Referee Report

Manuscript egusphere-2026-64

"The Maxey–Riley–Gatignol equations for macroplastics in the North West European Shelf region"

This paper deals with an application of the Maxey–Riley–Gatignol (MRG) equations to study the transport of macroplastics. The paper derives the MRG equations applied to macroplastics, modifying some parameters to suit the problem considered. After describing the numerical method employed, the authors assess the importance of the different terms in the MRG equation and compare the trajectories obtained with those of the slow-manifold form.

The paper does contain an interesting physical finding, namely that the difference between macroplastic trajectories and tracer trajectories is dominated by the Coriolis-related forcing rather than by the Du_f/Dt term. This is a clean result that could naturally serve as the central message of the paper.

However, regarding the results, the paper lacks any comparison with available data on the spatial distribution of macroplastics, or with other numerical simulations, which could help to understand the novelty of the theoretical model presented in the manuscript. Furthermore, the paper looks more like a technical report than a scientific paper. In particular, I am concerned with Section 4, which seems an enumeration of the contributions of the different terms of the MRG equation. I think the paper would be greatly improved if the authors were able to compare observed spatial distributions of macroplastics in the field, if available, with those predicted by the model, to strengthen the message of the paper. Therefore, the paper needs to be substantially modified before it can be accepted.

Also, I think the paper needs to be rewritten and tightened in length: it is difficult to grasp the central idea. In particular, I got a bit lost in Section 2, where the authors set the theoretical framework, since at some point it is difficult to know which are the actual equations they are dealing with.

Besides these general comments about the structure of the manuscript, specific points that need to be addressed are:

- Particles are supposed to have positive buoyancy, whereas the trajectories are only 2D. What is the role of the z component in the numerical integration?
- Both in Sections 2.2 and 2.3 the authors make a very long discussion of the drag force and the history term, which could be more condensed for the sake of clarity.
- In line 200 the authors consider an asymptotic expansion to obtain the slow-manifold equations. The validity of this expansion is discussed in Tio K-K, Liñán A, Lasheras JC, Gañán-Calvo AM, *On the dynamics of buoyant and heavy particles in a periodic Stuart vortex flow*, Journal of Fluid Mechanics 254, 671–699 (1993). In particular, for the value of the Stokes number considered in the paper ($\varepsilon = 0.32$), the expansion is not valid. Please discuss this aspect and modify the manuscript accordingly.

- In line 215 the authors discuss the increase in computational speed of the slow-manifold MRG equations versus the complete MRG equations. Computational issues of this kind are strongly linked to the Stokes number, which makes the full MRG equation a stiff problem that requires specific numerical schemes and smaller time steps, whereas the SM–MRG is a non-stiff problem that allows the use of larger time steps. Please comment on this aspect and discuss accordingly.
- Line 240: in Equation (19), when the authors define $\delta t = dt/2$, the truncation error of that derivative is then of $\mathcal{O}(dt)$, i.e. first order instead of second order as in (17) and (18). Also, for a fluid particle, the Lagrangian or total derivative needs to follow $d/dt = \partial/\partial t + \mathbf{u} \cdot \nabla$. Is this considered? Please comment on these two aspects.
- I didn't understand the forward and backward steps of the RK4 scheme; please clarify this.
- The authors use a fixed $dt = 5$ min. Did the authors perform any sensitivity test to check the solution for smaller or larger values of the time step? Initial conditions for the velocity are not specified in the manuscript; please include them.
- The authors mention in line 275 that particles get stuck. How is this produced? Is this physically relevant or just a numerical artifact? If so, could it be relevant?
- As stated before, my major concern is with Section 4, which needs to be deeply changed. As it reads now, it is a discussion of the impact of the different terms and models considered herein, but one could expect to calculate trajectory dynamics and quantities such as the Mean Square Displacement, to characterise macroplastic diffusivities or spatial heterogeneity, which could give more relevance to the paper.