Response to Reviewer 1:

Comments to the manuscript with title "Exploring ship track spreading rates with a physics-informed Langevin particle parameterization" by McMichael et. al.

General comments

This manuscript investigates the aerosol spread rate from a point source using a "Lagrangian particle model governed by a Langevin stochastic differential equation to create a simplified framework for predicting the rate of spreading from a ship-injected aerosol plume in sheared, precipitating, and non-precipitating boundary layers". The authors showed that "the stochastic particle-velocity representation can reasonably reproduce spreading rates in sheared, precipitating, and non-precipitating cases using domain-averaged turbulent statistics from the LES". Using statistical physics to study aerosol-airflow interactions and the consequential aerosol-cloud interactions is very novel. The manuscript is also well-written. I recommend the publication of this manuscript with the following comments for the authors to consider.

My main conceptual comment is the scale problem. It is surprising to see that using domain-averaged turbulent statistics from the LES as input, the stochastic model can somehow reproduce the LES spreading rate. This is because the aerosols as tracers interact with turbulence below the Kolmogorov scales. How can the domain-averaged turbulent statistics that filter out the native small turbulence scales transport aerosols?

Thank you for your time and comments. We are studying the spreading of a local aerosol source. Throughout its lifetime, that plume is mixed into the surrounding air of low aerosol concentration by small-scale eddies. However, the plume is observed to spread by approximately three boundary layer depths per hour. This spreading occurs, in part, through larger-scale eddies stretching/shearing the plume and transporting the high-aerosol parcels into the surrounding low-aerosol region. While mixing by small-scale eddies will, in reality, be required to homogenize the aerosol concentration locally, we chose to include only the transport by large-scale eddies in our stochastic model for simplicity. If the larger features are responsible for most the transport/spreading, it then seems logically feasible that domain-averaged quantities may be able to capture the general evolution of plume spreading, and perhaps, one of the more important components to capture is the daytime increase in the relaxation timescale likely associated with decoupling. We have slightly altered several references to "mixing" in the manuscript, emphasizing that larger eddies will transport/spread aerosol rather than mix/homogenize the aerosols (paragraph starting on line 439):

"It is then immediately obvious that the spreading of the aerosol is not solely related to mixing done by the smallest scales. As an alternative calculation, D can be estimated as the product of a characteristic zonal eddy velocity (uc) and a characteristic length scale (l) on which the transport occurs (D = ucl). In the CONTROL run, the characteristic zonal eddy velocity is ≈ 0.3 m s−1 and the characteristic length scale may potentially be driven by mesoscale eddies which are initially

≈ 8 km wide. The resulting constant Gaussian diffusion equation estimates a width of 26.3 km at hour 10, which is much closer to the LES plume width (Figure 10). While assuming the larger scales are doing a majority of the transport related to the ship track spreading does improve the simple Gaussian diffusion model performance,…"

We agree that the performance of domain-averaged turbulent statistics is surprising, but also encouraging given that subgrid plume properties are unavailable in most cases. It's also important to note that while domain-averaged turbulent statistics can reproduce spreading rates with reasonable accuracy, the in-plume statistics more realistically capture the geometry of spreading in the first 15 hours, with more linear spreading and a pronounced inflection point during the evening. It is only during the overnight period in which the in-plume statistics suffer, particularly in the STRONG case. Also, the in-plume statistics are directly sampling the region in which a local mesoscale circulation exists (for the precipitating cases), likely resulting in the better initial performance, but once multiple cells develop within the plume region the daytime relationship between the optimized constant (C_m) and the dissipation seems to break down entirely (in STRONG case). This is briefly discussed in the conclusions on lines 537-539. We have added additional explanation and rearranged the text to expound on the reasoning behind the in-plume statistics failure after sunset on line 535:

"In-plume turbulent statistics perform better than domain-averaged quantities during the first 15 hours after injection, but as nocturnal turbulence disrupts the mesoscale circulation the daytime relationship between the plume-optimized turbulence constant (C_m) and the dissipation rate breaks down and results in larger errors thereafter. As the sun sets, the domain-averaged statistics continue to represent spreading rates well during the night and into day 2, potentially as a result of the domain-averaged C_m being less sensitive to the termination of the mesoscale circulation."

Specific comments

- The numerical diffusion term is not included in Eq.1. How to deal with the numerical stability without the numerical diffusion term for the continuity equation, which is a well-known issue in many applications?

Our goal with Equation 1 was to start from first principles. If we were to discretize the conservation equation a numerical diffusion term would be introduced, but numerical diffusion is minimized in the LES by using a 5th-order advection scheme. In its current form, Eq. 1 represents a continuous conservation equation with no unphysical numerical diffusion. The main purpose of Section 2.1 is to provide a brief and vastly simplified review of the physics governing large-eddy simulation. We slightly modified the following sentence beginning on line 104 to clarify the purpose of Section 2.1:

"In the following sections, we will lay out the equations that govern our atmospheric plume model. We will begin with the Eulerian formulation representative of the LES framework and from there, work towards the Lagrangian formulation that corresponds to the numerical particle model we introduce in Section 2.3."

