Review 1

Comments on "Sensitivity of Marine Cloud Brightening over the Great Barrier Reef to Spatial Variability in Aerosol Forcing: A Case Study using convection-permitting model" by Wenhui Zhao, et al.

General comments:

This study examines the potential impacts of MCB in the Great Barrier Reef region by employing a convection-permitting WRF model to systematically investigate the effects of aerosol emission spatial configurations on cloud microphysical properties and radiative responses. The topic is of considerable scientific significance and practical relevance, with a clearly defined research question and a generally well-designed set of numerical experiments. The simulation results provide strong support for the authors' primary conclusion that the Twomey effect is dominant, while also demonstrating the modulating influence of meteorological conditions on cloud water path responses. Overall, the manuscript contains no major conceptual or theoretical flaws. The understanding of the underlying physical mechanisms, the numerical experiment design, and the interpretation of results are sound, and the main conclusions are valid and internally consistent within the current theoretical framework of cloud-aerosol interactions (including the Twomey effect, Albrecht effect, and related processes). However, the manuscript contains several technical, editorial, and consistency issues, which are listed in detail below. I therefore recommend Major Revision.

Specific comments:

- Line 185 Regarding the mention of "(not shown)", I suggest that if the data is crucial for demonstrating the model's skill, it should at least be included in the Supplementary Materials rather than being omitted entirely.

> As suggested, the relevant materials have now been added into Supplementary. The corresponding text has been revised too.

-Lines 239-244 The manuscript explicitly states that the nonlinear dependence of precipitation scavenging efficiency on aerosol number concentration in the WRF-Thompson aerosol-aware scheme causes more effective local self-scavenging in EXP100, resulting in a smaller domain-mean aerosol enhancement than in EXP20. However, the simulated regime is non- to weakly precipitating, with surface rain rates remaining extremely low (mostly < 0.01 mm h^{-1}; see Figure S3). Additional quantification of the dominant removal mechanism (precipitation scavenging by cloud/rain droplets versus dry deposition or coagulation) and supporting evidence for the enhanced scavenging efficiency under EXP100 would strengthen this explanation.

> We agree with the reviewers that our previous interpretation overemphasised precipitation scavenging. Given the non- to weakly precipitating nature of the simulated case, wet scavenging by precipitation is unlikely to be the dominant mechanism explaining the lower domain-mean aerosol enhancement in EXP100. We have therefore revised the manuscript to avoid attributing this behaviour primarily to precipitation scavenging efficiency.
>
> Instead, we interpret the results as arising from enhanced aerosol processing within concentrated emission plumes in the sparse-source configuration, which may lead to more efficient domain-wide dispersion and aerosol loss. Multiple processes may contribute to this behaviour, including cloud processing, dilution, possible coagulation, turbulent mixing, and deposition. In particular, processes such as coagulation may reduce aerosol number concentration without necessarily reducing aerosol mass, which could partially explain the differences between experiments.
>
> However, the current model output does not fully resolve or diagnose the relative contributions of wet scavenging, dry deposition, coagulation, and transport. This limitation has now been explicitly acknowledged in the revised manuscript.

Lines 239-244 have now been revised to

"This behaviour arises from the nonlinear response of aerosol processing and removal pathways to local aerosol number concentration in the WRF Thompson aerosol-aware microphysics scheme (Wang et al., 2010; Weston et al., 2022). In the sparse-source configuration (EXP100), aerosol emissions are concentrated in fewer locations, leading to enhanced aerosol processing within these concentrated plumes. Multiple processes may contribute to this behaviour, including cloud processing, dilution, possible coagulation, and deposition. Given the non- weakly- precipitating nature of the simulated case, coagulation within the highly concentrated aerosol plumes is likely an important, and potentially dominant, contributor, as it can efficiently reduce aerosol number concentrations near the source regions and thereby limit the enhancement of domain-mean aerosol concentrations. However, it should be noted that the current model output does not fully resolve or diagnose the relative contributions of wet scavenging, dry deposition, coagulation, and transport."

-Line 105 "All simulations are initialized at 00:00 UTC on 6 Feb 2024 and running for 3 days." However, all the observational data, satellite images, sounding charts, supplementary materials in the entire text are from 2022. Please confirm whether the actual year of this case study is 2022 or 2024?

This was a typo, the actual year is 2022. It has now been corrected.

-Line 162: "Himwari-8" change to "Himawari-8".

Done.

-Line 167: "As result" change to "As a result".

Done.

-Line 266: "aera-averaged" change to "area-averaged".

Done.

-Lines 311: "bule numbers" change to "blue numbers".

Done.

-Figure 7(d): "OPtical Depth" change to "Optical Depth".

Done.

-Figure 8(a): "Aeorosl N." change to "Aerosol N.".

Figure 8 has now been revised to exclude aerosol number concentration. In addition, the use of the term "aerosol" has been carefully checked throughout the manuscript.

-The spelling of "Cairns region" is consistent, but "Cairns" has multiple spelling errors.

Thank you for pointing this out. It has been checked throughout the manuscript and been corrected now.

-CWP: Change "g/m2" to "g m−2" in Figure 7(c), Figure 8(c), and Figure 10(a, b, c).

Done.

-rain rate: Change "mm/h" to "mm h−1" in Figure S3.

Done.

-In Section 2.1 (Line 96), the text explicitly mentions “two nested domains (denoted d01 and d02)”. However, the header for Table 1 (Line 104) lists “(d01, d02, d03)”. Howmany nested domains were actually utilized? If there are only two, please correct the Table 1 header. If a third domain (d03) exists, please provide its configuration details (e.g., resolution, location) in the text.

Two nested domains were utilized in this study. This was a typo, and Table 1 header has now been corrected. Thank you for the comments.

-The box in Figure 1 d02 is not clearly visible. It is recommended to change the color.

Thank you for the suggestion. Figure 1 has now been revised with d02 indicated in dark blue.

-Lines 37-38:

The text cites “AIMS, 2024” and “AIMS, 2026”. However, the reference list only contains an entry for “AIMS, 2026”.

The text has now been corrected from “AIMS, 2024” to “AIMS, 2026”.

-Line 286-287:

The text refers to “Figure 5c” regarding CWP. However, Figure 5 depicts CDNC. This should be changed to Figure 7c or Figure 8c

Thanks for pointing this out. It has now been revised to Figure 8c.

-Line 306:

The text lists “and (4) synthetic cloud albedo”. This is a typographical error and should be changed to (e) to match the figure panels.

Done.

-Supplementary material: The time label “02-07 09” in Figure S3 does not match the time format of the main paper. It is recommended to standardize the format.

Figure S4 (old Figure S3) has been updated with the time in the format of “%m-%d %H:%M”.

-The header colors/styles for Table 1 and Table 2 should be unified (one in blue, the other without any color).

Table 1 and Table 2 have now been revised to a consistent style/color. Thank you for your suggestion.

Review 2

General comments:

This paper reports on simulations, using WRF, of a case study in the Australian GBR region, of how cloud microphysical and macrophysical properties are affected by marine cloud brightening sea salt aerosol injections. Three injection cases are simulated, all with the same total mass of injected sea salt, but with different distributions of point sources (EXP20: 75 sources; EXP40: 12 sources; EXP100: 3 sources). The EXP20 case produces the largest cloud albedo changes, and this is shown to be mostly via the Twomey effect. The paper further breaks down the results from the EXP20 study to quantify differences in cloud brightening driven by three metrics for meteorological controls: above-cloud humidity, wind sheer below vs above cloud, and boundary layer estimated inversion strength (EIS).

In particular, the contribution of examining the effect on MCB efficacy of spray system spatial distribution is a valuable addition to the literature, and overall the analysis is technically sound. The paper is generally well-written and well structured, and the figures clear. However, I think the paper includes statements that I think are either too strong or unsubstantiated by the presented analysis, and in a few places where wording needs clarification.

Specific comments:

1. lines 21-24: It's not clear what's meant by "intensity". Is this just referring to the number of emission points? Or the rate of emission at each emission point? It's not clear from the text here that all simulations have the same total emissions, just distributed over a different number of emission points, as described in the main text. This text should be edited for better clarity.

