

The manuscript develops and applies a Maxey–Riley–Gatignol (MRG) formulation tailored to fully submerged buoyant macroplastics in the North-West European Shelf (NWES), implemented in Parcels and driven by Copernicus 1/36° hourly surface currents. The authors (i) derive a 2D slow-manifold form including Coriolis and non-Stokes drag, (ii) diagnose realistic particle Reynolds numbers, history-force magnitudes, and slow-manifold validity, and (iii) compare macroplastic trajectories against passive tracers for different drag regimes and temporal resolutions of the forcing. The topic is timely and relevant for both physical oceanography and marine litter modelling; the paper fits very well within Ocean Science.

Overall, I find the work potentially publishable after major revisions, mainly to (a) clarify and justify key modelling assumptions, (b) strengthen the connection to observations and practical macroplastic modelling, and (c) improve the structure and readability of the theory sections.

Major comments

Scope and physical realism of the configuration

1. Restriction to fully submerged particles and 2D surface flow

The study considers “fully submerged buoyant macroplastics” forced only by 2D surface velocities, explicitly ignoring air–sea coupling and vertical structure. This is physically restrictive because:

- Most macroplastics that are buoyant enough to remain near the surface will partially protrude into the air, so wind drag and wave-induced motions are non-negligible.
- Using a single surface level (0.5 m) implicitly assumes vanishing vertical shear and no Stokes drift vertical structure, which is not true in the NWES, especially under strong winds and waves.

The authors acknowledge windage and refer to BOM-type extensions, but in the main text the reader can easily lose track of what real macroplastic scenario is actually represented (e.g. fully submerged fishing gear vs bottles vs nets).

Recommendation:

- Sharpen the “physical scenario” paragraph in the Introduction: clearly state that the results apply to a very specific idealized class of fully submerged, nearly-surface macroplastics (and not to typical floating bottles or nets).
- Add a short subsection in Discussion quantifying under what density/size combinations a macroplastic is realistically “fully submerged” while remaining in the surface model layer, and what that implies for the applicability of your results to field situations.

2. 2D slow-manifold vs full 3D Maxey–Riley in shelf seas

The analysis and simulations are 2D, using surface velocities and 2D MRG/SM-MRG equations. However, tidal and wind-driven flows in the NWES have substantial vertical shear, and the Stokes number and effective Rep can differ with depth.

Recommendation:

- In the Theory or Methods, explicitly state that you approximate macroplastic motion as constrained to the surface layer, and briefly discuss how vertical shear and vertical

excursions would affect the Stokes number and the balance between drag, Coriolis, and history forces.

- At minimum, add a short discussion of how your conclusions may change if 3D currents were used (e.g. stronger role of vertical gradients in Du_f/Dt).

Treatment and interpretation of drag and Reynolds number

3. Use and meaning of $c(\text{Rep})$ and $cp(t)$

The paper nicely shows that Rep in the NWES is typically $O(10^2-10^3)$, justifying non-Stokes drag and motivating the use of the White (1991) drag correction. The numerical experiments with fixed $c(\text{Re}_p^{\text{In}})$ and with time-varying $cp(t)$ are informative, but some aspects are unclear:

- The ensemble-average argument that $c(\text{Re}_p^{\text{In}}) \langle \text{Re}_p^{\text{M}} \rangle$ is constant is elegant; however, it is effectively diagnostic, calibrated on the same flow used for the simulations. It would be useful to clarify that this is not a universal relationship but a practical property of the particular NWES flow statistics.
- The physical meaning of $cp(t)$ for a deformable, non-spherical macroplastic is questionable. The drag law and Rep used are for rigid spheres.

Recommendation:

- Emphasize that all drag parameterizations assume spherical rigid particles and briefly discuss how non-sphericity and flexibility could alter both the effective drag law and the interpretation of Rep , perhaps referring to Sunberg et al. (2024) more explicitly.
- Where you argue that $cp(t)$ “better captures reality” because drag should depend on the instantaneous boundary layer, clearly separate what is physically desired from what is actually supported by available drag data for macro-scale, irregular plastics.

4. Sensitivity to $cp(t)$ and temporal resolution of forcing

One of the most interesting results is that, for flexible $cp(t)$, trajectories differ more from tracers when using daily-averaged currents than when using hourly currents, contrary to naive expectations. The explanation—that coarse temporal averaging reduces velocity extremes, hence underestimates cp and drag—is plausible, but currently somewhat qualitative.