- Do we expect a −5/3 power law for the LWP spectra? If so, is it related to the turbulence energy spectra? How to explain the deviation from the −5/3 power law in Fig.1(b). In addition, the LWP spectra appear to be Δx independent if I am not mistaken. What is the reason behind this?

The near -5/3 power law for liquid water path power spectra was from satellite observations of northeast Pacific stratocumulus in Wood and Hartmann (2006) and also seen in a few other studies (Catalan and Snider, 1989; Wood and Taylor, 2001). The -5/3 slope is only expected at high frequencies (> 0.1 km^{-1}), which is in general agreement with the LWP spectra from the LES, although the -5/3 dashed line stretching the length of the x-axis in Figure 1b is confusing and has been altered to be consistent with the observations. It does appear that there is Δx independence in the spectra and the main rationale of showing the LWP power spectra was to illustrate that if one was to only examine the variance structure of LWP at different grid spacing the conclusion would be that 200 m is sufficient; However, the analysis of rain rates, boundary-layer aerosol, and boundary-layer depth tells a much different story.

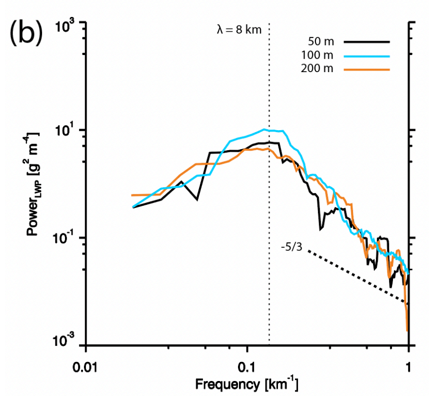

FIG: altered panel in Figure 1.

- Taking the Δx = 50m-LES as a reference, the Δx = 200m-LES-hyperdiffusion produces about two times larger values of LWP (Fig.2a) and smaller zinv (Fig.2d). However, it produces boundarylayer-averaged aerosol concentration well and Rsfc relatively well. This indicates the hyperdiffusion contributes more to the microphysical processes than to the macrophysical ones. What is the physical explanation of this observation?

This was an unexpected result and the mechanisms are not fully understood. Taken alone, the near double in LWP and in-line aerosol concentrations would be expected to produce much stronger rain rates than the 50m reference case. We mentioned in the manuscript that the inability of the 200m run to capture the rain rate seems unrelated to entrainment, but did not speculate on the potential reasons for the inability of the 200 m run to produce high enough rain rates. It's possible that at 200m the spatial organization is disrupted/under-resolved to a point where the structure of the precipitating cells is materially different. Note that the 200m run has the smallest peak in the LWP spectrum at 8km (Figure 1b). It's possible that the LWP field is more homogeneous with the thicker parts of the cloud not generating as much precipitation as in the finer grid spacing runs.

- The LWP from the weak-shear LES exhibits filament structure compared to the control and strong-shear simulations in Fig.5. Is this because of the competition between the buoyancy force and shear (Richardson number)?

It's difficult to pin down the exact cause of the filament-like structure, but it appears one of the main differences between the no shear run and the others is the much lower entrainment rate, which is likely due to less shear-driven mixing near cloud top (locally, Ri >> 1). The reduced mixing maintains lower boundary-layer aerosol concentrations and continued aerosol scavenging from ongoing precipitation which both promote larger precipitation rates. The larger precipitation rates are likely driving a faster transition to open-cellular convection as cold pools merge and the narrow cloud filaments form where cold pool mergers occur.

- It is interesting that the spatial plume evolution determines the spatial morphology of the surface precipitation rate, which should be taken into account for modeling ship tracks. Would this be one of the highlights of this study as well?

The down-shear enhancement of surface precipitation in the CONTROL and STRONG cases is notable and a signal that persists for the entire simulation. As far as we know, the down-shear precipitation enhancement has not been mentioned in previous ship track studies. We have added a few more words near line 338 to point out the interesting result in Figure 7.

"Local precipitation enhancement occurs on the down-shear side of the plume edge in the CONTROL and STRONG cases, becoming especially prominent during the second daytime period (Figure 7)."

- The PM width differs the most to the LES width for the strong shear case (Fig.12 and Fig.14b). Is this because the Langevin equation can not represent turbulence well at strong shear?

The PM seems to perform best during the most intense in-plume zonal variance (first ~10 hours after injection) and then begins to deviate from the LES strongly near sunset, as in-plume turbulence begins to wane and relax to domain-averaged values. It is after this reduction in turbulence that it seems the relaxation timescale may be artificially high in the absence of the mesoscale circulation that existed during the daytime.

- Why are the time evolution of TKE from the LES and PM so different for the control simulation in Fig.13?

In Figure 13, all time series are from the LES. The solid lines are the domain averages and the dashed lines are the in-plume averages. The confusion is understandable given that Figure 12 used solid lines for LES and dashed lines for the PM. We changed the dashed lines to dot-dashed lines to hopefully make this distinction more clear. Clarification was added to the figure caption to emphasize that the dashed lines are in-plume quantities and solid lines are domain averages.

"Figure 13. (a) The original relaxation timescale formulation (T_L) and (b) the modified version of the relaxation timescale (T_m), which focuses only on the zonal variance. Solid lines correspond to domain-averaged time series and dot-dashed lines correspond to in-plume (STcloud) averaged time series."