

Response to Reviewer 1

Legend: Reviewer comments - black, author responses - blue, manuscript text changes - green

Lucas et al. present an interesting study in which they use Langevin particle modeling applied to realistic LES outputs to investigate the importance of the emission spreading rate of sprayers (ships) for Twomey forcing. They further examine the influence of variability in aerosol properties, including the number of sprayers and their motion type, aerosol decay timescales and age, concentration bins, and background aerosol concentrations, as well as meteorological conditions. Their results show that the velocity and type of sprayer motion have little influence on Twomey forcing. Assuming that cloud adjustments are relatively small, they further demonstrate that the effect of the spreading rate on Twomey forcing is limited, with the forcing being strongly controlled by aerosol lifetime. Their findings suggest that the common assumption in GCMs of an infinitely fast spreading rate could result in a 10–200% overestimation of Twomey forcing. Overall, I found the manuscript to be well written, and the methodology to be sound. I have only a few minor comments that can be addressed easily by the authors.

We would like to thank the reviewer for their time, positive feedback, and comments that will help improve the clarity of our final draft.

Minor comments:

1. Line 43: Goren et al. (2025) also attributed the co-variability to two factors: (1) large-scale meteorological conditions, which influence cloud properties such as cloud liquid water path (LWP) and cloud droplet number concentration (Nd), causing these variables to covary simultaneously and inversely; and (2) microphysical processes associated with cloud development, during which an increase in LWP is generally accompanied by a decrease in Nd. Goren et al. (2025): Co-variability drives the inverted-V sensitivity between liquid water path and droplet concentrations, *Atmos. Chem. Phys.*, 25, 3413–3423. <https://doi.org/10.5194/acp-25-3413-2025>

We have added a citation to the Goren et al. (2025) study in the following sentence:

“When trying to constrain cloud adjustments from observations, separating the confounding influence of meteorological variability from the cloud responses to aerosols represents a major challenge (Goren et al., 2025; Gryspeerdt et al., 2016; Stevens and Feingold, 2009).”

2. Line 45: By constraining cloud morphology using MODIS observations, Goren et al. (2026) reported contrasting results, finding that LWP adjustments nearly offset the Twomey forcing. Goren et al (2026): Beyond discrete stratocumulus regimes: a ternary continuum of morphology reveals within-regime variability in cloud susceptibilities, *Atmos. Chem. Phys.*, 26, 7193–7206. <https://doi.org/10.5194/acp-26-7193-2026>

We have edited the following excerpt to account for the Goren et al. (2026) results which incorporate cloud morphology as an additional variable:

“After attempting to control for meteorological variability, several studies find that radiative forcing changes associated with cloud adjustments are relatively weak compared to the Twomey forcing (Tippett et al., 2024; Diamond et al., 2020; Christensen et al., 2026). In contrast, Goren et al. (2026) found that after accounting for cloud morphology, Twomey forcing and cloud adjustments can offset one another, highlighting the potential importance of targeting MCB to focus on the most susceptible cloud regimes.”

3. Line 75–76: Are there any references demonstrating that these 17 LES simulations are in close agreement with the observations. Or the term “observations” is used loosely here to represent general shallow marine clouds. Please clarify.

The term “observations” in this statement refers mainly to the SW CRE measurements from CERES, which are discussed later on in Section 2.2. But more broadly speaking, you are correct that the term is used somewhat loosely to encapsulate the general evolution of the clouds along the northeast Pacific trajectories, which aligns well with both CSET observations (Mohrmann et al., 2019; Bretherton et al., 2019) and previous satellite analyses of diurnal stratocumulus behavior (Wood et al., 2002; Wood and Hartmann, 2006; Burleyson and Yuter, 2015).

To clarify this, we have altered the following sentence:

“The following analysis is restricted to a subset of the full LES library (54 simulations), focusing on the cloudy LES cases (17 simulations with median cloud fraction >50%) that most closely match satellite observations of reflected shortwave energy and represent likely candidate environments for MCB.”

4. Line 76: The abbreviation LPM appears for the first time here without being defined.

Thank you for catching that. We have added the abbreviation to the following sentence:

“The horizontal spreading of aerosols injected from a point source into the marine boundary layer can be reasonably modeled by Langevin particle models (LPMs) (Stohl et al., 2005; McMichael et al., 2024),...”

5. Line 118: Could the authors also describe how Nd is derived from the CERES measurements? In addition, how is Na computed from the MERRA-2 data? A brief description or an appropriate reference would be sufficient.