> The "intensity" refers to the emission rate at each station. As the total emission is held constant across all simulations, the per-station emission rate increases when fewer stations are used (e.g., three stations in EXP100), and decreases when more stations are deployed (e.g., 75 stations in EXP20). Detailed values of the emission rate for each experiment are provided in Table 2.
>
> The text has been revised as follows:
>
> "A control simulation representing a non- to weakly-precipitating shallow trade-cumulus regime is compared with three MCB sensitivity experiments: a densely distributed (20 km apart), moderate emission flux scenario (EXP20), a sparsely distributed (100 km spacing), high emission flux scenario (EXP100), and an intermediate configuration (EXP40)."

2. lines 134-136: What size aerosol are emitted? This is important, given the strong dependence of aerosol activation on aerosol size.

> Thank you for this important comment. In the Thompson aerosol-aware microphysics scheme used in this study, the emitted aerosol is not represented as an explicit size-resolved aerosol distribution. Instead, aerosol perturbations are introduced as changes in the bulk number concentration of water-friendly aerosols, which are available for cloud droplet activation.
>
> As the scheme does not explicitly resolve aerosol size distributions, activation is not computed using a size-resolved Köhler framework. Rather, the number of activated cloud droplets is diagnosed using a bulk formulation, in which activation efficiency is implicitly controlled by aerosol number, vertical velocity, and thermodynamics.
>
> We acknowledge that this bulk treatment does not allow us to assess the sensitivity of activation to aerosol size, which may be important for real MCB particle design and deployment. This is noted as a limitation of the current modelling framework.

3. line 193-197: In reference to the comparison of the simulated (panels a and d) vs observed (panels b-c and e-f) cloud fields, shown in Figure 4: I'm struggling to agree with the authors that the simulations capture the cloud field "reasonably well". The cloud fraction in the study area is quite different (given later as ~7% in the simulations but what looks more like 50-60% in from Himawari obs), as is the morphology of the cloud fields (very small, broken, evenly distributed Cu in the simulation, vs broader and more variable areas of cloud cover in the obs).

This leads me to think that the boundary layer thermodynamics might be different in the model than in reality. Figure 3 shows good what looks like very agreement between the simulated and observed temperature, dew point and wind profiles, but for these clouds what will most matter is what's happening in the lowest few km of the atmosphere. In Figure 3a, the simulated and actual winds below ~800hPa are actually pretty different; in Figure 3b it's hard to see what the sounding winds are doing.

-> I'd suggest adding two additional panels to this figure, reproducing the current panels a) and b) but zoomed in on, e.g., 700mb to the surface.

-> Further, I think this statement needs to be softened. The truth is that it is really challenging to reproduce the specifics of a given cloud field, even with a perfectly initialized simulation. This can be acknowledged in the context of not overstating how well the model is reproducing reality.

> Thank you for this insightful comment. We agree that reproducing the detailed cloud fraction and morphology of a specific cloud field is inherently challenging, particularly for shallow cumulus regimes where cloud organization is highly sensitive to small-scale variability and stochastic processes.
>
> In response to the reviewer's suggestion, we have added zoomed-in panels of the sounding comparison (600 hPa to surface) in Figure 3. These additional panels show that the model reproduces the lower-tropospheric thermodynamic structure (temperature and moisture) reasonably well, while some differences remain in the wind profiles around 800 hPa.
>
> We acknowledge that the simulated cloud fraction is lower and the cloud field appears more homogeneous compared to Himawari-8 observations. Therefore, we have revised the manuscript to soften the statement regarding model performance as follows. Rather than focusing on detailed cloud morphology, we emphasize that the model captures the key thermodynamic environment and the presence of shallow cumulus conditions, which are the primary controls for aerosol–cloud interaction processes examined in this study.
>
> "Overall, the model underestimates the cloud fraction and does not fully reproduce the observed spatial organization when compared with Himawari-8 observations. In particular, much of the high-level cloud cover within the domain is underestimated or missing, leading to discrepancies in the representation of fine-scale cloud structures. Such differences are expected given the inherent difficulty in reproducing the exact realization of convective cloud fields. However, the model captures the key characteristics of the maritime shallow cloud regime visible in the true-color imagery and brightness temperature proxy, which is the primary focus of this study. In particular, the lower-tropospheric thermodynamic structure (Figure 3c and 3d) is reasonably well represented, providing a physically consistent boundary layer environment for shallow cloud formation. Therefore, although the detailed cloud morphology is not fully reproduced, the simulation is considered suitable for investigating aerosol–cloud interaction processes under shallow cumulus conditions."

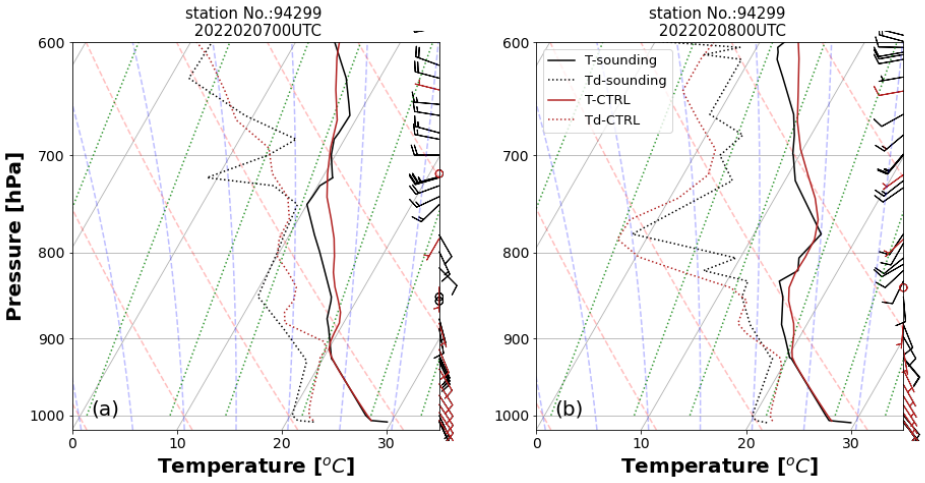

Figure R1: (a) Zoom-in comparison (surface to 600hPa) of the observed sounding profiles (black lines) from Willis Island alongside 1km WRF atmospheric profile simulations (red lines) from the nearest grid point on 07 Feb 2022 at 0000UTC. (b) same as (a), but for 08 Feb 2022 at 0000UTC.

4. Figure 5 vs Figure 6: Comparing these two figures it appears the model is only activating about 15% of the aerosol to CCN – an activation rate that seems very low. This could be due to the resolution of the model (1km in the inner domain; 5km in the outer domain), as the lower resolution could be leading to a low bias in updraft rates. Ultimately this will produce a bias in the mass of aerosol that needs to be emitted to achieve a given forcing. Perhaps in the discussion, the simulated aerosol activation rate should be put in the context of expected values for typical low marine stratocumulus and this potential source of bias acknowledged.

Thank you for this insightful comment. We agree that the initially reported activation rate (~15%) appeared low compared to expected values.

In the original analysis, aerosol concentrations were sampled at the surface level, while CDNC was derived from vertically integrated cloudy pixels. This inconsistency led to an underestimation of the activation fraction, as surface aerosol does not represent the population of particles available for activation at cloud base.

In the revised analysis, we instead diagnose aerosol number concentration in the sub-cloud layer (100–200 m below cloud base) and evaluate CDNC at cloud base (or the first in-cloud level). This approach is more physically consistent with activation theory, as it captures the aerosol population that directly participates in cloud droplet formation.

Using this updated methodology, the estimate activation fraction is approximately 30-35%, as shown in the Figure R2. This range is consistent with previous studies of marine shallow cloud regimes (e.g., McFiggans et al., 2006), particularly under moderately polluted conditions (e.g. MCB experiments) where activation fractions can exceed 30% depending on updraft strength and aerosol size distribution (McFiggans et al., 2006).

