Review of Wu et al., 2025: The transport history of African biomass burning aerosols arriving in the remote Southeast Atlantic marine boundary layer and their impacts on cloud properties

This paper combines in-situ data from the CLARIFY-2017 campaign to satellite data and backtrajectory analyses to investigate aerosol-low-level cloud interactions around the Ascencion Island in the remote South-East Atlantic. The authors show that cloud droplet number concentrations are increased, and droplet radii are decreased by biomass burning aerosols, especially if the aerosols are in the sub-cloud layer. This agrees with published literature that highlights the important role of the respective positions of aerosol and cloud layers for ACI in this region, even though the exact functional relationship between aerosol and cloud properties seem to differ compared to other biomass burning regions. The authors then carry out backtrajectory analyses to estimate the exact timing of FT air entrainment into the marine boundary layer for 3 different case studies and identify efficient mixing regions 2-3 days upwind of the Ascencion Island, which could be used to constrain climate models that struggle to accurately simulate ACI in the SEA. I think these results are interesting and can be published after major revisions.

Major comments:

The novelty of the paper is not immediately obvious to me, but I believe this could be fixed with some reorganization of its content.

- Many of the figures in the first half of the paper are reminiscent of the figures in Haywood et al., 2021. Since there are a lot of figures in the supplementary material, maybe it would make sense to remove redundant figures and instead move some of the supplementary material to the main text.
- I also wonder whether the instrument descriptions have previously been published in data papers, in which case it might not be necessary to have such a long method section.
- Since it has been a few years since the CLARIFY 2017 campaign, it might be helpful to summarize for the reader the findings that have already been published using the campaign data, and mention questions that have yet to be answered, including the one(s) this study wants to address, which would make the novelty obvious.
- The paper could be more succinct if the authors removed some redundancy in the explanations. For example, there are long paragraphs dedicated to reviewing the literature in the results section, but the relevance of the cited studies to this study might be more convincing if summarized into 1 or 2 sentences only.
- It might also be helpful to make the abstract more succinct.
- On Ln 540, you mention that your study has important implications for the radiative effects of aerosols in the region, but have you looked at how TOA radiative fluxes from SEVIRI change along the study period, depending on the aerosol load and location ?
- Section 4.2: In my opinion, this section needs to be rewritten.
    - I am concerned about the AOT analyses. Since AOT is a vertically integrated value, the analysis of AOT associated to either MBL or FT air parcels is misleading. For instance, an increase in AOT for the MBL air parcels could simply mean that the MBL is located under a polluted FT, but the MBL itself could still be pristine (and vice versa, for a clean FT over a polluted MBL). Could you address this caveat more explicitly?
    - Ln 490-510 is a very lengthy explanation mixing literature review and some interpretations. Some of the literature review could be moved to the introduction, which would allow the interpretations to stand out more convincingly.

Minor comments :

- some choices of words are a bit vague and confusing, for instance. For example, on Ln 28 : « a greater variability was noted in more polluted clouds» is imprecise. Same on Ln 352 (« a greater

