Referee Report

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

Format:
Comments referee in black
Author's response in blue (with reference to line numbers in original version of manuscript)
Author's changes in the manuscript in red (with reference to line numbers in revised version of manuscript)

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 Duf /Dtterm. This is a clean result that could naturally serve as the central message of the paper.
We do agree that this is one of the interesting findings in this paper, and we clarified it in the discussion. However the other main point of the paper for us is the particle Reynolds number dependence of the particle trajectory, where particles behave (more) as tracer particles for higher particle Reynolds numbers.

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.

We certainly appreciate the importance of validation for any model that attempts to predict near-surface distributions of macroplastics. Such a model would need to account for the effects of wind and waves (our model does not), be driven by realistic source distributions (which are poorly known), and validated using observed distributions (also poorly known). It is with this in mind that our focus has been on method development, and assessment of the relative importance of various terms in the MRG equations when applied to a strong and variable current systems. We note that the first reviewer has asked us to strengthen this message, making the idealized nature of the study more clear to the reader, and we have responded as detailed in our comment to that reviewer. The fact that we used velocities from a somewhat realistic ocean model has perhaps disguised the idealized nature of our work.

One of the main aims of the paper is method/theory-development: What is the functional form of the MRG equations, and what are the most important terms in this equations, for macroplastic in NWES where relatively strong and variable currents can be found? This is why we focus on the technical details in section 2. And in the results (section 4) we investigate how this functional form affects the trajectories of MRG particles compared with tracer particles.

To be able to make any meaningful comparison with observations, we would first need to add the effect of wind and waves. More importantly however, we find that when using the MRG equations for macroscopic objects in the NWES, we need to account for the higher particle Reynolds number regime. This results in particles that are very flow following (not significantly different to surface tracer particles). We thus expect that using the MRG equations will not change the match with observations compared to an (adjusted) leeway description such as developed in the work of Medina-Rubio et al. (2026).

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.

We appreciate the challenges of wading through a paper with substantial technical information. When it comes to the MRG equations, a certain level of technical detail is necessary in order to carefully assess the importances of various terms and the validity of the assumptions behind those terms. The hope is that, in the end, the results will allow future investigators simplify the technical formulation by avoiding terms that we have shown to be unimportant. Also, the first reviewer has requested additional technical information that we have felt duty bound to supply. Our revision is an attempt to balance the need for sufficient technical detail with your desire to make the manuscript more readable.

We believe the in-depth discussion of the theoretical framework in section 2 is needed to build the framework for macroplastic and give a clear argumentation on why we use this functional form. We, however, have now shortened the discussion of the history term (explained at the first specific point below)

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.
  We have the application to buoyant macroplastics in mind in this work, which is why we used 2D flows. Important to note is that macroscopic objects with a density lower than water will have a significant rising velocity. For our research, we performed a simple back-of-the-envelope calculation to calculate rising velocities from the force balance between the drag force and buoyancy force (see fig below) and find that typical rising velocities are larger dan 0.25 m/s; which is much larger than typical vertical velocities in the ocean.

