Review of Mifsud et al. “Coarse-mode sea spray aerosols enhance warm rain formation in in marine low clouds” submitted to ACP on 27.05.26

The paper discusses the influence of giant cloud condensation nuclei (GCCN) on rain formation in marine low clouds. This is done by analyzing in-situ data from the ACTIVATE campaign, supplemented by modeling in an idealized setup. In the literature, there are studies of GCCN, both from observations and modeling. The paper discussed does not uncover new effects. However, its value is mostly in analyzing GCCN from new observations that were taken over a long period of time (ACTIVATE lasted 3 years). The analysis results show effects that are in qualitative agreement with previous studies: that GCCN can increase rain amounts several times, that their effect is modulated by the amount of liquid water and by CCN concentration, and that GCCN concentration is correlated with wind speed.

Major comments

I think that the paper would be more impactful if several changes were made:

1. There are significant differences between GCCN concentrations from the two instruments used in the analysis: the Cloud Droplet Probe (CDP) and the Cloud-Aerosol Spectrometer (CAS). This difference is not explained, it is not clear which one could be more precise, and GCCN concentration is crucial for the study. This difference propagates into the entire analysis (e.g., discrepancies in Fig. 7). Including data from the third instrument available (Fast Cloud Droplet Probe, FCDP) might reveal which of the results is closer to reality. FCDP was not included because of a higher threshold of droplet detection than that of the other instruments. Still, its results could at least be used in Fig. 2 to give confidence in CAS or CDP. A comparison with data from the Giant Nucleus Impactor (GNI) used in other campaigns (e.g., Jensen and Nugent 2017) would also be welcome.

2. Authors conclude that GCCN concentration cannot be explained by wind speed only, but that other unknown factors are also important. This comes from looking at correlations between GCCN concentration and wind speed, which are weaker than in existing aerosol flux parameterizations. However, they measure GCCN concentration and wind speed well above the sea surface (around 700 m on average). This can lead to errors in the analysis. First, GCCN at these altitudes can come from prior emissions at different wind speeds or from advection from other regions (given GCCN lifetime could be between hours and days). Second, there could be significant error in deriving near-surface wind speed from measurements at 700 m using a logarithmic correction. These caveats need to be addressed.

3. On multiple occasions, authors conclude that rain enhancement is better correlated with GCCN mass than GCCN concentration. That is true for the modeling data only. In observations (Fig. 7), CAS data shows the opposite (that concentration has a stronger influence) and CDP data shows similar effects of GCCN mass and concentration. This could be related to higher uncertainty of GCCN mass measurements due to small sampled volume (rare, large GCCN are rarely sampled and they affect mass more strongly than concentration).

4. In section 3.3 (observations of GCCN influence on precipitation), GCCN measurements cannot be co-located with RWC and LWC measurements. Therefore, flight-averaged GCCN concentration/mass is used. How big is the variability of GCCN concentration/mass during such a flight? If large, this adds a lot of uncertainty to the analysis.

5. There is significant variability in observations (e.g., Figs. 4, 8, 9). The analysis is based on 6 months, but the ACTIVATE campaign lasted 3 years. Including the entire period could smooth the variability and more reliably reveal trends. Why were only 6 months used?

6. Results of modeling should be quantitatively compared with the other cited modeling studies on the subject.

Other comments:

- Fig. 1: Why are error bars shown only for selected points, with more error bars for CAS than for CDP?
- In Fig. 2: Plot aerosol distributions from other studies (e.g., Colón-Robles et al. 2006, Jensen and Nugent 2017) if possible.
- lines 488–489: *“the relative impact of microscale turbulence is greatest for the base Na ensemble, and is also most important at the lower precipitation rates”* — this is not clear to me looking at Fig. 5.
- Code used for simulations is not made available.
- Author contributions need to be more specific — currently only 3 of 13 authors are mentioned.

Editorial comments:

- line 641 — missing word after “slightly”
- line 674 — “this study” should be “their study”?