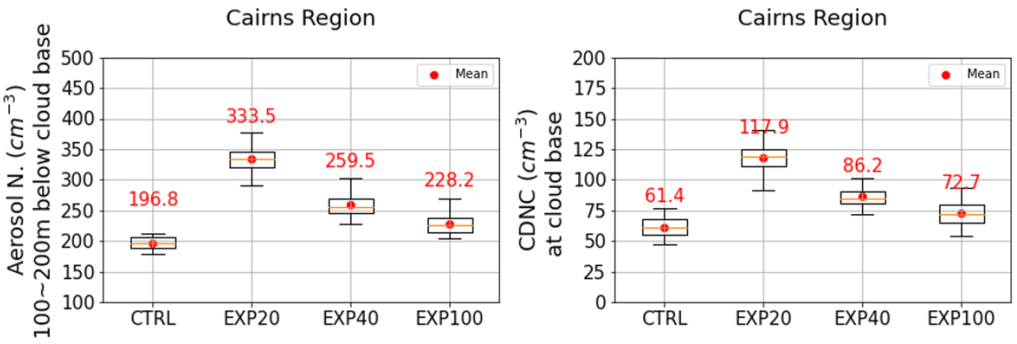

Figure R2: (Left) Aerosol distribution below cloud base from WRF experiments (Right) Cloud droplet number concentration at cloud base from WRF experiments. Time period for analysis is 08 UTC 07 Feb 2022 to 08UTC 08 Feb 2022. Red numbers are mean values of each experiment.

5. Figure 6 a-c and Figure 7a: Why show the *surface* aerosol concentration, rather than the concentration at cloud base? The variability in concentration at the surface isn't what matters; it's the variation in concentration at cloud base that will affect CDNC.

(Note that I'm asking this based on the assumption that "surface" means the value in the model's lowest layer, but maybe that's incorrect? If you're going to stick to showing 'surface' concentrations it would be good to at least define what you mean by 'surface'.)

> Thank you for this helpful comment. We originally showed aerosol concentrations at the surface to illustrate the spatial distribution and configuration of the aerosol injection in each experiment. Here, "surface" refers to the aerosol concentration in the lowest model level.
>
> We agree that aerosol concentrations near cloud base provides a more physically relevant representation of the aerosol field interacting with clouds. We have now revised Figures 7a to present aerosol distributions at cloud base (or the model level closest to cloud base, see Figure R3). Figure 6 keeps as initial to show the aerosol distribution at surface level. The corresponding text has been updated accordingly to reflect this change.

6. lines 241-244: The explanation given here of the precip scavenging efficiency at higher aerosol concentrations driving the difference in domain-mean concentrations across the different cases intuitively makes sense -- except that it is noted earlier that these are "non- to weakly-precipitating" clouds. Later, Figure S3 shows that the clouds were indeed only precipitating for a few hours in the middle of the simulation, and at very low rates (<0.01mm/hr). Can the simulated rain rate be used to quantify (even approximately) how much precip scavenging could be accounting for the differences in domain-mean aerosol concentrations across the different cases? And does this align with the simulated differences in concentrations? I think such an analysis should be included, especially given it affects the first main conclusion of the paper (lines 429-430)

More generally, is there a reason for ruling out other causes of the differences? e.g. Could coagulation or other processes be driving some of the difference? (This relates to the question of the injected aerosol size).

If a difference in precip scavenging is truly the main reason why the domain-mean aerosol concentrations differ, then the results shown here are very specific to the precip rates in this case, which brings into question whether the analysis of a single case allows one to reach a conclusion about the most efficient deployment configuration in general.

We agree with the reviewers that our previous interpretation overemphasised precipitation scavenging. Given the non- to weakly precipitating nature of the simulated case, wet scavenging by precipitation is unlikely to be the dominant mechanism explaining the lower domain-mean aerosol enhancement in EXP100. We have therefore revised the manuscript to avoid attributing this behaviour primarily to precipitation scavenging efficiency.

Instead, we interpret the results as arising from enhanced aerosol processing within concentrated emission plumes in the sparse-source configuration, which may lead to more efficient domain-wide dispersion and aerosol loss. Multiple processes may contribute to this behaviour, including cloud processing, dilution, possible coagulation, turbulent mixing, and deposition. In particular, processes such as coagulation may reduce aerosol number concentration without necessarily reducing aerosol mass, which could partially explain the differences between experiments.

However, the current model output does not fully resolve or diagnose the relative contributions of wet scavenging, dry deposition, coagulation, and transport. This limitation has now been explicitly acknowledged in the revised manuscript.

Lines 239-244 have now been revised to

"This behaviour arises from the nonlinear response of aerosol processing and removal pathways to local aerosol number concentration in the WRF Thompson aerosol-aware microphysics scheme (Wang et al., 2010; Weston et al., 2022). In the sparse-source configuration (EXP100), aerosol emissions are concentrated into fewer locations, leading to enhanced aerosol processing within these concentrated plumes. Multiple processes may contribute to this behaviour, including cloud processing, dilution, possible coagulation, and deposition. Given the non- weakly- precipitating nature of the simulated case, coagulation within the highly concentrated aerosol plumes is likely an important, and potentially dominant, contributor, as it can efficiently reduce aerosol number concentrations near the source regions and thereby limit the enhancement of domain-mean aerosol concentrations. However, it should be noted that the current model output does not fully resolve or diagnose the relative contributions of wet scavenging, dry deposition, coagulation, and transport."

7. lines 321-323: I have to object to the assertion that the cloud albedo exhibited "pronounced increases" in the injection cases relative to the CTRL case. The increases are well within the range of variability in the CTRL case (Figure 8e); in Figure 7e, only the EXP20 case has a cloud albedo that is notably and consistently higher with aerosol injection than without.

We thank the reviewer for this important clarification. We agree that the term "pronounced increases" may overstate the magnitude and robustness of the cloud albedo response across all experiments. It should also be noted that the statistics shown in Figure 8 have now been updated following the revised cloudy-mask methodology, in which cloudy conditions are defined using cloud optical depth (COD) > 1, as requested by other reviewers. The updated analysis still indicates relatively modest albedo responses compared with the CDNC changes, thus the overall conclusions remain consistent.

Accordingly, we have revised the wording in the manuscript to describe the cloud albedo response more cautiously and quantitatively.

We would also like to clarify that, although the domain-mean cloud albedo response is relatively modest, the shortwave cloud radiative effect (CRE) analysis introduced in the revised manuscript demonstrates more substantial radiative perturbations associated with aerosol injection. This difference partly arises because the synthetic cloud albedo in this study is diagnostically derived from cloud optical properties that primarily depend on cloud water path (CWP) and cloud droplet number concentration (CDNC), rather than being directly archived from the model output. In contrast, CRE provides a more direct measure of the net shortwave radiative impact associated with aerosol-induced

cloud modifications, which is a direct output from the model. We retain the synthetic albedo analysis because CRE is only physically meaningful during local daytime, whereas synthetic albedo can also provide information on cloud optical properties during nighttime.

To avoid overstating the implications of the albedo analysis, we have revised the manuscript to focus primarily on the simulated CRE response and the process-level sensitivity of cloud radiative properties to aerosol perturbations, rather than emphasizing large albedo increases alone.

8. lines 431-432: This is an incomplete sentence.

Also, again I think that other than (marginally) in the EXP20 case, the cloud albedo increases were modest to (EXP100) negligible.

Thank you for pointing this out. The sentence has been revised as follow:

"Substantial increases in CDNC and cloud optical depth lead to brighter clouds and higher mean albedo under additional aerosol perturbations."

9. line 441, Conclusions: "The findings confirm that radiative forcing can be meaningfully enhanced under suitable conditions through spatially coherent aerosol seeding. As such, strategic deployment of MCB during favorable weather regimes holds promise as a targeted intervention to mitigate extreme heat exposure over sensitive marine ecosystems like the GBR".

I feel strongly that these statements cannot be made. No radiative forcing calculations are shown. The paper later asserts that the cloud albedo change from EXP20 vs CNTRL could produce a forcing "potentially on the order of several tens of W m^{-2}" but I don't see how this is the case. In EXP20, the cloud albedo increases to 0.51 from 0.48. If there was 100% cloud cover this would, ballpark, result in an increase in reflected solar flux of ~10 W/m2 (downwelling 340 W/m2*(0.51-0.48)=10.2 W/m2). But cloud cover isn't 100% -- it's, in this case, 7%.

