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Discussions

Interactive comment on "The role of vertical shear on the horizontal oceanic dispersion" *by* A. S. Lanotte et al.

Anonymous Referee #1

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The paper is potentially interesting, but it needs substantial revision, in order to clarify and improve several points.

My general comments are the following:

The authors use the FSLE metrics computed from surface drifters as benchmark. More details on the drifter data set should be given. In particular, are they CODE sampling the upper 1 meter, or are they drogued at 15 m?

The authors compare the drifter results with results from MFS trajectories at the surface and they find a significant difference, since the model results show a plateau after approximately 30-40 km while drifter results keep increasing. This is actually a known result, as discussed in details in a number of papers (e.g. Haza et al., 2012), and it

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is due to the lack of resolution of the model. The authors mention this as a possible reason, but I think they should expand and introduce the references.

More interesting is the discussion on the effects of vertical shear, even though there is a conceptual problem in the way it is presented in the paper. The authors use the same FSLE metric from surface drifters as benchmark, but surface drifters (especially if CODE!!!) do not feel the effects of vertical shear on scales of approx. 40 m as considered in the model....And even if they would, it would be a vertically averaged effect, very different from the buoyant particle case of the model. As a consequence, in my opinion this comparison is seriously flawed.

What is interesting is the comparison between the model results, i.e. the MFS surface, the MFS at 2 levels and the case with KLM, that show the effects of shear for a model tracer distributed in the first 50 m. Results indicate that adding a Lagrangian Kinematic Model that parametrizes the action of 3d turbulence does not increase the effects of shear (it actually tends to reduce them in some cases...). The author rational for this, if I understand correctly, is that model vertical shear is unrealistically high because correlation time scales are too long compared with data. This point should be expanded and better explained.

Overall, the paper needs in my opinion some restructuring. My suggestion is the following: - a) The first step should be showing ADCP data to characterize vertical shear in 2 Mediterranean Sea sites b) ADCP results are compared to MFS results, indicating that correlation times are too long in the model c) The 3d KLM is introduced to parametrize the effects of turbulence and introduce short time scales d) the effects of shear are studied comparing model results: first surface MFS, then cseries I and II. e) I would remove the part on drifter comparison, because I am not sure it adds anything. It can be mentioned as previous results.

More in details: where ddid they get the KLM 3d results? - The discussion in the Introduction should be more focused and the specific scales of interest should be mentioned

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- The choice of the parameters for the 3D KLM should be discussed and motivated

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