Recommendation:

- Strengthen this point by adding a small diagnostic figure or table showing distributions of $|u_{\text{slip}}|$, Rep , and $cp(t)$ for hourly vs daily forcing, to demonstrate quantitatively that Rep is systematically lower in the daily case.
- Clarify whether this effect is robust across seasons and subregions (shelf vs slope). If not, explain where it is strongest.
- Explicitly state the implication for practitioners: using MRG-type models with $cp(t)$ on temporally smoothed currents is not conservative and can bias particles toward less flow-following behaviour than in reality.

History force: methodology and robustness of the “negligible” conclusion

5. Two-bracket estimate (Mei/Adrian vs Basset–Oseen) and parameter range

The manuscript uses two estimates for the history force: Mei/Adrian kernel (likely an

overestimate) and Basset kernel truncated at the Oseen time (likely an underestimate). Both suggest that history accelerations are at least an order of magnitude smaller than drag and Coriolis at $Re_p \geq O(10^2)$, leading to the conclusion that history can be neglected.

This is an important result, but some aspects should be tightened:

- The Mei/Adrian kernel is only validated for microscopic particles at Re_p up to $\sim O(10^2)$, in different flow configurations, and for $B > 1$; here you apply it to 25 cm buoyant particles with $B < 1$.
- The approximation for the Oseen-truncated Basset kernel assumes slowly varying slip over t_0 , which may break down near strong tidal accelerations.

Recommendation:

- Make the limitations of both approximations more explicit; clearly label the “history $<$ drag, Coriolis by $\geq 1-2$ orders of magnitude” as a robust order-of-magnitude inference, rather than a precise quantitative result.
- Consider showing a time series of the different acceleration contributions for a few representative particles (shelf, slope, weak-flow interior) to illustrate that history remains small across dynamical regimes.
- It would be useful to briefly comment whether there are plausible parameter ranges in the NWES (e.g. smaller particles, different B) where history might become non-negligible.

Slow-manifold validity and link to existing inertial-particle theory

Slow-manifold error and its dependence on ε and $c(Re_p)$

The slow-manifold reduction is justified for $\varepsilon/c(Re_p) \ll 1$, and the paper presents skill scores > 0.97 for MRG vs SM-MRG trajectories for $c(Re_p) \geq c(100)$ and $cp(t)$. This is a strong and useful result, but some connections to the theory could be clearer:

Recommendation:

- When discussing Fig. 3, explicitly relate the observed skill scores to the magnitude of ε and $\varepsilon/c(Re_p)$ in your NWES configuration, and connect this to Haller & Sapsis (2008) and Rypina et al. (2024) more concretely (e.g. “for $\varepsilon \approx 0.3$ we still find RMS separation of only X km after 2 days”).
- It would help to mention whether the remaining $\sim 1-3\%$ mismatch between MRG and SM-MRG trajectories is dominated by particular regions (e.g. strong tidal fronts) or times (spring-neap transitions).

Connection to observations and macroplastic applications

7. Lack of observational or empirical constraints

The paper is purely modelling/theoretical, which is acceptable, but the Introduction and Discussion emphasise macroplastics and cleanup strategies. Currently there is no attempt to link the diagnosed macroplastic behaviour (e.g. Re_p , slip velocities, dominance of Coriolis forcing) to observed macroplastic distributions, drifter data, or beaching statistics in the NWES.

Recommendation:

- Add at least a qualitative discussion comparing your simulated drift characteristics (e.g. typical slip speeds, length scales, timescales) to those of MetOcean Stokes drifters or other macro-scale drifters in the NWES, which you already use for parameter choices.
- If possible, mention what kind of observational datasets (e.g. drifters, camera-based macroplastic tracking, beach litter time series) could be used in future work to test the predicted near-tracer behaviour and the relative importance of Coriolis vs gradient forcing.

Specific / technical comments

Below are more detailed comments that may be useful when revising.

1. Abstract

- The abstract is clear but quite dense for a non-specialist; consider adding one sentence explicitly summarising the main methodological advance (“we derive and test a slow-manifold MRG formulation with non-Stokes drag for macroplastics in a realistic shelf-sea flow”).
- It may be worth specifying in the abstract that the particles are fully submerged and that wind effects are not accounted for.

