

Review of Transport Efficiency of Turbulent Parcel in the Marine Boundary Layer and Its Implications for Droplet Activation in Marine Cloud Brightening

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General Comments

The introduction clearly explains the motivation and scientific relevance of the study and is generally well supported by previous literature. The combination of WRF and FLEXPART to investigate the full surface-to-cloud transport chain appears novel and promising. The tracking is based on passive tracers, but the consequences and limitations of this assumption are not sufficiently discussed. I encourage the authors to better justify these methodological choices and elaborate on their potential implications for the interpretation of the results. In addition, several parts of the discussion would be strengthened by a more comprehensive comparison with existing literature. The conclusions are frequently phrased in a general manner, although the analysis is based on a single case study. The authors should consistently distinguish case specific findings from general statements about MCB transport efficiency.

Major Comments

- Section 3.3 (L360 -...): The interpretation of the resolution sensitivity remains unclear. A substantial fraction of the diagnosed reduction in transport efficiency appears to arise from the cropped analysis domain and cut-off integration period, as acknowledged by the authors. (L374-375). It would be useful to more clearly separate physical resolution effects from methodological ones. Otherwise, readers may overinterpret the apparent resolution dependency as a robust physical result.
- L395-397: I am confused by the interpretation of Figs. 10c-d. Figure 10c appears to show that the fraction of parcels embedded in a background ascending environment (f_{up}) is generally largest for the highest-resolution simulation (d04), whereas the text states that this fraction decreases with increasing resolution. The figure seems to indicate a temporal decrease in f_{up} (and corresponding increase in subsiding environments) for all simulations, but not a decrease with increasing resolution. Could the authors clarify this interpretation or revise the text?
- Section 3.4: you report AF of ~0.7-0.96 which could easily be interpreted as activation efficiencies of emitted particles. However, AF is conditioned on upward moving parcels that have already reached cloud base. The authors should more clearly distinguish this conditional activation metric from an overall aerosol activation efficiency and discuss how parcel transport and activation jointly determine the fraction of emitted particles that ultimately activate.
- L142: (1) what size of particles or undefined, (2) add reference for typical heights of MCB injection, (3) I think 50 000 is quite low number of particles being released. Typical MCB numbers are at least on the order of 10^{15} s^{-1} , this is only on the order of 10^3 (Wood et al. 2021). Are 50 000 parcels sufficient for statistical convergence of

the transport diagnostics? A suggestion is to include a sensitivity test for a larger parcel ensemble, demonstrating this.

- Section 3.1: Although the model evaluation provides useful context, this subsection feels disproportionately long given that it is not directly tied to the main research questions. The analysis effectively introduces an additional research question regarding the relative skill of WRF, ERA5, and MODIS in representing stratocumulus and LWP. I suggest streamlining this section and moving some of the supporting figures or discussion to the Appendix. Furthermore, this result is highlighted as a main finding, despite not being one of the primary questions posed by the study.
- Discussion: generally, you do not interpret or link the results to other literature on transport of particles through the lower atmosphere. It would benefit from a more explicit comparison of the reported efficiencies with previous LES studies of aerosol plume transport and MCB relevant aerosol delivery.

Minor Comments

- L46-48: authors should support claim with literature.
- L84: authors are advised to include a concise paragraph that outlines the chain of datasets used to set up your simulations and explained thereafter. The reanalysis dataset comes out of the blue. Or move L90-91 (ERA5 serves...) to beginning paragraph. Also, introduce that you use WRF in LES mode as the LES model. This would improve readability for readers that are less familiar with the used model set-up.
- L109-111: do you specifically filter for low clouds or cloud presence in general, could you comment on that?
- L117: I would argue that 100m is on the higher end of what a LES resolution should have, or no no bigger than 300m. Overall, you would expect the LWP to perform better for higher resolution, but this doesn't appear so in Figure 2. In combination with later limitations of such a small domain in the analysis, authors are advised to either look in the performance in more detail or suggest omitting.
- Section 2.4/2.5: for readability, please omit settings like `ra_sw_physics = 4` in main text, include a table with experimental overview in methods (or partially Appendix).
- (2) Release timing. How do you account for the carry-over effects of previous releases on later CDNC perturbations? Or do you have separate simulations for each release time? Please clarify and include a Table with experimental overview.
- L465: What do the authors mean with moving upward under the prescribed aerosol conditions. Could the authors explain, do you mean L477-478?
- L473: suggestion for the authors to add background to aerosol number concentration (if that is what you mean, which I suppose) or is it not? If its not, do you mean that you exclude all the information from the cloud-base particle number and residence time fully in this analysis? Please clarify and consider including this key information in your activation analysis.
- Suggestion: Rename Results to Results and Discussion

- Suggestion: Include Equation 30 of Abdul Razzak because it is the foundation of all your activation results.

Technical Corrections

- Abstract: omit d02 full or more explanatory name
- L167: some lay out issue with the 10⁻⁶ kg/kg
- L181: formatting units LWP unequal
- L184: formatting of coordinates not in bold
- Fig 2: clarify contrast between day 23 or day 24 and time this is unclear
- L239: specific humidity to water vapor specific humidity
- Formatting issues in caption figure 4, 5
- L247: insert space between 11 and g
- Fig 7: y-axis of (a) needs to include instantaneous
- Fig 8 title: clarify contrast between day 23 or day 24 and time this is unclear
- Caption Table 1 not on same page

Reference

Wood, R.: Assessing the potential efficacy of marine cloud brightening for cooling Earth using a simple heuristic model, *Atmos. Chem. Phys.*, 21, 14507–14533, <https://doi.org/10.5194/acp-2114507-2021>, 2021.