

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 $< 0.01 \text{ mm h}^{-1}$; 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)”. How many 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.