Further: Being able to say forcing is *meaningfully* enhanced, and that MCB could actually mitigate extreme heat exposure in the GBR, would require showing that the resulting forcing would be sufficient to produce ocean cooling.

All of this is well beyond the scope of the paper – but so is the assertion.

Unless the authors want to add this analysis, they can only assert what the paper has actually shown, which is articulated nicely on lines 446-495, and is of sufficient scientific value to stand on its own.

We thank the reviewer for this thoughtful and constructive comment. We agree that our original wording may have overstated the broader climatic implications of the simulated radiative responses. In response, we have revised the conclusions to better align with the actual scope and findings of the study.

Following this comment, and in conjunction with the Comment 7, we have now added analysis of the shortwave cloud radiative effect (CRE) associated with the aerosol perturbation experiments. In addition, the cloud mask methodology has been updated following reviewer recommendations to define cloudy conditions using cloud optical depth (COD) > 1, which represents optically significant clouds and is commonly used to improve robustness in cloud detection and radiative analyses (Heidinger et al., 2020). The corresponding analyses in Figures 7 and 8 have been updated accordingly, although the overall statistical behaviour and conclusions remain largely unchanged.

As shown in the Figures R3 and R4 (updated Figures 7 and 8), the aerosol-seeding sensitivity experiments exhibit enhanced shortwave cloud radiative effects relative to the control simulation,

indicating increased reflection of incoming solar radiation due to aerosol-induced cloud brightening. The largest CRE enhancement occurs during periods of high solar zenith angle near local midday, when incoming solar radiation is strongest. While the simulated case study covers only a limited daytime period, the results consistently demonstrate measurable increases in cloud reflectivity and shortwave radiative cooling associated with additional aerosols.

However, we agree with the reviewer that our simulations alone are insufficient to determine whether these radiative perturbations would be large enough to produce meaningful ocean cooling or mitigate marine heat stress over the GBR. Such conclusions would require substantially broader analyses, including coupled atmosphere–ocean modelling, longer temporal integrations, and regional energy budget assessments, all of which are beyond the scope of the present study.

Accordingly, we have revised the manuscript to avoid overstating the implications of the results. The conclusions now focus specifically on the demonstrated process-level sensitivity of cloud microphysical and radiative responses to aerosol seeding configuration and meteorological conditions, rather than asserting direct mitigation of extreme heat exposure over the GBR.

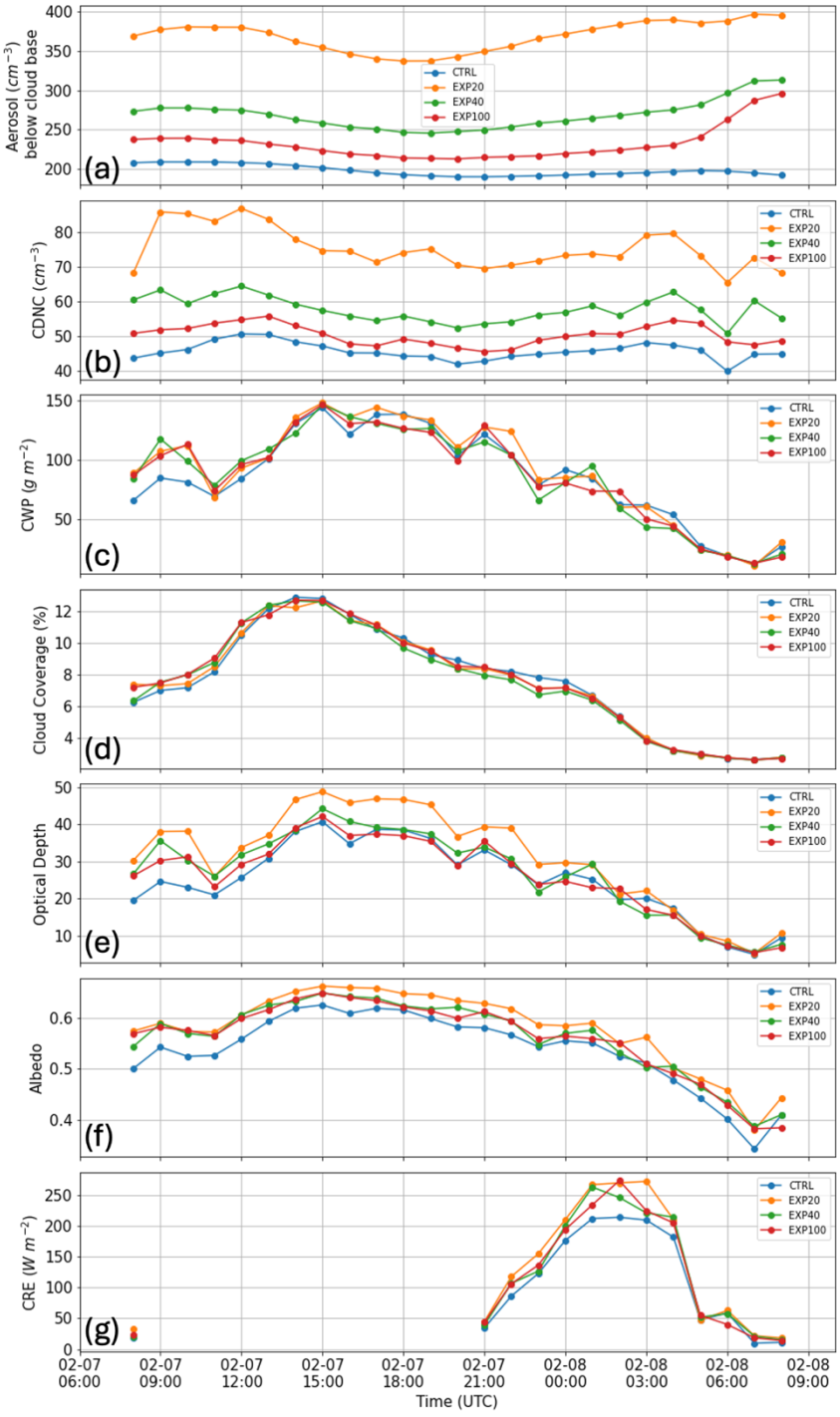

Figure R3: Time series of domain-averaged (a) aerosol number concentration below cloud base, (b) CDNC, (c) cloud water path, (d) Cloud Coverage, (e) Optical depth, (f) synthetic cloud albedo, and (g) cloud radiative effect over the Cairns Region. Note that all variables are averaged over cloudy grid points.

