EGUsphere, referee comment RC1
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Comment on egusphere-2022-1406

Anonymous Referee #1

Referee comment on "Aerosol–cloud impacts on aerosol detrainment and rainout in shallow maritime tropical clouds" by Gabrielle R. Leung et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1406-RC1, 2023

Leung et al. provide an interesting perspective on aerosol-cloud interactions in warm marine clouds by arguing that aerosol loading not only perturbs the clouds themselves but also the overall aerosol budget. These changes to the aerosol budget are primarily driven by changes to entrainment/detrainment and rainout. Overall I found the paper to be interesting, with nice figures, and it is a good fit for ACP. However, I would like to see more explanation of the experimental setup and analysis of the time-evolution of quantities the before recommending acceptance. Detailed comments below:

L44-45: it would be good to acknowledge here that aerosol changes can also produce changes in atmospheric circulation, which generate global-scale impacts and impacts on different cloud regimes. For example: Dagan (2022, JAMES) and Williams et al. (2022, Nature Climate Change).

L50: It took a few tries for me to understand this part of the sentence, could you please reword? "Aerosol-induced changes to clouds may feed back to how clouds and precipitation influence the aerosol field...". Maybe "Aerosol may alter the relationship between clouds and precipitation and the overall aerosol field..."?

L76: I appreciate that you don't wish to repeat everything about these simulations, but a few more details would be helpful here. For example, do you include a diurnal cycle or does the "diurnal cycle" of Line 79 just refer to a 24hr period? By the sounds of it you included a diurnal cycle in the solar insolation, which I imagine would also alter the aerosol budget through changes in cloudiness? If indeed there is a diurnal cycle in the simulations it would be good to analyse whether these effects vary depending on the time of day (or at least argue why you *don't* do this).

L82: "After initialization, the model was allowed to evolve freely without additional large-scale forcing." Without imposing large-scale forcing, how do you prevent the formation of deep convection in your domain? The lack of a large-scale forcing is confusing to me, why do the authors not just use an established case study like RICO?

L90: Do the 'microphysics-radiation' experiments this include the Twomey effect for ice as well as liquid?

Figure 1: Very nice schematic, I found it helpful!

L109: A bit pedantic, but I'm not sure if the use of "ensemble" is justified here (or indeed anywhere in the paper) as you only use one set of initial conditions per simulation. Instead, maybe just "multiple simulations were conducted where we varied X and Y...".

L120: SSA and other quantities have to defined at a specific wavelength, eg 550nm. Which is it? Also, for the ARI experiments it would be nice to also quote the AOD if the data is available, to more easily compare with other studies with simpler aerosol schemes.

L149: "Qualitatively similar cloud fields develop in all sixteen simulations..." It would be helpful if you could demonstrate this to the reader. For example, a figure showing the time-evolution of domain averaged fields would be helpful to get a sense of the variability over the 48hr period, and perhaps some sense of the spread in domain-avg properties across the experiments too.

L152: Again, I'm a bit confused how you don't get more deep convection if you don't impose large-scale temp/moisture tendencies?

L162-164: Is this just a snapshot at the end of the simulation? How much variability is there in the timeseries? I'm left wondering exactly how representative these changes are.

L169: Would the aerosol number not also be conserved? It sounds like if you are just tracking how this is partitioned across the four categories then you could also get a closed budget for aerosol number?

L172: Would it be possible to show this, at least in the reply? 5% is actually quite large relative to the magnitude of these changes with aerosol loading in Fig. 3. Also, I'm confused about why aerosol mass would be "lost" (i.e. not accounted for in your budget) due to dry deposition? Sorry if I'm missing something here.

Figure 3: Please add a '100' marker to the x axis :)

L179: 'simulations' not 'ensemble members'

L196: It is indeed clear from the figures, but I'm still wondering how representative these changes are if they're just calculated from snapshots? I assume there must be a decent bit of temporal variability in the simulated budget quantities?

L236: Why would larger droplets have a smaller surface area? I find the wording confusing.

L241: Regarding precipitation efficiency it would also be good to cite Lutsko et al. (2022, AGU Monographs) and Li et al. (2022, Nature Climate Change). Also it would be nice to discuss whether these results are consistent with the recent study by Dagan (2022, ACP) who also touched on changes in precipitation efficiency with aerosol loading.

Section 4: Just wanted to say that I think this section is great!

Figure 10: Could you add another row above this which shows the baselines radiative cooling rates for each aerosol type? It's difficult to interpret just the changes alone.

References:

Dagan, 2022 JAMES:
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022MS003368

Williams et al, 2022: https://www.nature.com/articles/s41558-022-01415-4

Lutsko et al 2022; https://www.authorea.com/doi/full/10.1002/essoar.10507822.1

Li et al, 2022: https://www.nature.com/articles/s41558-022-01400-x

Dagan 2022 ACP; https://acp.copernicus.org/articles/22/15767/2022/