CERES Nd is calculated based on Eq. (7) in Painemal and Zuidema (2011) from cloud optical depth and effective radius. MERRA-2 Na is calculated from the mass mixing ratio of aerosol species, assuming appropriate particle size distributions, following the methodology described in Appendix A of Erfani et al. (2022).

Reference: Painemal, D., and P. Zuidema, 2011: Assessment of MODIS cloud effective radius and optical thickness retrievals over the Southeast Pacific with VOCALS-REx in situ measurements. *J. Geophys. Res.*, **116**, D24206, <https://doi.org/10.1029/2011JD016155>.

We have added the following sentence to point to the relevant papers describing the methodology:

“Details on how CERES Nd is calculated based on measurements of cloud optical depth and effective radius are given in Painemal and Zuidema (2011). MERRA-2 Na is calculated from the mass mixing ratios of aerosol species, following the methodology described in Appendix A of Erfani et al. (2022).”

6. Line 263: This assumption is inconsistent with Equation (2), where only 80% of aerosol particles, rather than 100%, are assumed to activate as cloud droplets.

You are correct to point out that the two values are different, but they serve slightly different purposes. The 80% activation percentage in the CERES-corrected Na estimate is informed by observations, with the primary purpose being to accurately initialize the aerosol conditions in the LES. In contrast, the 100% activation rate in the particle model is used to provide an upper bound on the potential impact of plume spreading. Adding in differences in activation efficiency complicates the causal link between spreading rates and Twomey forcing, but it is certainly a relevant question that we are investigating with ongoing work.

We mention the rationale for 100% activation on line 477:

“Assuming 100% aerosol activation simplifies the analysis and provides an upper bound on potential spreading rate-driven Twomey forcing differences.”

7. Line 272: Please define NAc

Thank you for finding this. We have added the following:

“...and accumulation mode aerosol (NAc)”

8. Figure 3: Please define what the top panel in each panel group represents in the figure caption.

We added a short description of what the top panel represents:

“The top panels show each individual LES simulation, while the bottom panels show the ensemble median (black line) and IQR (green shaded region).”

9. Line 391 and Figure 10 (also in other instances): Is the Twomey forcing calculated for these simulations “global”?

That is correct for Figures 7, 9, 10, and 12. However, Figures 13 and 14 show the domain-mean Twomey forcing to quantify the grid box-level errors in a GCM, but these figures have been moved to the Supplement in an effort to shorten the manuscript (as suggested by Reviewer 2).

10. Section 4: Could the authors also comment on the role of cloud morphology and cell size on the spreading rate and Twomey forcing relationship?

The interplay between cloud fraction/morphology, cell size, spreading rates, and Twomey forcing is certainly worth discussing, as it is related to one of the main results of the paper. Day 2 conditions trend toward more broken clouds on average, along with deeper, more decoupled boundary layers with larger mesoscale cell sizes. The decoupling/cell size (more organized, stable boundary layer generally results in smaller dissipation rates) and velocity variance increases act together to increase the relaxation timescale (T_L) by nearly a factor of 2. The increase in the relaxation timescale will increase the spreading rate, given a constant background TKE, but this increase in the spreading rate is not shown to impact the Twomey forcing when we fix the cloud fraction (susceptibility analysis in Figure 8). Using Day 2 turbulence with Day 1 cloud morphology, background aerosol, and boundary layer depth does not change the Twomey forcing even though the underlying aerosol concentration distribution changes, meaning that the spreading rate differences, while substantial, don't translate to major differences in Twomey forcing. An important wrinkle to this relationship is that we have only focused on horizontal spreading rates in the current study. The deeper, more decoupled boundary layers may have a more difficult time effectively transporting aerosol to the cloud layer, violating our well-mixed assumption, but this is something we are in the process of exploring.

We want to more clearly state the result that Day 1 and Day 2 spreading rates are quite different, but when we fix the remaining variables (cloud morphology, boundary layer depth, background aerosol), the spreading rate differences do not translate to differences in Twomey forcing, despite shifting of the underlying aerosol concentration distributions.

“Day 1 and Day 2 spreading rate regimes differ by over 2 km hr^{-1} at times, with Day 2 relaxation timescales (T_L) that are nearly a factor of 2 larger than those on Day 1. These longer relaxation timescales are associated with dissipation rate decreases related to larger mesoscale cells, more decoupled boundary layers, and modest increases in velocity variances. To isolate the effect of turbulence-driven spreading differences on Twomey forcing, we repeated the Day 1 LPM simulations using the Day 2 turbulence characteristics while maintaining the Day 1 cloud morphology, boundary-layer depth, mean wind, and background aerosol concentration conditions. Despite the substantially faster spreading associated with the Day 2 turbulence, the resulting Twomey forcing changes are smaller than 1%. Therefore, in these simulations, the differences in turbulence-driven plume spreading alone do not produce appreciable changes in

Twomey forcing, even though the underlying aerosol concentration distributions are impacted by the spreading rate differences.”