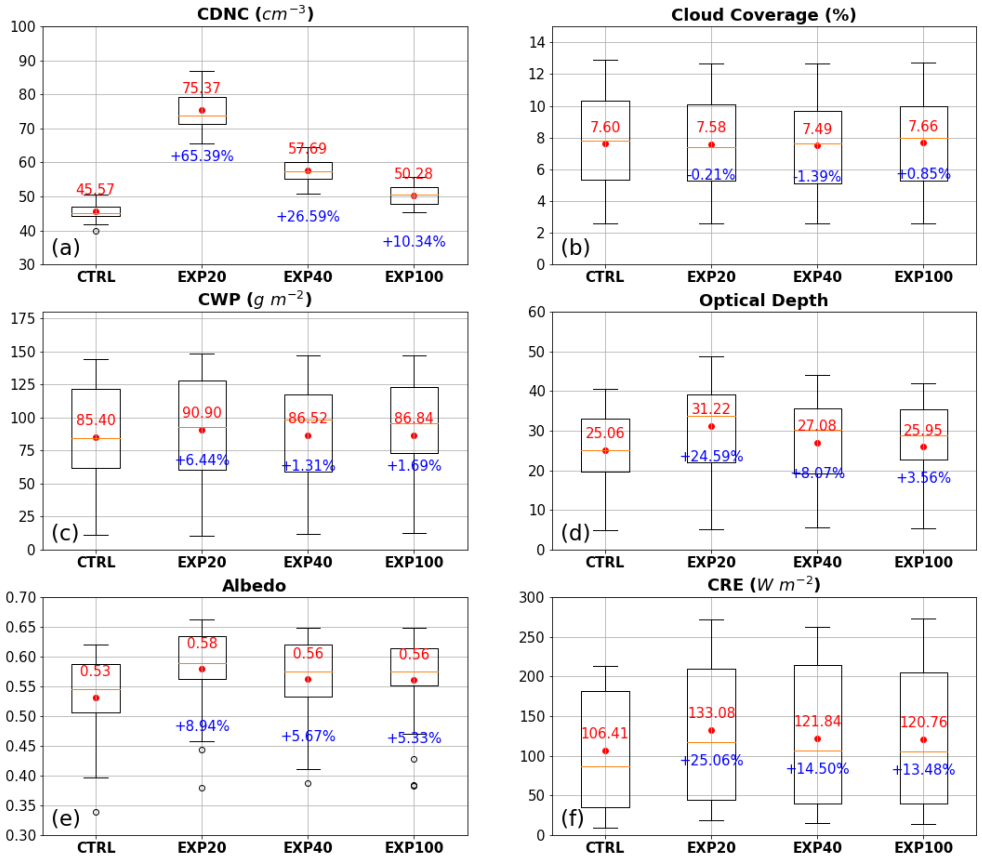

Figure R4: Boxplots of cloud properties over Cairns Region from CTRL and sensitivity experiments. Time period for analysis is 08UTC 07 Feb to 08 UTC 08 Feb 2022. Red numbers are mean values of each experiment, and blue numbers are differences with respect to CTRL experiment.

Technical comments:

- lines 68-69: A classic paper that should probably also be cited here is the Stevens and Feingold (2009; https://doi.org/10.1038/nature08281) paper that's also cited in another context later in the paper.

  The reference has been cited as suggested.

- line 184: skilfully -> skillfully

  Done.

- line 184-185: "The control simulation skilfully simulated the evolution of the synoptic-scale mean surface level pressure and surface winds (not shown)." The evidence showing this should be included in the Supplemental data/file.

As suggested, the relevant materials have now been added into Supplementary. The corresponding text has been revised too.

- Figure 5: It appears that none of the CDNC values in any of the panels exceed 150/cm3, so why not set the colorbar range from 0-150/cm3 (or maybe even 0-125/cm3)?

  The colorbar range has now been revised to 0-140/cm^3.

- line 231: “In the contrast, …” -> “In contrast, the …”

  Done.

- line 233: “exhibited” -> “exhibits” (for consistency present tense used elsewhere)

  It has now been corrected.

- line 435: “mid-level” is a bit ambiguous; I’d say “above-cloud”

  Thanks for your suggestion. It has been revised to “above-cloud”.

- line 524: this is the first use of LWP; for consistency with earlier usage, suggest changing to CWP

  Revised as suggested.

- Figure 9: The caption says that “colors with black dots indicate *significant* differences”. Is this correct? I suspect that this should read *insignificant* differences, given that the back dots correspond to when there is near-zero delta-density.

  Thank you for your comment. We confirm that colors with black dots indicate significant differences. It can be observed that most of these black dots are located in darker-colored regions, which represent larger differences in delta-density.

References:

McFiggans, G., Artaxo, P., Baltensperger, U., Coe, H., Facchini, M.C., Feingold, G., Fuzzi, S., Gysel, M., Laaksonen, A., Lohmann, U. and Mentel, T.F.: The effect of physical and chemical aerosol properties on warm cloud droplet activation. *Atmospheric Chemistry and Physics*, *6*(9), pp.2593-2649, 2006.

Heidinger, A. K., Foster, M. J., Walther, A., and Zhao, X.: NOAA Enterprise Cloud Mask Algorithm Theoretical Basis Document (ATBD), NOAA/NESDIS, 2020.

Review 3

In their manuscript, the authors run a convection-permitting simulation of MCB over the Great Barrier Reef and show that a dense network of sprayers using moderate emission rates is substantially more effective at dispersing aerosol and brightening clouds than a sparse network of high-emission sprayers. The core idea is important and worthy of publication. Unfortunately, much of the supporting commentary requires major revisions before I can recommend the manuscript be published.

Major comments:

A) Interpretation of aerosol "scavenging": As the authors show elsewhere, precipitation (and therefore wet deposition) is not particularly high in their simulations. Increasing aerosol would also tend to decrease precipitation. Denser aerosol plumes therefore would not be expected to result in greater wet scavenging; this explanation for the differences between EXP20 and EXP100 is implausible. More likely, the aerosols are coagulating more in the sparser-but-denser plumes, reducing number but not necessarily mass. An easy way to check this would be to look at the total or average mass concentration of aerosol between the experiments in addition to number concentration.

> We agree with the reviewers that our previous interpretation overemphasised precipitation scavenging. Given the non- to weakly precipitating nature of the simulated case, wet scavenging by precipitation is unlikely to be the dominant mechanism explaining the lower domain-mean aerosol enhancement in EXP100. We have therefore revised the manuscript to avoid attributing this behaviour primarily to precipitation scavenging efficiency.
>
> Instead, we interpret the results as arising from enhanced aerosol processing within concentrated emission plumes in the sparse-source configuration, which may lead to more efficient domain-wide dispersion and aerosol loss. Multiple processes may contribute to this behaviour, including cloud processing, dilution, possible coagulation, turbulent mixing, and deposition. In particular, processes such as coagulation may reduce aerosol number concentration without necessarily reducing aerosol mass, which could partially explain the differences between experiments.
>
> However, the current model output does not fully resolve or diagnose the relative contributions of wet scavenging, dry deposition, coagulation, and transport. This limitation has now been explicitly acknowledged in the revised manuscript.
>
> Lines 239-244 have now been revised to
>
> "This behaviour arises from the nonlinear response of aerosol processing and removal pathways to local aerosol number concentration in the WRF Thompson aerosol-aware microphysics scheme (Wang et al., 2010; Weston et al., 2022). In the sparse-source configuration (EXP100), aerosol emissions are concentrated into fewer locations, leading to enhanced aerosol processing within these concentrated plumes. Multiple processes may contribute to this behaviour, including cloud processing, dilution, possible coagulation, and deposition. Given the non- weakly- precipitating nature of the simulated case, coagulation within the highly concentrated aerosol plumes is likely an important, and potentially dominant, contributor, as it can efficiently reduce aerosol number concentrations near the source regions and thereby limit the enhancement of domain-mean aerosol concentrations. However, it should be noted that the current model output does not fully resolve or diagnose the relative contributions of wet scavenging, dry deposition, coagulation, and transport."

B) Overinterpretation of aerosol-cloud interaction results: The novelty of the current work lies in the exploration of the denser versus sparser sprayer setup, not in exploring ACI under shallow cumulus conditions. The general finding that adjustments to the Twomey effect would tend to promote cloudiness under moist and precipitating conditions and decrease cloudiness under dry and non-precipitating conditions is very well established at this point. The results and discussion should be rewritten to better highlight which areas of the study are a novel contribution versus which are reinforcing the preexisting consensus. The abstract already does a nice job of this!

> Thank you for this helpful comment. We fully agree that the primary novelty of this study lies in examining the impact of spatial variability in aerosol injection (i.e., denser versus sparser sprayer configurations), rather than in establishing the general behaviour of ACI under shallow cumulus conditions. Our intention was not to present these behaviors as novel findings, but rather to provide context for interpreting the sensitivity of cloud responses to aerosol forcing configurations.
>
> In response, we have revised the Results and Discussion sections to more clearly distinguish between:
>
> (1) well-established ACI mechanisms that are consistent with previous studies, and
>
> (2) the novel aspects of this work, namely the sensitivity of cloud and radiative responses to the spatial distribution of aerosol injection in a convection-permitting framework.
>
> We have also reduced interpretative emphasis on well-known processes and instead highlight how different sprayer configurations modulate the magnitude, spatial extent, and efficiency of these responses. We appreciate the reviewer's suggestion, which has helped improve the clarity and positioning of the manuscript.

