Aerosol Concentrations over the Southeast Atlantic" by Gupta et al.

This paper presents airborne observations from the ORACLES project that examine how cloud and precipitation characteristics vary with perturbations driven largely from the entrainment of free-tropospheric biomass burning aerosols into the southeast Atlantic marine boundary layer. The authors extend their previous study by incorporating a much larger observational dataset from additional flight years and they extend their prior work to look at precipitation susceptibility. I commend the authors for synthesising such a large dataset and I found the paper to be generally well written. The topic area is certainly suitable for publication in ACP. However, I do have a major concern about the use of the CAS probe to measure liquid water content and cloud drop size from the 2016 campaign (see below), that I feel the authors need to address before this paper can be published.

Main concern

- Use of CAS probe for 2016 flights. I have concerns about the use of the CAS data for calculating microphysical properties on the 2016 campaign. Although the cloud drop number concentration from the CAS looks reasonable when compared against the PDI (Fig S1a), the LWC looks to be underestimated when compared against both the PDI and King probes (Fig S1b, Fig S2). The authors show that the PDI can give higher LWC values when compared to the adiabatic value and so choose not to use that instrument. But given that the bulk LWC estimate from the King probe is also much higher that the CAS (Fig S2), I think the authors need to provide some additional justification as to why the CAS probe is thought to be reliable (for measurements of LWC and effective radius). A possible approach would be to look at cases where the cloud was expected to be more adiabatic (well mixed boundary layer, non-drizzling etc) and examine if the difference with the adiabatic LWC value shown in Fig S2 is still apparent. Alternatively, in precipitating clouds, can the overlap with the 2DS probe be looked at to at least check for consistency at the larger cloud drop sizes that contribute significantly to LWC. I also note however that a similar low bias in LWC from the CAS is shown in the 2017 and 2018 campaigns when compared against a CDP (Fig S4 and S6), which suggests that it could be a general measurement issue with the CAS measurements. Related to this point, if the authors removed the 2016 data from their analysis, do any of the conclusions of the paper change?

Minor comments

- Line 34: Suggest changing to "changes in microphysical properties" in this sentence as the proceeding sentence states that LWP and cloud thickness are similar. Also it is not clear what the reference to "existing relationships" means. Which relations are you referring to? Are these based on previous observations or parameterized/simulated in models for example?
- Line 68: assuming constant LWP in what?
- Line 78: sink of liquid water rather than LWP?
- Line 82: Suggest changing to "relates the change in Rp…." as the actual definition is Eq 9.
- Line 87: Suggest changing to "parameterized in models"
- Line 93: Suggest changing to something like "A focus of recent field experiments in the southeast Atlantic Ocean has been to study ACI in this unique meteorological ……..".
- Line 98: I assume the values of single scattering albedo and above cloud optical depth are from ORACLES. Please make that clear. Much higher optical depths can occur in the region e.g. Peers at al. (2021).
- Line 102: Suggest changing "positive forcing" to "aerosol absorption of SW radiation". Also please expand on why this decreases entrainment e.g. strengthening inversion.
- Line 149: Please include references for the different probes.
- Line 159: Change to "Hawaii"
- Line 165: What thresholds on the PCASP data were used to screen cloud?
- Line 168: Change to "with the CDP N(D) for 50 < ….."
- Line 170: Can the authors comment on how well the different probes compared for drop sizes where they overlap?
- Line 176: refer the reader to the supplement.
- Line 215: Please briefly outline what you mean by shallow boundary layers can provide an underestimate of LWP adjustments. And I don't understand what you mean in the sentence on line 217. Please clarify.
- Line 219: Have you done any analysis of the thermodynamic data to ascertain the frequency of well-mixed vs decoupled boundary layers from the vertical profiles used in this study? And are the clouds studied typically a single layer of stratocumulus, rather than cumulus rising into stratocumulus. If the latter form a significant number of profiles, can the authors comment on how that may impact the results e.g. cumulus could transport aerosol from the surface mixed layer up into the overlying cloud.
- Equation 1: Is effective radius calculated as a function of height or is it a cloud top value.
- Equation 4: What does LWC(zB) mean? Is it your fixed value of 0.05 g m-3 to define cloud base?
- Line 283/Table 4: Is the effective radius calculated at cloud top, or is it an average value through the depth of the cloud?
- Line 377: Suggest changing "ascent" to "updraft"
- Line 385: Suggest changing "explained" to "parameterized"

- Figure 5: y-axis labels should be SAUTO95 and SACC95
- Line 420: Add comment as to why you use CO as a proxy for the airmass from biomass burning aerosol source regions.
- Line 422: Suggest the authors may want to comment on how the different regimes occur. For example, does the S-H regime have high boundary layer aerosol loadings because of previous entrainment events prior to the aircraft sampling and does the C-L regime have low aerosol loadings because there hasn't been sufficient time for aerosol to be mixed down into the boundary layer from the overlying aerosol plume.
- Line 433: Figure 6 does not show comparisons of Re as stated in the text.
- Line 443: The authors test boundary layer aerosol concentration thresholds of 300 to 400 cm-3 to split "low" and "high" aerosol conditions. Even the value of 300 cm-3 seems like a moderately polluted boundary layer though compared to pristine marine conditions, where I would expect values < 100 cm-3 to be typical. Did ORACLES measure cleaner boundary layer conditions and if yes, how do the cloud properties in these cleaner clouds compare to the broader "low" aerosol regime used in this study? Or do future studies need to compare/contrast with more offshore airborne measurements from the CLARIFY campaign for example?
- Line 448: Is quartiles the correct term, given that the bins don't have an equal number of profiles (82,80,85,82 in Table 6)?
- Figure 9: It looks like there are data points with Nc = 0 cm-3. Is that correct?
- Line 489: Suggest adding "decreased with H from *…..include your numbers for the lowest H bin data*….to 0.53"
- I found some of the text hard to follow in section 6.3, that was at least in part due to the number of figures that were referred to in short succession. For example, the short paragraph beginning on line 489 refers to four different figures. I would suggest the authors consider rewriting some of the text and highlighting key points, to make it easier for the reader.
- Line 491: I struggle to see how the reader can see this change by looking at Fig 9 b,c.
- Line 513: The paragraph starts by mentioning appendix B, but does not then summarize the key point of that sensitivity study. Further, it states that the appendix investigates the inclusion of precipitating clouds, but I think it instead looks at the impact of the removal of non-precipitating clouds.
- Line 600: What is the mechanism that results in higher RWP for these contact profiles?
- Line 625: Suggest rephrasing this statement. The separated polluted boundary layers have presumably also experienced entrainment events prior to the aircraft sampling, even though there is no contact at the time of the measurements. The timescales of entrainment and history of the airmass do also need to be considered (Diamond et al., 2018), rather than just an instantaneous measure of "contact" vs "separated".
- Line 630: Make it clear that this is when compared to separated profiles.

References

Diamond, M. S., et al., 2018. Atmos. Chem. Phys., https://doi.org/10.5194/acp-18-14623-2018, 2018.

Peers, F., et al., 2021. *Atmos. Chem. Phys.*, 21, 3235–3254, https://doi.org/10.5194/acp-21-3235-2021.