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:

We thank the reviewer for their constructive and insightful suggestions, which have greatly improved the quality of our manuscript. Responses are indicated below in blue.

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).

We have included a sentence on this in lines 49-52: "Perturbations to the aerosol environment can also drive changes in the atmospheric circulation, with local, regional, and global impacts on cloud regimes (van den Heever et al., 2011; Haywood et al., 2013; Grant and van den Heever, 2014; Kim et al., 2016; Herbert et al., 2021; Williams et al., 2022; Dagan, 2022; Park and van den Heever, 2022; Leung and van den Heever, 2023).

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..."?

We have altered this sentence in lines 54-55: "Aerosols may induce changes to clouds and precipitation, however these changes to clouds and precipitation may, in turn, influence the 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).

We have now clarified that insolation does vary with the diurnal cycle in Table 1 and line 92: "[…], though the solar insolation varied according to the diurnal cycle".

While the reviewer makes a good suggestion to analyze the diurnal variation in these trends and we agree that such analysis would be interesting, we have decided not to include this in the paper. Given that the simulations were only run out for 48 hours, there are only two diurnal cycles to average over (and less if the first 7-8 hours of spin-up time are excluded). As such, we do not feel confident in presenting any diurnal trends without running the simulations out for a longer time period.

That being said, we did some brief analysis of how the trends presented in this paper vary as a function of time of day, as shown in the figure below. Due to the aforementioned small number of full diurnal cycles, the trends are fairly noisy. However, there is generally a peak in clouds, precipitation, and associated aerosol regeneration and rain-out in the evening hours (centered on ~6PM). There are not clear trends in the diurnal cycle as a function of aerosol type or concentration. Overall, the trends we discuss in the paper (e.g., more regenerated aerosol with increasing aerosol loading) are fairly consistent throughout the diurnal cycle. Again though, we emphasize that given the number of diurnal cycles represented in these simulations, we choose not to include this analysis in the paper, though such analysis would certainly be interesting to do in the future with longer-term simulations.

Figure R1. Diurnal cycle of (a-d) number of raining clouds, (e-h) number of non-raining clouds, (i-l) mean rain rate, (m-p) percent regenerated aerosol mass, and (q-t) percent rained-out aerosol mass as a function of aerosol type and concentration. Values were averaged according to time of day in 2-hour bins. The first 8 hours of simulation time were excluded due to the low number of clouds. The aerosol budget terms show the change in each term over the 5-minute interval (as opposed to the accumulated terms shown in the main text of the paper) prior to diurnally averaging.

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?

In this study, which was done as part of the $CAMP^2Ex$ campaign, we were specifically interested in cloud fields in the region around the Philippines, which is why we did not use a case study from other regions as the reviewer suggests. Certainly, similar analysis as we performed here could also be performed for other case studies and variability in trends across regions can be explored; we think that those results would be highly interesting, though would not be under the scope of this particular paper.

We used actual dropsonde observations to initialize the domain, since we are interested in understanding the formation and response of shallow clouds to the observed environments, as opposed to an idealized scenario with prescribed standard forcing. In our simulations, the environment develops as expected and forms clouds that are representative of those observed in reality; no deep convection develops, which is not surprising given that the dropsonde profile was specifically selected from a day without deep convection, i.e., it represents an environment that supports congestus clouds, but not deep convection.

Given that the type of cloud field we are simulating here is not dependent on large-scale subsidence for its formation, we have opted for this initialization from an observed profile to allow the environment and the clouds forming within it to evolve together, rather than prescribing a large-scale forcing which would force clouds back to a base state. We are of the opinion that our approach and the large-scale forcing approach suggested by the reviewer are equally valid, but our approach is more appropriate for the specific goals of this paper.

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

Yes, and we now explicitly state this in lines 100-101: “Both aerosol-radiation and microphysics-radiation interactions (liquid and ice phase) were included in the simulation.”

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

Thank you!

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...".