C) Analysis of cloud and radiation variables: A number of issues arise with the treatment of cloud properties, including an inappropriate means of calculating cloud fraction. See specific comments below. I would also encourage the authors to consider evaluating cloud radiative effect (CRE; all sky minus clear sky net fluxes) directly.

> We thank the reviewer for this thoughtful and constructive comment. Following this comment, and in conjunction with the Specific Comment 12, we have now added analysis of the shortwave cloud radiative effect (CRE) associated with the aerosol perturbation experiments. In addition, the cloud mask methodology has been updated following reviewer recommendations (Specific Comment 10) to define cloudy conditions using cloud optical depth (COD) > 1, which represents optically significant clouds and is commonly used to improve robustness in cloud detection and radiative analyses (Heidinger et al., 2020). The corresponding analyses in Figures 7 and 8 have been updated accordingly, although the overall statistical behaviour and conclusions remain largely unchanged.
>
> As shown in the Figures R1 and R2 (updated Figures 7 and 8 in the revised manuscript), the aerosol-seeding sensitivity experiments exhibit enhanced shortwave cloud radiative effects relative to the control simulation, indicating increased reflection of incoming solar radiation due to aerosol-induced cloud brightening. The largest CRE enhancement occurs during periods of smaller solar zenith angle near local midday, when incoming solar radiation is strongest. While the simulated case study covers only a limited daytime period, the results consistently demonstrate measurable increases in cloud reflectivity and shortwave radiative cooling associated with additional aerosols.

However, we agree with the reviewer that our simulations alone are insufficient to determine whether these radiative perturbations would be large enough to produce meaningful ocean cooling or mitigate marine heat stress over the GBR. Such conclusions would require substantially broader analyses, including coupled atmosphere–ocean modelling, longer temporal integrations, and regional energy budget assessments, all of which are beyond the scope of the present study.

Accordingly, we have revised the manuscript to avoid overstating the implications of the results. The conclusions now focus specifically on the demonstrated process-level sensitivity of cloud microphysical and radiative responses to aerosol seeding configuration and meteorological conditions, rather than asserting direct mitigation of extreme heat exposure over the GBR.

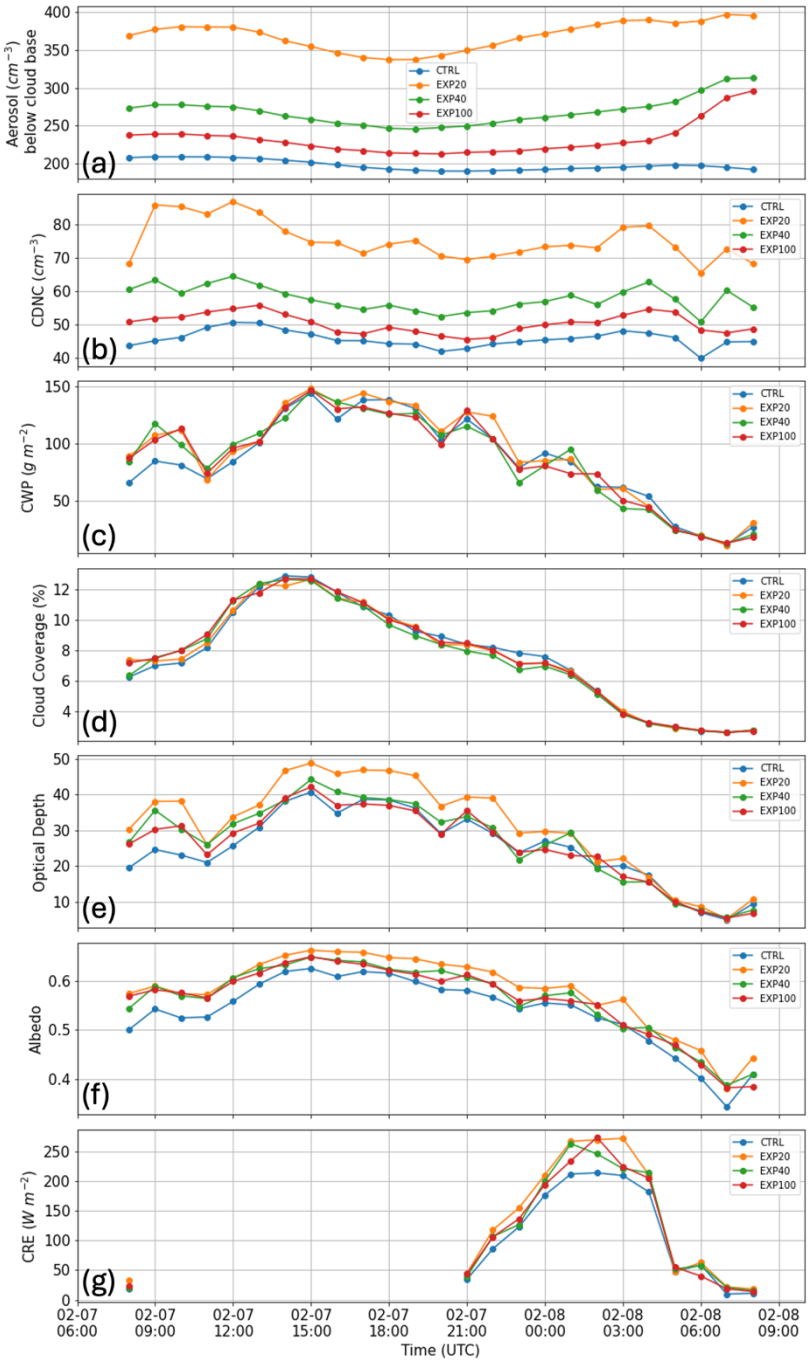

Figure R1: Time series of domain-averaged (a) aerosol number concentration below cloud base, (b) CDNC, (c) cloud water path, (d) Cloud Coverage, (e) Optical depth, (f) synthetic cloud albedo, and (g) cloud radiative effect over the Cairns Region. Note that all variables are averaged over cloudy grid points.

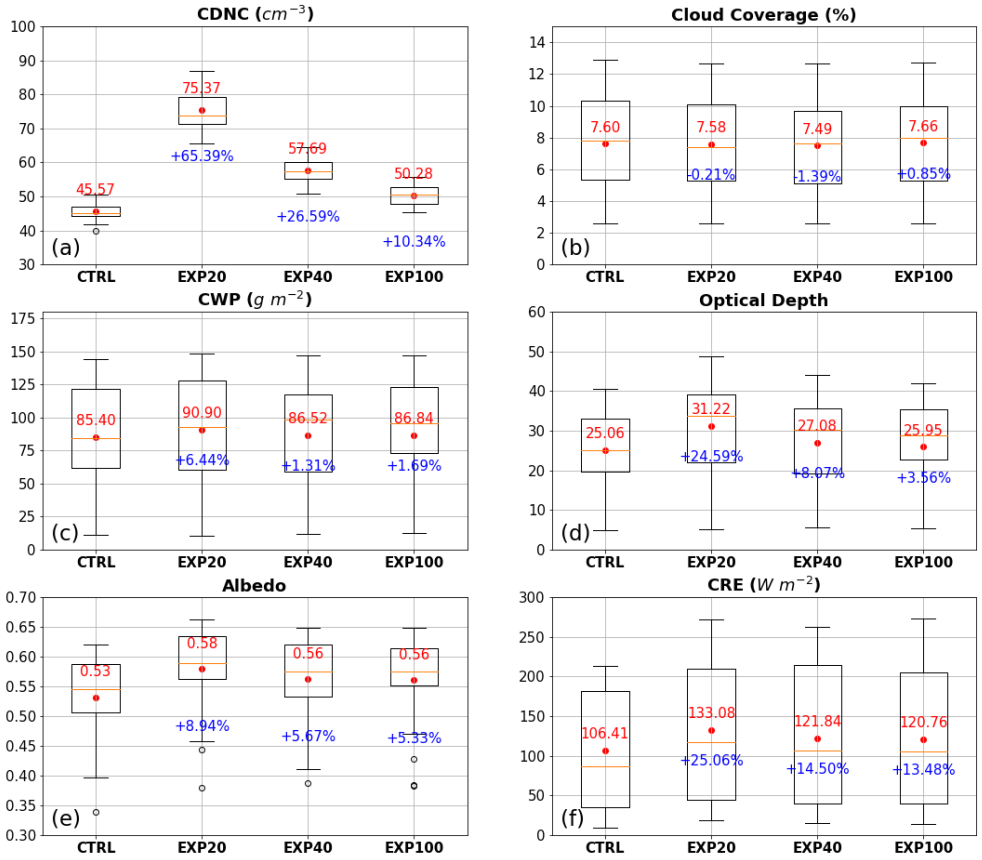

Figure R2: Boxplots of cloud properties over Cairns Region from CTRL and sensitivity experiments. Time period for analysis is 08UTC 07 Feb to 08 UTC 08 Feb 2022. Red numbers are mean values of each experiment, and blue numbers are differences with respect to CTRL experiment.