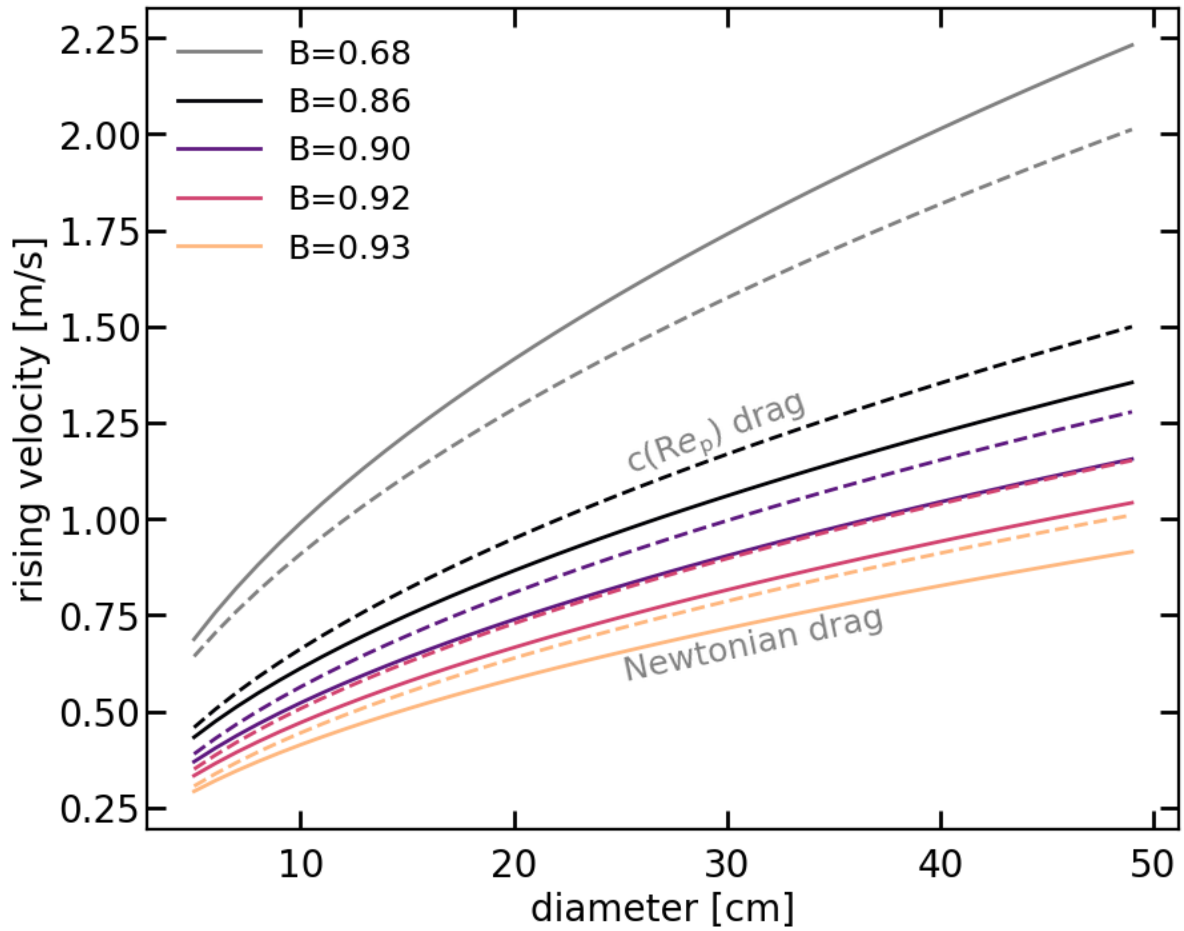

Figure 1: rising velocity for spherical particles with different buoyancies. The buoyancies used are indicated in the legend. The grey one is the value used in this work and the range shows values typical for pure polypropylene (PP) and polyethylene (PE). The solid lines show the results from Newtonian drag force and the dashed line show the rising velocities calculating using a drag force with a Reynolds number calculated from the rising velocity.

Thus, we expect that particles will quickly rise to the surface. Note also that most plastics are introduced into the ocean at or near the surface. In the case that the particles have the same density as the seawater surrounding them (e.g. B=1 in the equations), a lot of the “correction” terms in MRG equations vanish and thus we expect that the particles follow the flow.

We try to sharpen the paper, by adding more motivation on what we want to discuss in section 2 in

Line 94-95:

Before we can use the MRG equations to simulate idealized spherical macroplastics, we first discuss the drag force and history force in more detail; as their functional form changes depending on the system properties.

To tighten the work we shortened the discussion of the history term

Line 173 - 174

~~Their original work was tested for a flow over a stationary sphere, where the flow had small fluctuations in the free-stream velocity. They studied systems with~~ $\cancel{Re_p \in [0.1,100]}$ ~~and found~~ $\cancel{c_1 = 2}$ ~~and~~ $\cancel{c_2 = 0.105}$~~. Kim et al. 1998 tested the Mei/Adrian history kernel for a freely moving sphere injected into an initially stationary or oscillating fluid with~~ $\cancel{Re_p \in [2,150]}$~~. They found~~