2. Section 2.1, Eq. (1)

- You state that Faxén corrections are ignored because gradients are on scales much larger than the macroplastics. It would help to quantify this: e.g., typical horizontal shear scale $L \gg d = 0.25$ m, so Faxén terms are smaller than X .
- Clarify whether you entirely ignore vertical components of Coriolis and flow, or only project onto the horizontal plane.

3. Section 2.2 (Drag force)

- White (1991) drag law is used up to $Re_p < 10^5$. Your Re_p values reach $\sim O(10^3)$; that’s fine, but it may be helpful to mention any known deviations for rough or deformable objects in this range.
- When you estimate $Re_p \approx 867$ from the geostrophic balance example, you rely on $U = 1$ m s^{-1} ; please justify that this is representative of strong tidal currents in your NWES product.

4. Section 2.3 (History force)

- When introducing t_d , t_O , and $t_{Mei/Adrian}$, the discussion is quite long; consider adding a small schematic/table summarizing the timescales for $d = 0.25$ m and $d = 0.025$ m to make the scaling argument more transparent.
- Clarify whether you include the $f \times (u_f - u_p)$ term inside the history integral in the numerical evaluation or whether it is approximated separately; this could matter for fast inertial oscillations.

5. Section 3.1–3.2 (Numerical integration)

- i. The modified RK4 scheme for second-order ODEs is correct but slightly heavy to read. Consider moving the full algorithm derivation to an Appendix and keeping only a short description in the main text.
 - ii. You use $dt = 10$ s for full MRG and $dt = 5$ min for SM-MRG. It would be useful to state the Courant number relative to the $1/36^\circ$ grid to reassure the reader that advective CFL is well-respected.
6. **Boundary treatment and coastal interactions**
 - i. Particles within one grid cell of the coast are displaced normal to the coast by $1 \text{ m s}^{-1} \cdot dt$. This is a strong and somewhat ad hoc rule that can impact trajectories near shore where macroplastics accumulate and beach.
 - ii. Please discuss how sensitive your results are to this displacement magnitude and whether it can artificially affect Rep and $cp(t)$ estimates near the coast.
7. **Metrics for trajectory comparison**
 - i. The choice of trajectory length difference, relative distance, and skill score is appropriate. It might help to give a short physical interpretation of typical values (e.g. $\langle D \rangle = 30$ km after 30 days corresponds to ~ 1 km/day separation).
 - ii. In the skill score definition, you only show the case α vs β symmetrically; clarify which type is used as “reference” in your figures (tracer vs MRG vs SM-MRG).
8. **Figures**
 - i. Fig. 1: In panel (a), consider plotting the full range including $c(0)=1$ point and then using an inset to zoom the region of interest; currently the omission may confuse readers.
 - ii. Fig. 2: PDFs of accelerations use log axes and multiple colours; the legend is dense. Consider splitting into two panels or adding small insets focusing on the history-force distributions to highlight the relative magnitudes more clearly.
 - iii. Figs. 4–5: The example trajectories are helpful. You might add markers at 5-day intervals to indicate how phase differences accumulate over time.
9. **Discussion and conclusions**
 - i. The Discussion section is thoughtful and touches on many relevant points, but it is long and could be more sharply structured (e.g. subsections “Implications for macroplastic modelling”, “Limitations of our approach”, “Future extensions”).
 - ii. The final recommendation that a modified leeway-type formulation may ultimately be more appropriate than fully fledged MRG for macroplastics in the NWES is very important; you might want to state this more prominently and earlier in the Discussion, as it is a key “take-home message” for applied users.
10. **Code availability and reproducibility**
 - i. Code is made available via GitHub, which is excellent. Please ensure that the repository includes:

- a. A configuration file or notebook reproducing the main experiments (including $cp(t)$ vs fixed $c(\text{Rep})$ cases).
- b. Scripts for computing the history-force diagnostics and skill scores, not only the forward advection.

Recommendation

I recommend major revision. The manuscript is scientifically sound and potentially impactful, but the presentation and interpretation of key assumptions (drag law, fully submerged 2D particles, history-force approximations, $cp(t)$ sensitivity to temporal resolution) need to be clarified and more clearly framed in terms of applicability to real macroplastics in the NWES. Addressing the points above will significantly improve the paper's robustness, clarity, and usefulness both for inertial-particle theorists and for practitioners modelling marine macroplastics.