Response to Reviewer 2:

McMichael et al 2026 ACP:

This article investigates the brightening potential of an MCB like aerosol perturbation to the NEP marine boundary layer. The authors have cleverly combined LES simulations with an offline particle transport model to estimate the Twomey component of the cloud radiative effect. This is an extension of the Idealised Wood 2021 ACP paper and will be of interest to the MCB community. While the authors have explored several subtopics in this idealised study, it would have been better if they had tried to match some of their results with the existing LES studies. The data are all publicly available. This would add more credibility to their model before embarking on an exercise with GCM(s), which I believe is the next logical step. Additionally, it would be useful if the authors could trim certain portions of the results section to enhance the readability of the manuscript. As it stands, it is slightly on the longish end. My recommendation: publish after the comments below are addressed.

We would like to thank the reviewer for their time, efforts, and helpful comments. We hope that our responses satisfactorily address the reviewer's concerns.

In McMichael et al. (2024), we matched the particle model results to 4 different large-domain LES simulations (204.8 km wide in the cross-plume direction, 25.6 km in the along-plume direction), and demonstrated that the particle model can reproduce LES spreading rates. Outside of this small library of simulations, there are not many simulations using domains large enough to do meaningful particle model-LES comparisons, which was the main motivation for using the particle model to estimate plume spreading rates within our much larger library of background LES conditions.

We agree that it would be beneficial to have a much larger library of large-domain, long duration LES plumes, but each of these simulations can cost hundreds of thousands of core hours (for context, the 4 simulation library from McMichael et al. (2024) cost well over a million core hours), limiting our ability to create a large library at the current time. We believe the existing comparison in McMichael et al. (2024) provides a reasonable validation of the particle model behavior in various environments, but we acknowledge that additional validation simulations would be helpful.

We appreciate the reviewer's suggestion regarding manuscript length. We reviewed the results section and we believe there is one main area where we could shorten the manuscript without substantial loss of information (Section 3.6). We have removed Figures 13 and 14, since the results largely agree with the analysis in Figure 12. We have moved these figures to the Supplement and have edited the discussion in Section 3.6 accordingly. Additionally, the reviewer's comments on Figures 7 and 8 led to new versions of Figures 7, 8, 9, and 10, which emphasize that the markers on the original plots are ensemble means/medians and the underlying cases do exhibit variability.

Comments:

1. Sec 2.1 and 2.2, can you attribute any physical significance to PC1 and PC2?

Although the PCs are statistical constructs, the Pearson correlation coefficient (R) between each PC and each physical variable provides insight into which variables are most strongly associated with the variability captured by the PCs. The highest R with PC1 is for the change in estimated inversion strength along the trajectory (ΔEIS) and the free-tropospheric mixing ratio (FT q), showing that these two are the most important properties driving variation in PC1. For PC2, EIS and the change in horizontal surface wind speed (ΔWS) are the most important variables, highlighting their influence on the variability represented by PC2. See Fig. 3 in Erfani et al. (2025) and the accompanying discussion for more details.

2. Line 101: quantify "reasonably capture". What does it mean?

At each of the six source locations, nine trajectories corresponding to combinations of -1.5σ , 0, and $+1.5\sigma$ (where σ is the standard deviation) in PC1 and PC2 are selected using the methodology of Erfani et al. (2025). That study then mapped the resulting 54 data points to the corresponding cloud-controlling factors and cloud properties and showed that these 54 trajectories selected from the PC1–PC2 space effectively span the observed ranges of the physical properties and provide a much more representative sampling of the full 1663-trajectory dataset than random sampling.

3. Lines 146 - In LES you are not solving N-S equations. You are solving a filtered form of the N-S equations.

We have added “filtered” to the following sentence:

“As an alternative to explicitly modeling fluid motion and aerosol transport in physical space using the filtered Navier-Stokes equations solved on a numerical grid, as done in the LES,...”

4. Why is the LPM domain size substantially different from that of the LES?

The LPM domain is substantially larger than the LES domain because the two models serve different purposes and have very different computational costs. With the LPM, we can use the LES turbulence statistics and cloud morphology to examine plume evolution over spatial scales that would be prohibitively expensive to simulate directly with LES.