~~c_1 = 2.5 and $c_2 = 0.126$, which are the values we use in this work. Dorgan and Loth 2007 fitted the kernel to experimental data of settling spheres. They used the constant terminal velocity to calculate the $Re_{p,term}$ which is larger than the instantaneous particle Reynolds number a particle experiences when it is released from rest. They used data with \$$e_{p,term} \in [6.5, 853]$ and found c_1 = 2.5 and $c_2 = 0.2$.~~
This equation has been validated for flow over a stationary sphere (Mei and Adrian, 1992), a freely moving sphere in a stationary or oscillating fluid (Kim et al., 1998) and settling spheres (Dorgan and Loth, 2007). They all found slightly different values of c_1 and c_2, here we use the values $c_1 = 2.5$ and $c_2 = 0.126$, based on the work of Kim et al. (1998).
And we added a table for the different timescales for a small and large particle and change the text

diameter	t_d	t_O	$t_{Mei/Adrian}$
2.5 cm	$5 \cdot 10^2$ s	40 s	60 s
25 cm	$5 \cdot 10^4$ s	0.3 s	$9 \cdot 10^2$ s

Table 1 Diffusion, Oseen and Mei/Adrian timescale associated with the history term for a particle with a diameter of 2.5 cm and a particle with a diameter of 25 cm.
Lines 183 - 185
In Tab. 1 we show the diffusion time, Oseen time and the cross-over time for two different particle sizes using the geostatic balance (Eqs. (7) and (8)) with the average surface velocity of the NWES to determine the slip velocity and particle Reynolds number. For macroscopic particles we find that there is a difference of multiple orders of magnitude between the timescales.

- 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, Li˜n´an A, Lasheras JC, Ga˜n´an-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 (ε= 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.

The work of Tio et al (1993) mentions that we need $\gamma \ll 1$(where γ is equal to ε on our case). However, they do not give a specific upper value for γ. We agree that 0.29 is not much smaller than 1 and we also find this in our numerical checks in section 4.3. As we find that for fixed Rep= 0 or 10 the SM-MRG and MRG trajectories deviate (this would correspond to $\tilde{\varepsilon} = \varepsilon / c(Re_p)$ >0.16) while for $Re_p \geq 100$ corresponding to $\tilde{\varepsilon} \leq 5 \cdot 10^{-2}$ the SM-MRG and MRG trajectories do not deviate significantly. For the flexible $c_p(t)$, the expansion is more complicated, and we did not develop a derivation in an adjusted $\tilde{\varepsilon}$ in this case. The fact that we write down the expansion in terms of ε here might make it seem that it is not valid at first glance, especially comparing to the work of Tio et al, (1993) and to the work of Rypina et al. (2024), which find that the expansion is not valid for $\varepsilon = 0.33$. The value to estimate whether the expansion would work might be better captured by $\frac{\varepsilon}{\langle c_p(t) \rangle} \approx 2.6 \cdot 10^{-2}$, with $\langle c_p(t) \rangle$ the mean value as reported in Fig.1(a). Another problem with the analysis based on the value of ε is that the choice of typical velocities, length-scales, and timescales for the NWES - and thus the value of ε - will always be arbitrary to some level, as no single value can represent the entire system. This is why we decide to test the performance of the SM-MRG equation with the skill score in section 4.3.

We added the stiffness explicitly in the discussion about the SM-MRG vs MRG equation
Lines 236 – 240
In addition, the algorithm with the SM-MRG equation is more stable, allowing to use bigger integration timesteps. ~~This is because the drag force in the MRG equations scales with c(Rep)/τs. This term becomes big for large Rep, and thus we need a small timestep to have c(Rep)dt/τs ≲ 1, which is needed to keep the numerical integration stable.~~ **This is because the MRG equations are computationally stiff due processes that take place on different timescales. This stiffness is caused by the drag force in the MRG equations, which scales with c(Rep)/τs. The drag term forces the particle velocity to rapidly adjust towards the surrounding fluid velocity, solving this rapid adjustment requires small numerical timesteps, even when the other terms evolve much more slowly (Haller and Sapsis, 2008).**

