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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Modeling the dynamical sinking of biogenic particles in oceanic flow" *by* Pedro Monroy et al.

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Response referee 2:

We acknowledge Referee 2 for the careful reading and detailed comments. Referee 2 states that the manuscript "**is well-written and the topic is of clear interest both for the physical and biological oceanography communities**". However, he/she gives a series of concerns that should be addressed to improve quality and clarity. We report in the following detailed point-by-point responses, together with the specific changes made in the manuscript. Page, figure and line numbers refer to the revised version of the manuscript.

1) Abstract The sentence "Our aim is to unify the theoretical investigations with its applications in the oceanographic context and considering a mesoscale sim-

ulation of the oceanic velocity field." sound to me a bit too ambitious for what is really done in the paper. In the end the authors never really integrate the particles motion via their dynamical equations.

Also, the sentence "By using the equation of motion of small particles in a fluid flow, we assess the influence of physical processes such as the 5 Coriolis force and the inertia of the particles" is a bit exaggerated. Rather then assessing, I think that the authors estimate the influence of physical processes, on the basis of some simplifying assumptions.

- The abstract has been edited to gain clarity. The sentences pointed out by the referee are now written as *"Our aim is to evaluate the relevance of theoretical results of finite size particle dynamics in their applications in the oceanographic context. By using a simplified equation of motion of small particles in a mesoscale simulation of the oceanic velocity field, we estimate the influence of physical processes such as . . ."*

2) In the introduction the same kind of sentences is used. "We assess the influence of physical processes such as the Coriolis force and the inertia of the particles with respect to the settling velocity. We also study the spatial distribution of particles falling onto a plane of constant depth above the seabed and we observe clustering of particles that is interpreted with simple geometrical arguments which do not require physical phenomena beyond passive transport and constant terminal velocity."

I think that these sentences should be somehow smoothed, since the statistical analysis here reported on both the contributions of different forces in the particles dynamics, and the clustering are interesting, but rather simple.

- The final paragraph of the introduction has been rewritten.

3) Sect. 3.1

3a) A relevant reference about the importance of Basset history term is missing.

The effect of the Basset history force on particle clustering in homogeneous and isotropic turbulence Phys. Fluids 26 (2014)

- We acknowledge Reviewer 2' suggestion: the reference by Olivieri et al is certainly relevant here. We have included in the revised version of the manuscript just before Eq. (2).

3b) Page 6, line 10: a discussion here starts about the validity of MRG eq. and the specific ocean conditions. I find this discussion too vague. Authors should be more carefully their working hipothesis. In particular: Kolmogorov scales (spatial and temporal) are based on the kinetic energy dissipation rate, ϵ. This can change a lot in the ocean, both horizontally, and vertically. So how do the authors choose the adopted value (which should be $\epsilon = 10^{-6} m2/s3$)? Have they obtained it from ROMS stat of the Benguela region? Does it vary vertically and /or horizontally? If $\epsilon = 10^{-6} m2/s3$, then $\eta = 1mm$ and $\tau_\eta = 1s$: are these the values used by the authors to assess MRG eq. Validity?

- These values are adopted from observational measurements in the ocean, and mainly taken from Jimenez (1997). Specifically, they correspond to wind driven turbulence at the surface. Certainly all these turbulent parameters vary in the horizontal, in the vertical, and in time, but to estimate the validity of the assumptions leading to MRG and its simplifications one only needs to consider worst-case situations for which the wind-driven surface values, in the open-ocean case, are good representatives (oceanic deep currents are sluggish and turbulence intensity at depth is typically weaker, except maybe in the vicinity of topographic features not considered here).

In the new version of manuscript we have clarified where these values are taken from, and we have also indicated to which ϵ do they correspond (in paragraph after Eq. (2)):*"Because of wind, turbulence intensity is typically larger at the ocean surface, with values of turbulent energy dissipation in the range* $1 \cdot 10^{-6} m^2/s^3 < \epsilon < 3 \cdot 10^{-5} m^2/s^3$ *(Jimenez,1997), than at depth. The first condition for the validity of the MRG equation* *that Maxey and Riley discussed in their original paper (Maxey, 1983) is that the particles have to be much smaller than the typical length scale of variation of the flow. This means that for multiscale (turbulent) flows the radius of the particle a has to be much smaller than the Kolmogorov scale η, which according to the previous values of ϵ, is typically $0.3mm < \eta < 2mm$ in the ocean surface (Okubo 1971, Jimenez 1997). Note that we need only to consider worst-case situations for assessing the validity of the different approximations".*

4) Sect.3.2 4a)There are two references about inertial particles in rotation flows that the authors seem to miss. These are: Dynamics of Particles Advected by Fast Rotating Turbulent Fluid Flow: Fluctuations and Large-Scale Structures, Phys Rev Lett 14 (1998). Coherent Structures and Extreme Events in Rotating Multiphase Turbulent Flows, Phys Rev X 6 (2016).

