Reviewer 3

This study uses numerical simulations to compare Lagrangian statistics in two configurations: (i) an ensemble of simulations in different realizations of the flow, and (ii) simulations in the same flow realization, but with different release times and locations. In the first case, the flow realizations can be assumed independent. In the second, the flow remains correlated in time and/or space across the different releases. It is my understanding that the central question is how much these correlations influence the overall statistics of Lagrangian trajectories. The second configuration is particularly relevant to practical applications, since in the real ocean, drifter releases may occur at different times and locations but within the same flow realization. This is an interesting and important topic, and the study provides a thorough and valuable analysis.

I have a few comments and requests for clarification listed below, but overall, I recommend publication following revision.

We thank the reviewer for this very encouraging view on our study. We have below carefully addressed all the comments.

l.93: Please briefly clarify how this perturbation is applied.

We have now expanded the explanation of how the OCCIPUT developers applied this perturbation: "*The inter-member dispersion was generated by activating small stochastic perturbations in the density gradients resolved by the model during 1993, and deactivating these perturbations for the remaining simulation time, as presented in Bessières et al (2017) and based on the algorithm of Brankart et al. (2013)*" (lines 95-98 of the track-changed pdf).

ll.135–136: The model has an eddy-permitting resolution of ¼°, and the shortest meaningfully simulated spatial scale is most likely longer than 100 km, which implies that submesoscale motions are not represented. Please clarify why the release radius was made as small as 9 km, and what is meant by "submesoscale features" in this context. It is not surprising that particles released within 9 km and even 90 km radii exhibit only modest dispersion. I suggest the authors include a discussion of the spatial scales resolved by the model and how they relate to the choice of small release radii.

We thank the reviewer for this good comment. Indeed, our goal was to test the sensitivity in relation to the grid scale. We have now rephrased this sentence in the revised manuscript to "*The choice of spatial release radii (9 km-180 km) spans the range from sub-grid scales to ten grid cells apart, allowing us to test how initial condition uncertainties at different (grid) scales affect long-term particle dispersion*" (line 139 of the track-changes pdf).

l.160: This comment echoes my earlier concern. At this resolution, the shortest resolved spatial scale exceeds 100 km, so the meaningfulness of the estimates at smaller spatial scales is unclear.

We agree that the way we linked the numerical simulation to (sub)mesoscale ocean dynamics in the real ocean may have been confusing. We have therefore removed the reference to the Rossby radius of deformation in the revised manuscript (lines 162-163 of the track-changed pdf).

l.199: The choice of Kh and the purpose of this experiment require further justification. While such a low diffusivity may indeed be used in simulations at 25 km resolution, here it appears the diffusion is intended to mimic ensemble variability due to resolved mesoscale motions, rather than unresolved subgrid-scale processes. As such, the very limited spread of trajectories in Fig. 1d is not surprising. A much larger value, such as 1,000 m²/s or more—as used in coarse-resolution simulations—would likely be more appropriate. I would recommend a simulation with high diffusivity.

This is a very good suggestion by the reviewer. In the revised manuscript, we have included another set of simulations for K_h = 1000 m^2/s, and compare them with the K_h = 10 m^2/s simulations throughout the manuscript. Notably, the simulation with K_h = 1000 m^2/s performs remarkably well, with respect to the entropy but not at all with respect to the connectivity. We now also highlight that in the abstract and conclusions (lines 14-16 and 584-596 of the track-changed pdf).

l.235: Is "mixture distribution" the same as the probability distribution referenced in Equation (5)? This is somewhat unclear. Since the mixture simulation is intended to represent the full ensemble, while this section discusses a single-member simulation, clarification would be helpful.

The reviewer is right that the relation between these two mathematical concepts should have been clearer. In the revised manuscript, we now extended the sentence to "*We then used these probability distributions to compute the mixture distributions over all grid cells* $\mathbf{P}_{\mathrm{mix}}(X|r,t)$ *for each single-member strategy …*" (lines 244-245 of the track-changed pdf).

l.340: This sentence needs clarification. While I agree that choosing a release radius smaller than the model's spatial resolution effectively results in a point release, it is unclear what is meant by "full ensemble variability" in this context.

We have now clarified this sentence to "*Throughout this analysis, we use the mixture distribution with* δ_r*= 0.1°* *as our reference, as it represents the closest approximation to a point release, while still being controlled by the variability in ocean velocities from the full ensemble variability*" (lines 350-351 of the track-changed pdf).

l.460: It would be helpful to remind the reader how to interpret the results shown in Fig. 7, and to explain why low values of relative entropy indicate a good approximation of the reference case.

We have now added a sentence to the revised manuscript to help the reader interpret the results of Fig 7: "*In these plots, low values of the relative entropy indicate a good agreement to the reference case, as relative entropy is a measure of the mismatch between each of the distributions shown and the reference distribution (technically: the cost of assuming that each of the distributions shown is the reference distribution)*" (lines 493-495 in the track-changed pdf).

l.555: While I agree with the conclusion stated here, the purpose of this experiment remains unclear. If the goal is to mimic mesoscale-induced ensemble spread, it would make more sense to use a much larger diffusivity.

With the inclusion of the new simulations with K_h = 1000 m^2/s, we can now much better reflect on the underperformance of the K_h = 10 m^2/s simulations, so we thank the reviewer again for this suggestion.

l.574: Please remove the extra "the."

We have removed the repeated "the" from the revised manuscript.