The larger LPM domain allows us to represent changes in plume geometry associated with the evolving mean wind field, including plume elongation and orientation. We state this on line 187, but we modified the following sentence to clarify what we meant by plume geometry changes:

“A 200 x 200 km² domain was used to minimize artificial plume self-interaction under periodic boundary conditions on diurnal timescales and to capture changes in plume geometry, such as elongation and changes in plume-axis orientation, associated with ship motion and time-varying mean winds.”

5. Lines 201 to 204: I am not able to understand what the authors are trying to say. Can you rephrase? An equilibrium in what sense?

This section does require some clarification. We are approximating equilibrium “injected aerosol” conditions, assuming a 12-hour active aerosol injection period each day. Different particle decay timescales take different amounts of time to reach peak concentrations that don’t change much between one day and the next (this is illustrated by the figure below). We call this periodic equilibrium, since the domain-mean peak concentration does not vary day-to-day with the periodic injection cycle.

We have edited the following text to hopefully provide clarity:

“To approximate equilibrium aerosol conditions representative of multi-day, repeated injections as expected in an MCB scenario, we repeat the 12-hour daytime aerosol injection until the domain-mean peak aerosol concentration changes by less than 2% between consecutive days. Under this criterion, the number of repeated diurnal cycles necessary for periodic equilibrium for $\tau_{\text{decay}} = 0.25, 0.5, 1, 2$ days is 1, 2, 4, and 8 cycles (days), respectively.”

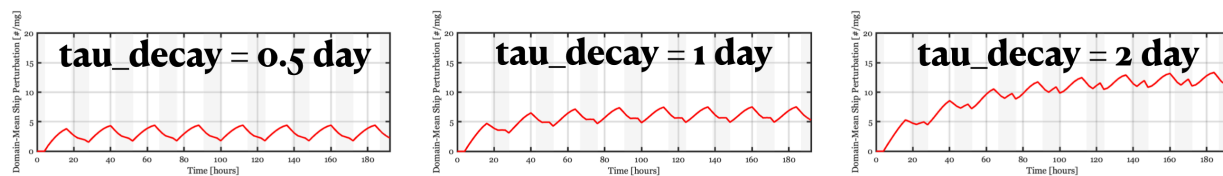


Figure: Domain-mean concentrations as a function of time for repeated daily injections as a function of the particle decay timescale (varying 0.5-2 days).

6. Line 221: Isn't Finc a constant in space? Then why the overbar?

That’s correct. We have removed the overbar and additionally, removed the dependence on x and y, since Finc is only a function of the diurnally varying zenith angle.

7. Lines 229-30: Is it SW radiative forcing or radiative effect? From what I understand it should be a radiative effect. Please clarify and check throughout the manuscript. Forcing is used repeatedly.

Radiative forcing, or Twomey forcing in this study, refers to the radiative flux change attributable to the aerosol-induced cloud albedo change relative to the unperturbed reference state, consistent

with the treatment in Wood (2021). We also present the radiative forcing changes as global averages in Figures 7, 9, 10, and 12.

8. Sec 3.1, how are the LWP and cloud fractions defined? Are there any thresholds on LWC or cloud optical depth?

LWP is calculated from the vertically integrated liquid water condensate in each model column. Cloud fraction uses a minimum optical depth threshold of 0.3.

9. Line 317: How can they all influence in nonlinear ways? Can you explain this a bit? It was stated at multiple locations throughout the manuscript.

By “nonlinear” we mean that these turbulence and cloud properties do not influence Twomey forcing independently or in simple proportion to one another. Processes such as turbulence, mean winds, boundary layer depth, and precipitation can alter the spatial distribution of injected aerosol through the spreading rate, while the Twomey response itself depends nonlinearly on local aerosol/cloud droplet concentration. The structure of the overlying cloud field also matters, as the same plume can produce different radiative responses depending on how it overlaps with the cloud morphology, LWP (this would be indirectly accounted for using the LES radiation), and background NAc.

10. Fig 7: is there a way to bring in some uncertainty estimates? Without that I am not sure how to interpret these numbers. Are all of these upper bounds?

The markers represent the ensemble average of all 17 cloudy simulations. We have added IQR shading to the plot, to emphasize that these are ensemble averages. Additionally, we have added IQR shading to Figures 9, 10 and plotted individual cases, medians, and means in Figure 8. We believe the addition of the IQR and case-level data should reduce confusion for readers, as it becomes more clear that the single point is a representative value of the LES/LPM ensemble and not an upper bound or some other ambiguous quantity. While we still want to focus on the regime-specific means (Day 1 versus Day 2) for the discussion, the library variability component is certainly worth demonstrating.