variation»). Do you mean that the linear fit is weaker ? Another example on Ln 295 : « Larger ranges of Nd and LWC values … a smaller range of Re ». Do you mean that Nd and LWC values are larger on average and Re values are smaller on average, or do you mean that there is a larger/smaller standard deviation in the observed distributions (to me, « range » suggests the latter). There are several other occasions where « range » is used in this way in the text (e.g., Ln 404, 572). Could you use more precise mathematical language to help readers better understand your point ?
- Ln 50 : « affecting », not « ~~affect~~ ». The last part of the sentence (starting with « underscoring... ») sounds redundant and could probably be removed.
- Ln 120 : remove « ~~within~~ », correct « AfricaN continent »
- Ln 125 : This study is obviously focused on stratocumulus clouds, and the study area is the Ascension island, but traditionally, Sc are thought to form closer to the coast (e.g., areas defined by Klein and Hartmann, 1993), and then transition to cumulus clouds as they move westwards to Ascension Island, which might leave some readers confused. It would be good to address this. Since you actually already have a list of cloud types observed during the campaign in Table S1, it would be good to reference it in the main text.
- Ln 138 : «straight and level runs » not defined
- Ln 142 : why would a POC event not be relevant to this study ?
- Ln 145 : as written above, the instrument description could be shortened if the material is already published somewhere else. What would be interesting to add though is the typical size range of BBAs so that the readers can easily assess the suitability of the measurements.
- Ln 182-185 : the difference between the 0.1 g/m³ LWC and 0.2 g/m³ 'bulk' LWC thresholds was not clear to me
- Ln 190 : (CTO) instead of ~~(COT)~~
- Ln 194 : I believe there is an error in the equation, it should be $COT^{1/2}$ and not $COT^{-1/2}$
- Ln 196 : Did you check the quality of your SEVIRI products close to sunrise and sunset times ? There might be biases around those time, this could impact your results if sunrise/sunset points are colocated to the backtrajectories.
- Ln 197 : The NAME description section got me a bit confused. I did not understand why there seems to be 2 sets of backtrajectories, one at 3-hr resolution, and the second one at 15-min resolution. What is the point of having both sets of trajectories ? why use NAME backtrajectories vs. using the already produced HYSPLIT ones from the Haywood et al., 2021 paper ?
- Ln 230 : here you introduce one definition for the inversion height, but later (Ln 246) you use a different one, why is that ?
- Ln 263 : The FT humidity comparison between clean and BB-impacted FT cannot be inferred from Figure 3, can it ?
- Ln 265 : what is the interpretation for this positive correlation between BBA and humidity in the FT?
- Ln 269 : Could the MBL CMD simply be lower because the BBAs are mixed with smaller aerosols (e.g. sulfate, sea salt, etc.) in the MBL but not in the FT ?
- Ln 279 : CCN/CN_3 and not ~~N_a/CN_3~~
- Ln 288-291 : as per my comment above, it would be helpful to already include a description of observed cloud types during CLARIFY in the introduction, to contextualize the method and results.
- Ln 292 : near *cloud* top (at the end of the line)
- Ln 321 : what does a « «relatively » negative correlation mean ? Relative to what ?
- Ln 345-346 : could you provide the reference for these numbers again ?
- Ln 347 : maybe « indicated » could be replaced by another verb, like « hypothesized »
- Ln 353 : remove « recent »
- Ln 361 : how were these contact profiles selected ?
- Ln 362 : what does a « central » relationship mean ?
- Ln 365 : just a note that entrainment of warmer and drier FT air could also lead to cloud droplet evaporation and decreases in Nd (cases of extreme inhomogeneous mixing, see Hill et al., 2009)

- Ln 371 : Can you quantify the goodness of fit (for instance with the R2 for the linear regression between logNa and logNd) ?
- Ln 372/372 : from the confidence intervals, it looks like the fit is quite uncertain. How should these parameters be used if there are so uncertain ?
- Ln 382 : Do you have a more detailed hypothesis of why SEA BBAs might have a better CCN ability compared to other regions ? (based on the literature ?)
- Ln 410 : in this paragraph, what can the given fit be interpreted, for instance does it tell us anything about the adiabaticity of the sampled clouds ?
- Ln 428 : maybe « linked » can be replaced with « co-located »
- Ln 450 : in the methods, it is said that the trajectories are initiated from an altitude of 341m, yet on Fig. 7, it looks like they come from an altitude of 2 km, why is that ?
- Ln 478: Have you explained what the air-density-weight transformation means for AOT?
- Ln 527: “observed larger droplets” is mostly true, but at -24h on Fig. 8, the droplet sizes are decreasing. Why is that?
- Ln 573: “a stronger relationship” could be made more precise with “a stronger linear correlation”
- Ln 576-577: Could it be interesting to add a quantification of relatively how much mass of aerosols is entrained along the transport vs at the place of observation? Or how much percent of the mass is entrained in the identified “efficient mixing regions”?
- Ln 845: “Langitude” typo in the x-axis of Fig.1.
- Ln 845: why are the fire counts cumulated over August 2017 only, and not over the exact study period?
- Ln 860: the f) and g) labels have been swapped compared to what is shown in the figure’s subplots

References:

Haywood, et al.: The CLoud–Aerosol–Radiation Interaction and Forcing: Year 2017 (CLARIFY-2017) measurement campaign, Atmos. Chem. Phys., 21, 1049–1084, https://doi.org/10.5194/acp-21-1049-2021, 2021.

Klein, S. A., & Hartmann, D. L. The seasonal cycle of low stratiform clouds. *Journal of climate*, *6*(8), 1587-1606, 1993

Hill, A. A., Feingold, G., & Jiang, H. The influence of entrainment and mixing assumption on aerosol–cloud interactions in marine stratocumulus. *Journal of the Atmospheric Sciences*, *66*(5), 1450-1464, 2009