- We now cite these references at the beginning of Sect. 3.2, where rotation is introduced.

4b) In particular in the latter, it is discussed, in addition to the Stokes drag and added mass terms, the role of centrifugal and Coriolis forces onto the dynamics and dispersion of inertial particles, by using DNS at high resolution. Authors should consider their assumptions - and conclusions- in view of these results. The Rossby number for the particle turbulent dynamics matters and clearly distinguishes cases when rotation related effects are sub-dominant from cases when rotation dominates both the flow and the inertial particles dynamics. E.g. it seems that if $Ro = (\epsilon k_f 2)1/3/\Omega$ with $k_f = 2\pi/10km$ $Ro = 1$; if $k_f = 2.\pi/100km$ $Ro = 0.2$ and rotation might matter. Although in the ocean these turbulent parameters might not be the relevant ones and considering also that for the biogenic particles the Stokes number are very small, the authors should better justify their choices.

- The turbulent parameters used in this reference are not the appropriate for sinking of biogenic particles in the ocean. However it is true in our case that Coriolis is much more important than inertia (about a factor of 100). But still both effects are negligible compared with plain advection and sinking velocity. We have included the following discussion close to the end of Sect. 4: *“It is worth noting that although the small value of Rossby number 0.01 for mesoscale processes might indicate a strong influence of the Coriolis force in Eq. (8), its influence on particle dynamics becomes negligible because it is multiplied by* tau_p *or equivalently, the Stokes number, which is significantly small for biogenic particles. Nevertheless Rossby number coincides with the ratio of inertial term to Coriolis term in Eq. (8) and its value 0.01 explains the difference of two orders of magnitude among the corrections arising from the inertial force and from Coriolis.”*

5) Sect. 4 5a)The authors consider daily averages of the ocean currents and vertical velocity calculated from ROMS. This implies that not only the dynamics at spatial scales of the order or smaller that the grid size is lost, but also the temporal variability.

The authors choice is to use a white noise to the flow velocity to account for the huge gap of scales existing between 200 microns and about 10km (and similarly for the time scales). This is clearly a crude approximation, in view of the fact that there is a consistent literature showing the role of such sub-grid motions in the Lagrangian dynamics of particles, by using stochastic or kinematic closures (see Lagrangian stochastic modeling in coastal oceanography, J. Atmos. Oceanic Technol., 19 (2002), and Lagrangian simulations and interannual variability of anchovy egg and larva dispersal in the Sicily Channel, J. Geophys. Res. Oceans, 119, 2014)

- Yes, we agree with the referee that our ROMS data contains less temporal and spatial scales than the real ocean (this is acknowledged line 1-2 p. 18, conclusion sect.). We also agree with the referee that our representation in terms of a white noise is a crude approximation. But note that we are not analyzing the different ways to resolve sub-

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grid motions, but studying the relevance of the different terms (Coriolis, inertia...) in the particle dynamics. The limit situation of sub-grid motion resolved by white noise is the one that introduces stronger temporal and spatial gradients, and then it serves well to our purposes: we expect that under more realistic sub-grid motions the impact of the corrections to simple sinking we evaluate (Coriolis, inertia, ...) would be still smaller than found here.

Nevertheless we agree with the referee on the convenience to mention more elaborated approaches to the modeling of sub-grid scales. We cite now just before Eq. (7) the two references suggested by the referee.

5b) Also, how are the eddy diffusivity values (or and vert) chosen? Are these extracted from ROMS simulations in the Benguela region? Before analysing simulations of eqs 11 or 10 compared to eq. 9, the authors should show what is the effect of the noise. So it is crucial to see first how the trajectories are modified by considering the advection by the ROMS velocity only, or the advection by the ROMS velocity and the noise term. Ans also estimate in these cases the mean traveled distance. If the noise contribution is an order (or more) of magnitude larger that inertia or rotation contributions, then we might question the choice of applying these noise terms as representatives of the dynamics at all unresolved scales.