- Line 240: in Equation (19), when the authors define δt= dt/2, the truncation error of that derivative is then of 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= ∂/∂t+ u ·∇. Is this considered? Please comment on these two aspects.
  The truncation error for the forward and backward finite differences is indeed of order $O(\delta t)$), however the temporal resolution of the field (1 hour) is always much larger than δt (max 5 min). Thus, the temporal resolution of the field is in the biggest limitation in estimating the derivative. In Lagrangian analysis using $u_p = u_f$ makes the tracer (fluid parcel) follow d/dt= ∂/∂t+ u ·∇.
- I didn't understand the forward and backward steps of the RK4 scheme; please clarify this.
  This is an approximation made in the way the temporal derivatives are calculated and a limitation of parcels v3. The work only loads two fieldsets into memory at time t_i and $t_{i+1} = t_i + \Delta t$ with Δt the time resolution of the fieldset. Thus, we can only interpolate between this time instances. So, if the current time t is closer than dt (timestep used for finite differences calculation) to either t_i or t_{i+1} the code tries to sample outside the loaded fieldsets and will not work. Instead by making sure that the first step in the RK4 uses not central finite difference but forward difference it will never sample before t_i, and same for the 4th step, if it never samples beyond t_{i+1} by using backward difference. As raised in the point above this leads to an error for these 2 steps that is of order $O(\delta t)$ instead of $O(\delta t^2)$.
  We do clarify this by adjusting the wording in the method description.
  SI Lines 29 - 30
  To make sure we never overstep the time instances loaded into memory, we use a timestep of $\delta t = \frac{1}{2} dt$, with dt the numerical integration timestep of the simulation. **In addition**, we use a forward ~~derivative~~**finite difference** when calculating **the time derivative in** the first step of the RK4 scheme and a backward ~~derivative~~ **finite difference** when calculating **the time derivative in** the fourth step of the RK4 scheme.

- 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?
  The 5 minute timestep was used for the tracer advection and advection with the SM-MRG equations of 1 month. For the short simulations of 2 days a timestep of 10 s was used (needed to keep the advection with the full MRG equations stable). To make sure that there was no effect of the difference in timesteps used, we used this timestep for all particle types (tracer, SM-MRG and MRG. We performed some sensitivity tests for the 5 min timestep for the SM-MRG equation (with Rep =0 as this is the value for which the additional terms maximized) where we find that when comparing to a timestep of 30s, the relative distance does not decrease anymore below a