11. Lines 360-370: I am not able to follow how the susceptibility analysis was carried out. Can you please explain it better? Consequently, I am not able to appreciate the importance of Fig. 8.

The confusion may stem from an inaccurate usage of “susceptibility”, which is normally reserved for dX/dY -type perturbation studies. This particular analysis is better characterized as a sensitivity analysis, as we are swapping certain Day 2 processes into the Day 1 particle model simulation. We have replaced the “susceptibility” language with “sensitivity” when discussing this section.

We have added two sections of text to hopefully provide clarity and edited the Figure (panel a) to more clearly demonstrate the case-by-case variability.

Section of text in Section 3.3:

“It is not immediately clear which of the meteorological, aerosol, and turbulence variables are most responsible for the weak regime sensitivity. To assess their relative roles, we perform a sensitivity analysis in which Day 2 values of boundary-layer depth, cloud fraction/morphology, particle model turbulence forcing, mean winds, and background aerosol concentration are substituted individually into the corresponding Day 1 simulations for each case, while all other variables retain their Day 1 evolution. We then quantify the resulting change in ΔF relative to the corresponding Day 1 control simulation.”

Section of text in conclusions:

“To isolate the effect of turbulence-driven spreading differences on Twomey forcing, we repeated the Day 1 LPM simulations using the Day 2 turbulence characteristics while maintaining the Day 1 cloud morphology, boundary-layer depth, mean wind, and background aerosol concentration conditions. Despite the substantially faster spreading associated with the Day 2 turbulence, the resulting Twomey forcing changes are smaller than 1%. Therefore, in these simulations, the differences in turbulence-driven horizontal plume spreading alone do not produce appreciable changes in Twomey forcing, even though the underlying aerosol concentration distributions are impacted by the spreading rate differences.”

12. How important is the subgrid scale representation of plume tracks in GCMs considering the high uncertainty associated with ACI representation? Would including these subgrid scale effects reduce the uncertainty or increase the uncertainty from an MCB perspective?

Great question. It remains to be seen just how large of an impact the three-dimensional spreading may have on the aerosol size distribution evolution and subsequent activation. However, this study suggests that the horizontal spreading (assuming well-mixed, perfectly activated aerosol) would likely not be of first-order importance in many MCB environments, but getting rid of the current instantaneous spreading rate assumption and instead using a fixed spreading rate would be a relatively simple improvement that could reduce the risk of substantial overcooling.

The purpose of the subgrid-scale plume parameterization would be to reduce structural uncertainty in plume evolution, which could also improve the representation of ACI within the GCM. However, we cannot necessarily conclude that this would reduce the overall uncertainty in simulated MCB forcing, as that would require evaluation against an observational benchmark or a GCM intercomparison study.

13. How would incorporating activation fraction change your results? Have a reservoir of aerosol and activate them when the need arises. This probably is more closer to reality than the current treatment.

We agree that explicitly representing aerosol activation would be more realistic and could potentially be quite important. We are examining this in ongoing work using a three-dimensional particle model to simulate transport to the cloud layer, with this information passed to a parcel model that explicitly represents aerosol activation.

In the case of incorporating a fixed activation fraction, we would expect this to generally scale the magnitude of our current results, without altering the qualitative behavior of the results.

14. Line 453: shouldn't it be divided by TL (TKE/TL)?

TL has units of seconds [s] and TKE has units of [m^2/s^2], which means the product gives units of [m^2/s].

15. The results here seem to suggest that the timing of the aerosol injection is important. Can you explain this a bit? Is it coming from the role played by precipitation? Prabhakaran et al 2023 and 2024 seems to indicate that timing of the aerosol injection doesn't matter for non-precip. clouds. However, Zhang et al 2023 looked at an ensemble of LES studies and concluded that timing matters even in non-precip clouds. Thoughts?

The only pathway for precipitation to impact results in the current study would be through the change in the C_0 parameter in the particle model, which can change the spreading rate. Since we are not coupling the injected aerosols to the cloud microphysics, we cannot comment on the implications of early- versus late-morning precipitation suppression. The primary takeaway from our sensitivity study is that, in the absence of cloud adjustments, the pre-dawn injection allows time for the plumes to spread and reflect sunlight more effectively over the course of the day.