Atmospheric Chemistry and Physics Discussions

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Comment on acp-2021-677

Anonymous Referee #2

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-RC2, 2021

- Does the paper address relevant scientific questions within the scope of ACP?
  - The paper addresses "Precipitation Susceptibility of Marine Stratocumulus" to variations in aerosol concentrations. This is an important topic within the scope of ACP.
- Does the paper present novel concepts, ideas, tools, or data?
  - The novel part of this research is the aircraft dataset. No new concepts, ideas or tools are introduced.

- Does the title clearly reflect the contents of the paper?
  - The title places emphasis on precipitation susceptibility, which is only a small component of the paper, has questionable validity, and lacks proper discussion.

- Does the abstract provide a concise and complete summary?
  - The title emphasises "Precipitation Susceptibility". This is not mentioned at all in the abstract, but it is discussed in the conclusions.

- Are substantial conclusions reached?
  - The main results stem from figures 3 and 7, which demonstrate that there is some difference in the cloud microphysical properties between two subsets of the data. This is attributed to biomass burning aerosols entrained into the boundary layer from above. The paper also explores potential impacts of aerosols on precipitation formation, and the role of meteorological factors also affecting the microphysical properties. The conclusions are rather limited in scope, and in part unsubstantiated. I understand observational based research is important and also difficult to publish in its own right without complex modelling, so maybe a "Measurement Report" is

more suitable format?

- Are the scientific methods and assumptions valid and clearly outlined?
- Are the results sufficient to support the interpretations and conclusions?
  - The paper uses specific statistical terminology (e.g. "95% Confidence Interval") which inherently implies parameters are known to exhibit normal distributions when properly sampled. Is this valid? Is this approach needed?
  - Adiabatic approximations of Brenguier are used. Limitations should be discussed/quantified.
  - Measurement uncertainties are not presented alongside observations. E.g. What is the estimated uncertainty in measurements of droplet effective radius, and how does this relate to changes between cloud base/top, and also between the aerosol regimes studied?
  - Calculations in the paper suggest the thinnest clouds have large precipitation sensitivity to aerosols. This seems odd given these thin clouds have nominally the same droplet concentrations as thicker clouds, but only have the smaller droplets. This raises concerns with how data are handled and the overall validity of conclusions drawn from the analysis. From the text I don't fully understanding what was done here with the data to determine the precipitation susceptibility, so maybe my interpretation is wrong, but I speculate it is a result of using outputs from regression analysis which are statistically meaningless. If this is true, the paper is presenting misleading results which is very undesirable. If this is not true it needs making clearer.
  - The stratiform clouds are shown to be around 200m thick, and often occur in the vicinity of convective clouds. Is direct comparison of high resolution in-situ datasets with relatively coarse resolution ERA5 reanalysis data sufficient to untangle effects of meteorology? What small-scale/local variations in SST could you expect based on other studies? What are the actual sizes/resolutions of ERA5 grid boxes in units relatable to the observations? Can ERA5 resolve the inversions etc? The correlations in Fig10b between LWP from in-situ and ERA5 are poor, which casts a large doubt over the validity over the in-situ LWP vs SST/LTS/EIS from ERA5. Why aren't in-situ observations of inversion strength analysed?
  - From the very beginning this paper places emphasis on the role of aerosols from above cloud and their ability to modify clouds via entrainment etc. There is no discussion of the potential for the boundary layer being polluted with Biomass Burning aerosols in its own right, without the requirement for entrainment from above the BL inversion. Is there data showing the transition of the BL from clean to polluted as aerosol mix downwards? If so it would be very useful to show it.
  - The paper filters data according to aerosol concentrations above cloud ("contact" vs "separate" using a 500cm^{-3} threshold) and below cloud ("high" and "low" Na with a threshold of 350cm^{-3}). However, the cloud droplet concentrations in Fig 6 do not show evidence of enhancement due to above cloud aerosols for the "clean" BL cases. The only strong response in droplet concentration is when there are lots of aerosols also in the boundary layer. It seems impossible to disentangle the below and above cloud aerosols and therefore the role of entrainment and above cloud aerosols is ambiguous.
  - There is no contextualisation of the results. For instance, are the calculated changes in re, or values of S_o "large" or "small"? Are changes in these clouds due to the Biomass Burning aerosols having any *meaningful* impact? What have other studies found?

- Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
- Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
  - Undefined formulae: Z_N
  - Confusing presentation of $\Gamma^{850}_{\text{ð□□□},}$ in eqn 10
  - Description of LCL is confusing and the equation is poorly formatted
  - Some of the technical details of data processing are in figure captions, but should be included in the text.
  - What are the "kernel density estimates" mentioned in caption for Figure 3? They are not mentioned anywhere else in the paper.

- Is the overall presentation well structured and clear?
- Is the language fluent and precise?
- Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
  - The paper was difficult to follow. I feel the paper is too long and lacked coherence. It is very ambitious, and the authors have covered lots of areas which are all important and related, but the balance is not quite right. The paper has lots of useful data but in its current form does not provide concrete outputs which can be used by the broader community.
  - Most figures should be improved and are poorly rendered, and some do not have proper legends etc (e.g. Fig 10b has a 1:1 line listed as "x=y", wrong coloured text in legends).

- Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
- Are the number and quality of references appropriate?
  - Yes there are a good number of quality references. Some references are missing (e.g. description of instruments in section 2) but nothing major.

- Is the amount and quality of supplementary material appropriate?
  - Yes, supplementary material is of good quality and is a useful addition.