Specific comments:

1. Title: "A Case Study using a Convection-Permitting Model"

   Revised as suggested.

2. Line 46: The most current review of MCB science is: Feingold, G., Ghate, V. P., Russell, L. M., Blossey, P., Cantrell, W., Christensen, M. W., Diamond, M. S., Gettelman, A., Glassmeier, F., Gryspeerdt, E., Haywood, J., Hoffmann, F., Kaul, C. M., Lebsock, M., McComiskey, A. C., McCoy, D. T., Ming, Y., Mülmenstädt, J., Possner, A., Prabhakaran, P., Quinn, P. K., Schmidt, K. S., Shaw, R. A., Singer, C. E., Sorooshian, A., Toll, V., Wan, J. S., Wood, R., Yang, F., Zhang, J., and Zheng, X.: Physical science research needed to evaluate the viability and risks of marine cloud brightening, Science Advances, 10, eadi8594, doi:10.1126/sciadv.adi8594, 2024.

   Thank you for this reference. It has now been cited properly in the revised manuscript.

3. Line 52: As the authors note elsewhere, secondary indirect effects/cloud adjustments can also decrease cloudiness by increasing dry air entrainment

   Thank you for this helpful comment. We agree that aerosol-induced cloud adjustments are not unidirectional. While increased CCN can suppress precipitation and potentially enhance cloud lifetime (the so-called cloud lifetime effect), previous studies have also shown that enhanced entrainment of dry air may reduce cloud liquid water and cloud fraction under certain conditions.

   We have added the text as follows to acknowledge this:

   "However, aerosol-induced cloud adjustments are complex and may also act in the opposite direction: enhanced entrainment of dry air can reduce cloud liquid water and cloud fraction under certain environmental conditions."

4. Section 2.1: Please clarify if the nesting is one-way or two-way

   The simulations were conducted using one-way nesting, in which the parent domain provides boundary conditions to the nested domain without feedback. This configuration ensures that the large-scale environment remains unaffected by the inner-domain processes. The manuscript has been revised to clarify this.

5. Line 195: Can the authors elaborate on the "key features of maritime shallow clouds, visible in the true-color imagery" here? In my read of Figure 4, the agreement is not at all obvious

   Thank you for this comment. Specifically, the "key features of maritime shallow clouds" is referring to the spatial distribution characteristics of scattered and broken cloud structures visible from true-color image and the cloud top temperature range indicated by brightness temperature proxy, which are reasonably well captured by the model. We acknowledge that the model underestimates the total cloud fraction and does not fully reproduce the observed spatial organization when compared with Himawari-8 observations. In particular, much of the high-level cloud cover within the domain is underestimated or missing, leading to discrepancies in the representation of fine-scale cloud structures. Such differences are expected given the inherent difficulty in reproducing the exact realization of convective cloud fields. We have clarified this in the revised manuscript and avoided overstating the level of agreement in cloud morphology as follows:
   "Overall, the model underestimates the cloud fraction and does not fully reproduce the observed spatial organization when compared with Himawari-8 observations. In particular, much of the high-level cloud cover within the domain is underestimated or missing, leading to discrepancies in the representation of fine-scale cloud structures. Such differences are expected given the inherent difficulty in reproducing the exact realization of convective cloud fields. However, the model captures the key characteristics of the maritime shallow cloud regime visible in the true-color imagery (Figure 4c and 4f) and brightness temperature proxy, which is the primary focus of this study. In particular, the lower-tropospheric thermodynamic structure (Figure 3c and 3d) is reasonably well represented, providing a physically consistent boundary layer environment for shallow cloud formation. Therefore, although the detailed cloud morphology is not fully reproduced, the simulation is considered suitable for investigating aerosol–cloud interaction processes under shallow cumulus conditions."

6. Lines 241-242: Precipitation scavenging efficiency increasing nonlinearly with aerosol concentration is not what I would expect… presumably this is referring to scavenging by impaction of aerosol on raindrops? Nucleation scavenging should be a much larger sink and will decrease in efficiency with increasing aerosol.

Please see response to major comment A.

7. Line 242: What is meant by “self-scavenging” here? Dry scavenging? Are the aerosol truly being lost (in terms of mass)? This sounds more like coagulation to me.

   Please see response to major comment A.

8. Figure 6: What are the “-C” and “-U” after EXP40 and EXP100, respectively?

   These were typos, which have now been revised.

9. Line 274: It does not seem like you have shallow stratocumulus cloud regimes here… cumulus?

   This was a typo. It has now been revised.

10. Lines 296-297: This is an inappropriate way to define cloud fraction, if I’m correctly interpretating this as dealing with 3D outputs, and results in values that seem wrong on their face (e.g., Figure S2 definitely shows cloud fraction is much higher than 7%!). The fraction of columns with cloud optical thickness or liquid water path about a given threshold (e.g., COT > 1 or COT > 3) would be a better choice here.

    We thank the reviewer for this helpful comment. We agree that the previous definition of cloud fraction based on 3-D cloud fields was not ideal for this analysis. Following the reviewer’s suggestion, we have revised the cloud coverage calculation using a column-based approach. Specifically, a grid column is now classified as cloudy when the cloud optical depth (COD) exceeds 1, and cloud coverage is calculated as the fraction of such cloudy columns within the analysis domain (Figure R3).
    Using this revised definition, the domain-mean cloud coverage remains relatively low, generally around 8-12% (Figure R1). As an example in Figure R3, although the cloud field appears spatially widespread, the clouds are highly scattered and broken, with many small cloud elements. As a result, the visual impression of broad cloud occurrence does not directly translate into a large fractional area of optically significant cloud.
    We have updated the corresponding analyses and figures using this revised cloud-mask method. The manuscript has also been revised to clarify the definition of cloud coverage and to avoid confusion between visually apparent cloud occurrence and quantitatively defined cloud fraction.

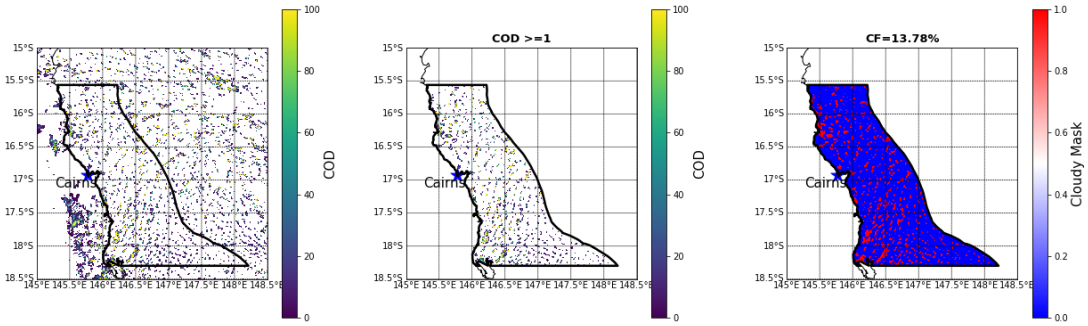

Figure R3: (Left) Example of spatial distribution of cloud optical depth (COD) at 1400UTC 07 Feb 2022 derived from WRF, (middle) Threshold of 1 and Cairns region applied to COD distribution, and (right) cloudy mask defined by COD >1 over the Cairns Region.

