2nd Review of *Observed impacts of aerosol concentration on maritime tropical convection within constrained environments using airborne radiometer, radar, lidar, and dropsondes* by Amiot et al. (2024).

I commend the authors for making significant methodological improvements in their manuscript. Indeed, it appears that these changes had a substantial impact on some of the correlations and I believe that the results should be more robust now. Nevertheless, I still find some major problems with the manuscript and I suggest to return the manuscript to the authors for ***major revisions***.

Please note that line numbers are based on the manuscript without tracked changes.

General comments

1. My most major concern regards the overall interpretation of the results. Despite the more robust methodological approach, there are still many unexpected results. Furthermore, if one investigates correlations for such a large number of variables one is bound to find a correlation somewhere. What about all the non-correlated examples? There is little to no discussion about them. Yes, there are some potentially interesting correlations that the authors can report, but I still feel that these correlations are strongly influenced by one or two outliers. Take these points away and the correlation disappears. Visually it seems that correlations could be entirely different in most cases if just a few points are changed. Are the outliers really related to aerosol-cloud interactions or is there something entirely different going on? To me the authors have not sufficiently demonstrated that different processes can be ruled out (see for example comment 4).

   In fact, in Figures 5 and 7 there appears to be a general trend that for larger sample sizes correlations are smaller. Below I also show an example from Figure 7. For 850-500 LR there is a strong correlation in the medium bin, but for the 700-500 LR there is strong correlation in the high and low bins. It seems likely that this happens because of just a few points shifting between bins.

700-500 LR	0.78 (9)	0.74 (9)	0.82 (9)	0.83 (9)	0.79 (9)	0.69 (9)	H
	0.09 (10)	0.04 (10)	0.16 (10)	0.17 (10)	0.12 (10)	0.16 (10)	M
	0.90 (8)	0.80 (8)	0.96 (7)	0.96 (8)	0.82 (9)	0.92 (9)	L
850-500 LR	0.28 (9)	0.27 (9)	0.41 (9)	0.43 (9)	0.45 (9)	0.53 (9)	H
	0.83 (9)	0.81 (9)	0.84 (9)	0.84 (9)	0.79 (9)	0.72 (9)	M
	0.07 (9)	-0.04 (9)	0.07 (8)	0.01 (9)	0.06 (10)	0.07 (10)	L

   To me personally the results remain rather inconclusive. Maybe the authors can rethink their interpretation of the data by emphasizing inconclusiveness due to limited samples and many unexpected trends. I do believe that such results should be reported as well. The way the results are currently reported I find it hard to justify publication in ACP. We have seen in previous literature that sometimes there are correlations between convective parameters and aerosol measurements. Is it really surprising to find this in a new dataset when looking at 100s of potential correlations?

2. Maybe a bootstrapping approach could help investigate the issue of outliers. Randomly remove 10 – 20% of the data points many times and recalculate correlations to achieve a mean correlation. For the small sample sizes this might need to be limited to unique realizations of the dataset. However, I fear that overall correlations would become much weaker.

3. The introduction has improved significantly in terms of the covered literature. However, I still feel it is not very engaging, mostly it is just listing previous research without telling a story that motivates the research. Still the paragraphs starting in lines 42 and 103 seem out of place to me. One part that is missing is why we care about aerosol impacts on convection. It is interesting that there are interactions but what are the potential consequences (radiation, precipitation, etc.)? I think that is what the introduction should start with.

4. I am unsatisfied with the answer to my previous comment 4 about different convective regimes. I do believe that these can significantly impact results since development mechanisms will differ between types of convection. The authors mentioned a squall line. More organized convection might have developed 100s of km away in a different aerosol and thermodynamic environment. Maybe some shallow cumulus clouds could be invigorated compared to other shallow cumulus clouds, but we cannot really see this because all shallow cumulus will appear as weak convection compared to deeper cumulus. Just to name a couple of problems. In essence, this introduces a lot of noise to the results which might be causing some of the outliers that appear to be causing many of the trends.

5. I am also still unsatisfied with the description of the CAMP2Ex campaign. Can the authors please make a dedicated section about the campaign in the methods section? In general, the methods section would benefit from some dedicated subsections so it is easier to find specific details about the methodology.

Specific comments

6. 57: Probably better to use ‘clouds’ here instead of storms.
7. 59: ‘describes’ instead of ‘favors’.
8. Figure 1: Excuse me if I do not fully understand the measurements, but why is there significant CLW where the radar does not observe any cloud?
9. 127: These variables have not been defined.
10. 157: ‘absolute deviation’, does this refer to CLW?
11. 262: I don’t quite understand why scenes longer than 11 minutes had to be masked. Couldn’t you just use the 10 minutes around the dropsondes?
12. 347-350: Can points with significant masking be indicated somehow? Maybe use the same symbol but only show the outline for scenes where masking exceeded a certain threshold.
13. 549-551: I think I understand what the authors are trying to say, but please consider rephrasing.
14. 585: The following paragraph is missing some kind of discussion about why at least some of the things mentioned here were not done in this study.