Review of manuscript titled: “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:

In this study, nested WRF simulations of a few stratocumulus days over the southeast Atlantic are coupled with FLEXPART-WRF to assess the efficiency of aerosol delivery in the context of MCB. Overall, the authors find that, for the limited environments examined, the aerosols are rapidly transported to the cloud layer and activation fractions exceed 70%. I find the overarching framework to be attractive, particularly the combination of WRF, FLEXPART-WRF, and simple representations of activation. However, I have several major questions surrounding each stage of the analysis.

1.) Nested LES: It is unclear that the LES domain (WRF d04) is performing better in comparison to the lower-resolution nested domains. In fact, it looks like the LES is performing more poorly than the coarse grid for LWP (Fig. 2). If running WRF at LES grid spacing actually degrades model performance, does it provide any additional benefit to the study? I think there needs to be some effort to improve the performance of the LES with respect to the observations, which may require boundary layer profile sharpening and/or initial condition corrections (Erfani et al., 2025) and comparisons to variables other than strictly LWP (compare to SW cloud radiative effect from CERES, for example).

2.) FLEXPART-WRF: I’m confused by the connection between the nested LES and the particle modeling. My initial expectation was that the LES turbulence properties (variances and dissipations rates) would be used to directly drive the particle model, as was done for horizontal transport in McMichael et al. (2024, 2026). If I’m interpreting the current setup correctly, the author’s use TURB_OPTION = 1 in FLEXPART-WRF, which uses a Hanna-type boundary layer turbulence scheme to estimate the turbulence properties. This seems like an unnecessary step when you have direct access to the relevant turbulent parameters.

More importantly, we do not necessarily expect a subgrid TKE or boundary layer closure to improve monotonically as grid spacing decreases. As the grid spacing is reduced from 2.7 km to 100 m, the partitioning between resolved and subgrid turbulence changes, and the assumptions underlying eddy diffusivity or Hanna-type boundary layer closures may become inapplicable to LES scales. Can the authors clarify what turbulence/boundary layer quantities from WRF are actually used by FLEXPART-WRF in this configuration, and why TURB_OPTION = 1 is appropriate for the 100 m domain?

I also encourage the authors to consider additional calibration tests. For example, why not inject passive tracers directly into the WRF domain at several release locations and compare their dispersion with FLEXPART-WRF? Ideally, a sea salt aerosol plume could be simulated directly in WRF-CHEM. Demonstrating that the WRF-FLEXPART method reproduces the plume evolution from a WRF-CHEM or passive tracer simulation would provide a compelling validation step and would make the FLEXPART-WRF framework more useful for future MCB transport studies.

3.) Activation parameterization: The authors outlined a 2-D parameter space of number concentration and mean diameters, but FLEXPART-WRF should be able to explicitly provide 3-D concentrations from particle positions. Is there a reason the concentrations from FLEXPART are not used in the calculation of the activation? Since activation is inherently local and depends on the aerosol concentration reaching the cloud layer, this seems like a central quantity that should be linked directly to the particle simulation.

Overall, I think the methodology has promise, especially using 2.7 km WRF simulations to drive FLEXPART-WRF, where Hanna-type closures are more appropriate. However, without some calibration or justification of FLEXPART-WRF at LES scales using TURB-OPTION = 1, I think the study should focus on the grid spacings that FLEXPART-WRF was originally designed for. Even at 2.7 km grid spacing, it would be nice to see a calibration study using injected MCB plumes. If it can be demonstrated that FLEXPART-WRF does a good job of recreating injected plumes in WRF (likely using both precipitating and non-precipitating cases, as the background environment can have a strong influence on the plume spreading dynamics (Prabhakaran et al., 2024; McMichael et al., 2024)), it opens the door to a WRF-ensemble study where this same type of analysis is done for a wide range of environmental conditions.

Minor comments:

Figure 2: I would redoing this plot with lines that are easier to differentiate. Perhaps, only showing d01, d04 could reduce clutter?

Lines 222-224: I think it's important to mention that d04 (the LES domain) does not improve LWP agreement. Also, is this cloudy LWP in both WRF and MODIS, or scene-averaged LWP? You could get agreement in scene-averaged LWP while getting mesoscale morphology/cloud fraction completely wrong. This is why it's important to compare other observed variables such as cloud fraction, precipitation, boundary layer height, and radiative fluxes.

References:

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