Horizontal diffusivity is taken according to Okubo (1971) formula at the spatial scale of the model resolution. This is stated in the paragraph before Eq. (9). For the vertical diffusivity, which is orders of magnitude smaller, it is much more difficult to find systematic studies of its dependence with resolution and with depth. Estimating realistic mixing coefficients in the ocean is perhaps one of the most important, still unresolved, challenge facing physical oceanographers today. Because of that, we have taken an average value (constant with depth) from different bibliographical sources, as in Rossi et al. (2013), again with the idea of considering a rather generic situation. The difference in particle trajectories with and without noise could be large (see response to first point of referee 1). This is why we keep the same sequence of random numbers when repeating our simulations in adding new terms (as stated before Eq. (11)). By using the same noise realization we can focus our primary objective, i.e. to compare the impact of adding or removing the Coriolis or the inertial terms. We have reworded the pertinent sentences to make this clearer here (page 11, paragraph after Eq. (11)): *"We use in each case identical initial conditions and the same sequence of random numbers for the noise terms. In this way we guarantee that any difference in particle trajectories arises from the inclusion or not of the inertial and Coriolis terms in Eqs. (8)-(11)."*

5c) How is exactly estimate the inertial term in eq. 11? The appropriate choice would be to estimate it from the true ROMS simulations and not from the daily averaged snapshots, since these last never had the correct temporal variability. Indeed we could make a crude approximation to compare the inertia and Coriolis force: the former is the resolved velocity divided by the time scale between one snapshot and the other, 1/T = 1.15e-5/s, while the latter is the resolved velocity times 2*Omega=14.5e-5/s. So this already tells us that the Coriolis term will be more important. . But this is due to the way forces are calculated here.

- We first note that our ROMS configuration uses climatological forcing. Thus, intraday variability is not really strong. But in any case its impact can be assessed qualitatively. We have followed Tang et al (2012) to estimate the term Du/Dt, under the assumption that unresolved small scales/times behave as an added random contribution. Thus we have for the flow velocity $u = u_{resolved} + noise$. As indicated in Tang et al, the inertial term gets a contribution Dnoise/Dt, but it turns out to be negligible since it is multiplied by the Stokes time. Nevertheless we agree with the conclusion of the referee that Coriolis is more relevant than inertia, as explained in the response to the point 4b.

5d) A convenient and more informative choice to estimate the importance of different terms would be to plot the horizontal and vertical root-mean-square error growth between different models trajectories, as a function of time. Table

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2 is indeed not really informative.

- This is a very pertinent suggestion, also reported by referee 1. We have added a new Figure 4 showing the time-dependence of the mean root square differences between trajectories.

6) Sect. 5 I find this section highly speculative and not very informative. It is clear that a 2D cut of a 3D incompressible field will be compressible, and hence exhibit some sort of clustering. But unless the analysis becomes more specific, I think the authors should remove this section and keep the comments there contained for the conclusions. Similarly, the abstract should be rephrased.

In earlier drafts Sect. 5 was not present and we decided to include it since it was a simple way to answer the natural question that arose during our investigations: If there is not important impact of non-inertial dynamics in the sinking dynamics of biogenic particles, why clustering is observed? We show that spatial inhomogeneities can arise simply by the geometric way in which measurements are done. We do not claim this is the only explanation, but it is certainly the simplest one. Then we do not agree with the referee and we consider that our paper becomes more self-contained and useful to the community by keeping Sect. 5.

MINOR COMMENTS

i) it seems to me that there is an incorrect sign in the expression of the modified terminal velocity at page 7, line 15. it should be $v' = (1 - \beta)[g - \Omega \times \Omega \times r]$; moreover it should be emphasized that the "r" appearing in the centrifugal force expression is the distance from the rotation axis. So unless this last is zero, this is not correct either.

- Thanks for the correction, now the mistake is fixed and we include in the text (before Eq. (5)) a clear definition of the vector "r".

ii) label of figs. 4 and 5 seem to me wrong. The dimension is not m/s but (m/day),

I guess. Similarly, at page 11 line 21, v_s should be $v_s = 5m/day$ and not 5 m/s. Please check this throughout the paper.

- We have fixed these mistakes. Thanks for pointing them out.

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