11. Lines 313-315: I'm surprised COT needs to be estimated instead of output directly by WRF... it probably doesn't matter, but you do have the information required to calculate COT explicitly

    We agree that WRF provides the necessary variables to calculate cloud optical thickness (COT) explicitly using cloud liquid water content and effective radius. However, effective radius and/or COT are not a standard WRF output and was not archived in this study. In this study, COT is diagnosed using a bulk parameterisation based on cloud water path (CWP) and cloud droplet number concentration (Nc), following a commonly used approximation derived from adiabatic cloud assumptions (Zhang et al., 2005).
    This formulation provides a consistent estimate of COT from the available model outputs and is appropriate for assessing relative changes in optical thickness associated with aerosol-induced changes in CWP and Nc. We have revised the manuscript to clarify that COT is diagnosed from CWP and Nc, rather than output directly by WRF.

12. Lines 329-330: Why not calculate CRE and show this?

    Please see response to major comment C.

13. Figure 8: Something seems off in this figure. For example, in (f), the EXP100U

    The labels have been revised now.

14. Figure 8: Why are EXP40 and EXP100 given U's in their names?

    These were typo, which have now been revised.

15. Figure 9 and related discussion: This figure does not show your results very clearly, at least in my read. Figure 10 is much easier to interpret and there doesn't seem to be anything Figure 9 is doing that Figure 10 doesn't already show more clearly. Consider removing Fig. 9; perhaps you can add a third bin to Fig. 10 if you wanted to show finer-grained variability?

    We appreciate this suggestion. However, Figures 9 and 10 are intended to convey different aspects of the analysis and therefore we prefer to keep them separate. Figure 9 is used to illustrate the density distribution and methodology underlying the regime classification thresholds, while Figure 10 presents the resulting statistical characteristics based on the thresholds identified in Figure 9. To improve readability, we will revise the captions and strengthen the cross-reference between the two figures in the main text.

16. Section 3.4: I would hesitate to use words like "marked" and "clear" here, these results really are not that dramatic... which makes sense, given elsewhere you discuss how the aerosol is not changing the cloud macrophysics much!

    The manuscript has been carefully reviewed throughout and revised accordingly.

17. Lines 438-440: I don't disagree here, but the results in this paper alone suggest the meteorology/cloud adjustments really don't matter much here...

    We agree that the potential for marine cloud brightening over the GBR depends not only on aerosol loading and composition, but also strongly on the prevailing meteorological conditions. In fact, one of the key findings of this study is that the cloud response varies substantially under different humidity, wind shear, and lower-tropospheric stability regimes. For example, CWP responses were

found to differ markedly between moist and dry free-tropospheric environments, highlighting the important role of meteorological control on aerosol–cloud interactions. We acknowledge the limitation of case studies, however this result is physically consistent with the mechanism proposed by Ackerman et al. (2004).

18. Line 471: "Our work extends this understanding to shallow maritime cumulus" is greatly overstated. There is an extensive literature at this point on cloud adjustments outside the core stratocumulus regimes. Chen et al. (2024) is one recent, prominent example that has essentially identical findings about cloud adjustments as those presented here: Chen, Y., Haywood, J., Wang, Y., Malavelle, F., Jordan, G., Peace, A., Partridge, D. G., Cho, N., Oreopoulos, L., Grosvenor, D., Field, P., Allan, R. P., and Lohmann, U.: Substantial cooling effect from aerosol-induced increase in tropical marine cloud cover, Nature Geoscience, 17, 404-410, 10.1038/s41561-024-01427-z, 2024.

    Thank you for this important comment. We have revised the text accordingly to avoid overstating the novelty and to clarify the specific contribution of this work as follows.

    "Our results are consistent with previous studies based on satellite data demonstrating aerosol-induced cloud adjustments in tropical marine cloud regimes (e.g., Chen et al., 2024). Building on this understanding, we investigate the sensitivity of these responses to the spatial variability of aerosol forcing using convection-permitting simulations, emphasizing that MCB will be most efficient when timed with supportive weather regimes."

19. Lines 478-479: My read of the state of the ACI literature is that there's a general consensus that LWP adjustments on net are relatively small and offset a fraction of the Twomey effect but cloud fraction adjustments are potentially quite large, maybe even rivaling or surpassing Twomey in importance. The revised Rosenfield et al. (2019) paper does show limited LWP changes, but what they originally attributed to LWP are actually cloud fraction increases (at least in their revised analysis).

    We thank the reviewer for this important clarification and agree that the broader aerosol–cloud interaction (ACI) literature suggests a more complex picture regarding cloud macrophysical adjustments. While aerosol-induced changes in liquid water path (LWP) are often found to be small and may partially offset the Twomey effect, cloud fraction (CF) adjustments can be substantial in some cloud regimes.

    However, it is important to note that the present study examines a weakly precipitating shallow trade-cumulus regime over the Great Barrier Reef, which differs from many previous studies that focused on marine stratocumulus or more extensive marine cloud fields. Rosenfeld et al. (2019) showed that aerosol-induced responses occur in both marine stratocumulus and cumulus regimes, but that the magnitude of the response weakens when transitioning from marine stratocumulus to cumulus clouds. They further showed that cloud fraction responses are strongest in deeper clouds and become weaker in shallower cloud fields.

    In our simulations, aerosol perturbations resulted in clear increases in CDNC, cloud optical depth, and cloud albedo, but only minor changes in both LWP and CF. This suggests that the radiative response in this case is primarily associated with the Twomey effect. We also acknowledge that the present modelling framework is not designed to explicitly assess cloud lifetime effects, which may contribute substantially to CF adjustments. The simulations focus on the short-term response of clouds to aerosol perturbations during a single case study and therefore do not provide a robust basis for quantifying aerosol impacts on cloud persistence or cloud lifetime adjustments. Consequently,

while cloud lifetime effects may be important in other cloud regimes, our simulations neither provide strong evidence for nor rule out their importance in the present trade-cumulus case.

Our original wording may have overly emphasized the relatively weak LWP response without sufficiently acknowledging the potential importance of cloud fraction and cloud lifetime adjustments. Accordingly, we have revised the manuscript text to avoid overstating the role of weak LWP responses alone and to more accurately reflect the current understanding of ACI processes and their associated uncertainties.

20. Line 529: Do the authors consider local MCB to be "geoengineering"? I don't necessarily disagree, but the "intervention" phrasing used in the introduction is perhaps more accurate.

    The "geoengineering" has now been revised to "climate intervention".

21. Data availability statement: Where can readers find the model outputs needed to reproduce the analyses in the text?

    WRF outputs of all model simulations in this study have been uploaded to figshare for public access. References and Data Availability sections have now been updated accordingly in the revised manuscript to include follows:

    Wenhui Zhao, Huang, Y., Siems, S. T., Harrison D.: WRF outputs of CTRL simulation [Dataset]. University of Melbourne. https://doi.org/10.26188/30071935, 2026a.

    Wenhui Zhao, Huang, Y., Siems, S. T., Harrison D.: WRF outputs of EXP20 simulation [Dataset]. University of Melbourne. https://doi.org/10.26188/32592879, 2026b.

    Wenhui Zhao, Huang, Y., Siems, S. T., Harrison D.: WRF outputs of EXP40 simulation [Dataset]. University of Melbourne. https://doi.org/10.26188/32593914, 2026c.

    Wenhui Zhao, Huang, Y., Siems, S. T., Harrison D.: WRF outputs of EXP100 simulation [Dataset]. University of Melbourne. https://doi.org/10.26188/32593482, 2026d.

22. Supplemental figures: Consider bringing these into the main text

    All figures in main text and supplementary have now been updated.

References:

Zhang, Y., Stevens, B., and Ghil, M.: On the diurnal cycle and susceptibility to aerosol concentration in a stratocumulus-topped mixed layer, Quart. J. Roy. Met. Soc., 131, 1567–1583, s2005.

Heidinger, A. K., Foster, M. J., Walther, A., and Zhao, X.: NOAA Enterprise Cloud Mask Algorithm Theoretical Basis Document (ATBD), NOAA/NESDIS, 2020.