General comments

The article submitted by V. Onink and collaborators and entitled **Empirical Lagrangian parametrization for wind-driven mixing of buoyant particles at the ocean surface**, presents numerical results on the vertical motion of plastic particles induced by wind-driven mixing in a one-dimensional Lagrangian model of the ocean surface. The authors investigate two types of stochastic approaches to mimic the upper-ocean turbulent diffusion, as well as two different profiles of diffusion in the vertical based on published studies. They compare their numerical outputs, mainly the mean concentration profiles for plastic with different rising velocities, with observations from 5 previous studies (4 published, 1 unpublished).

The material presented here is well structured and clear, with the appropriate level of English. It corresponds to an interesting implementation of a Lagrangian transport model for plastic pollution based on models reproducing the properties of turbulence in the upper-ocean, and the authors indeed emphasized that their approach is compatible with more complex OGCM (Ocean Global Circulation Model) approaches. However, the discussion of the results made by the authors is limited to simple metrics. Furthermore, more efforts could be made in the description of the model implementation (although the code is available at a Zenodo deposit).

In the end, I have the impression that the results are not sufficiently discussed, and below are my main recommendations for the manuscript to be improved, before granting publication.

1. In §2.1, the code used for the study is described with little details. The code Parcel is clearly made for 2D or even 3D studies, but it is not clear to me how it is transformed to solve one-dimensional problems, in the vertical. What is the horizontal domain like, what is the rule of transport for the 100,000 particles transported all simultaneously launched at the same depth at the beginning? Much more details are required here. There are no details on the spatial resolution as well.

2. The comparison of the model outputs with the observations is made by using a single metrics, the root mean square error between mean profiles and a "normalized " field measurements. First a clear definition of the expression used is required although it might seems obvious, to avoid any confusion. Furthermore, it seems a bit too simplistic. Since the many profiles are not all with the same uncertainty, or the same flow conditions, some higher level of analysis could be made for the observations. Similarly, the temporal "steady" profile is not the only quantities to extract and variance at least would be of interest. Furthermore, the global comparison of a profile with observations by averaging with depth is possibly putting a lot of importance on strong errors at large concentrations although the overall profile could be 'on appearance' correct.

3. The case of the fastest rising particles is disappointing. The difficulties in terms of temporal resolution should be discussed in more depth, with some comments made on time intervals for fast objects related to the vertical resolution of the models too (0.03*30 1m ... to compare with vertical resolution). Furthermore, for the numerics to be relevant, some stronger recommendations in the conclusion should be made. To my mind, the modeling of such particles is not possible for current OGCM models unless a specific choice of temporal / spatial resolution is made, but I am not sure it is the correct interpretation to have here.

Other comments

Here is a list of other points of lesser importance.

- *l.78.* What is the value of alpha for δt larger than T_L (should be 0 I guess) ?

- *l.102.* The study is based on three sets of particles having different 'rise' velocities. It would be useful to discuss the values in comparison with the turbulent properties of flow (variance of w' for instance).
- *l.140.* The introduction of θ is too succinct to be understood, more details like 'θ is a Langmuir circulation enhancement factor that one can adjust between XX and YY, we choose $\theta = 1$ which corresponds to ..."
- *p5-6.* No reference in the text to Figure 1 for KPP profiles.
- *p7.* Table 1 introduces unpublished data which is almost invisible in the corresponding figures, and it represents a small number of profiles with little representation. Maybe it is not worth including them that way.
- *l.177.* Typo 'w_{10}' instead of 'u_{10}' ?
- *l.186.* (and at other lines too) The use of greater downward mixing is unclear. Discuss it in terms of depth, or larger number of particles at some depths, etc.
- *l.195.* 'With both KPP and SWB diffusion, M-1 models show increased leads to increased downward mixing of particles with increasing'. I am not sure I get this sentence clearly.
- *l.233-235.* The comment suggest that more plastic sampling in depth is needed, which is true, but I think they should also emphasize on the estimates of a proper diffusion model too (or of the eddy viscosity) !
- *l.238.* About the consistency of models. I understand the point by at the same time, why should it be consistent with other tracers if the model is inadequate? Plastics can also be a good indicator of a better diffusion model to be implemented, because it has a different nature (buoyancy, size, passive, etc). The reverse is of similar interest (test other model for tracers).
- *l.246-247.* One reference is missing for microplastic properties (Kooi. et al,... -¿ Poulain et al. 2018.