Yes, good point. We have replaced “ensemble” here and elsewhere in the paper with “set of simulations” or “simulations” as appropriate.

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.

RAMS, like many other cloud-resolving models, computes aerosol-radiation interactions integrated across multiple radiation bands rather than at each wavelength in order to save on computational cost (Saleeby and van den Heever, 2013). The quantities given in Table 2 are computed over the band covering 245-700nm, with a band midpoint of 472.5 nm, which we have now included in the table. We have also included information on AOD for the same radiation band in the supplementary information (Figure S1i-l).

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.

This is a good suggestion. We have now added timeseries in the supplementary information (Figure S1).

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?

Please see our above response to L82.

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.

The reviewer is correct that these are snapshots at the end of the simulation. However, we have found that the major trends in the percent aerosol mass that is regenerated or rained-out are consistent with time. We have added a sentence to this effect in lines 239-240: "While Figure 3 shows a snapshot in time, these trends are largely consistent throughout the course of the simulation (full timeseries shown in **Figure S2**)."

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?

As it is treated in RAMS, aerosol number is not conserved when multiple aerosol particles are collected by one hydrometeor. We have added clarification of this on lines 201-203: "whereas all aerosol number is not conserved when multiple aerosol particles are collected by a single hydrometeor (i.e., it is assumed that interstitial aerosol that are collected by a droplet cohere with the original activated particle)."

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.

Thank for your raising this point. We have now included the residual term in the supplementary material in Figure S2. Under the RAMS aerosol budget, only aerosol removal to the surface via wet deposition is tracked, and there is no budget term for dry deposition. We have now included a brief statement on this in lines 205-206: "treated as a residual that is lost due to dry deposition (which is represented in RAMS, but not tracked in the RAMS aerosol budget)". That being said, we emphasize that this residual term is quite small relative to all the other terms in the aerosol budget.

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

We have now done this for Figures 3, 4, 5, 6, 7.

L179: 'simulations' not 'ensemble members'

We have replaced "ensemble members" here and elsewhere in the paper with "simulations"

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?

As stated above, the snapshot at the end of the simulation appears representative of the overall trend, and we now discuss this lines 239-240: "While Figure 3 shows a snapshot in time, these trends are largely consistent throughout the course of the simulation (full timeseries shown in **Figure S2**)."

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

We meant here that the larger droplets had a proportionally smaller surface area compared to smaller droplets, but we understand that the wording was previously confusing. We have now rephrased this to say "a lower surface area-to-volume ratio" (lines 280-281).

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.

Thank you for this recommendation. We have added citations to Lutsko et al. (2022) and Li et al. (2022) in line 288. While Dagan (2022) primarily focused on precipitation efficiency under different CO_2 concentrations in conjunction with different aerosol loadings, they found generally that increasing the aerosol concentration increases the precipitation efficiency—which is not the same as what we have found here (where precipitation efficiency changes non-monotonically with aerosol loading, but generally decreases). This discrepancy may be due to a myriad of factors, including different aerosol loadings or meteorological conditions tested, and may also be due to the differences in grid spacing between the Dagan (2022) paper and this paper, where the 1km grid spacing in Dagan (2022) may not sufficiently resolve shallow cumulus clouds. We have added a sentence to the paper discussing this (lines 374-378): "Furthermore, this shows that the impact of increasing aerosol on precipitation efficiency is dependent on cloud type. This may explain differences between this study and recent studies such as Dagan (2022), which saw monotonic increases in precipitation efficiency with increasing aerosol. The latter study used a coarser grid spacing that was not resolving the shallow cumulus cloud field which we find in this work to have decreasing precipitation efficiency with increasing aerosol."

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

Thank you very much for your comments!

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.

This is a great idea! We have added panel (a) to show the baseline radiative heating rate for each aerosol type. The difference in heating rate between the different aerosol types is very small, and it is primarily the changes with increasing aerosol loading that are different as a function of aerosol type.

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/