timestep of 5 min

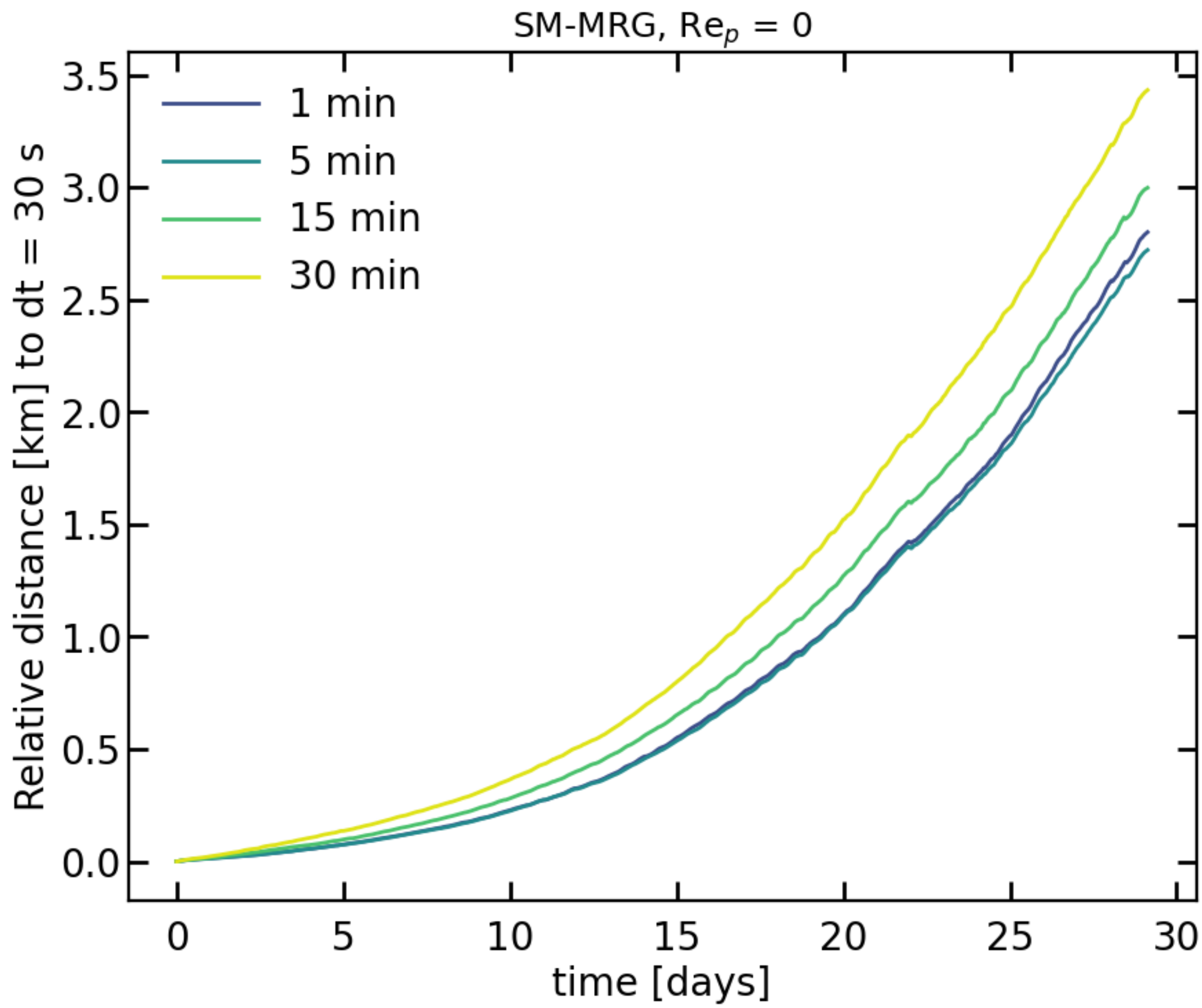

Figure 2 ensemble averaged relative distance to SM-MRG particles with Rep =0 between particles with integration timestep dt as indicated in the legend and particles with integration timestep dt = 30s for September 2023

- Initial conditions for the velocity are not specified in the manuscript; please include them.
  Thank you for pointing this out, we added the information.
  Lines 276 - 277
  For the MRG and SM-MRG particles, the fluid velocity at the position of the particle was used as initial particle velocity.
- 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?
  This is a numerical artifact because the Copernicus marine velocity dataset is re-interpolated on an A-grid. The original simulation is a c-grid with closed boundary conditions, so in the original simulation there is no flow onto land by construction. The re-interpolation by the Copernicus Marine Service breaks this and can create flows onto land. As this is not something physical modeled in the flow field but a result of the re-interpolation, we try to “ignore” this effect. We originally did this by displacing particles that (almost) move onto land further back into the ocean. However, as discussed in the comments on referee report 1, this leads to artificial jumps in the measured Reynolds number. Therefore, we now delete particles if they are pushed onto land.

We updated the description in the paper:
Line 271 - 272
To exclude the effect of stuck particles at the coast, we deleted particles from the simulation when they come closer than 0.1 grid cell from land.

- 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 characterize macroplastic diffusivities or spatial heterogeneity, which could give more relevance to the paper.

  The main aim of our work is to investigate how, when, and why the MRG equations could impact the trajectory of a (idealized rigid spherical buoyant fully-submerged) macroplastic. In section 4, we show that the model is sensitive to including non-stokes drag and Coriolis forcing, that the choices of the functional form of the MRG equations will impact simulation outcomes, and that for a large class of idealized macroplastic objects that we consider, the MRG effects are small, and the particles could be successfully modeled as being passive and flow-following.

  For simulations of realistic actual macroplastic distributions, the effects of wind and waves have to be included, and the non-spherical and non-rigid nature of microplastic floating objects needs to be accounted for. Also, realistic simulations will depend on the sources / initial distributions which are currently poorly known. Thus, such realistic modeling is beyond the scope of this work..

Additionally, we added 2 references we did not see when writing the original manuscript but think are relevant to this work:

Line 103 - 105:

Here we approximate the drag force in the transient regime by introducing a drag correction factor to the Stokes drag, similar to the work of DiBenedetto et al. (2022) and Davidson et al.(2023).

References

Medina-Rubio, J., Nussbaum, M., van den Bremer, T. S., and van Sebille, E.: Using surface drifters to characterise near-surface ocean dynamics in the southern North Sea: a data-driven approach, Ocean Sci., 22, 49–74, https://doi.org/10.5194/os-22-49-2026, 2026.