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

Anonymous Referee #2

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Report on the manuscript "Modeling the dynamical sinking of biogenic particles in oceanic flow" by Pedro Monroy, Emilio Hernandez-García, Vincent Rossi, and Cristobal Lopez

SUMMARY

The paper discusses the problem of modelling the dynamics of small, biogenic particles in the ocean, with a specific attention to the sinking motion.

The authors focus on the behaviour of small organisms whose dynamics can be described by using the Maxey-Riley-Gatignol equation for the unsteady particle motion in creeping flow conditions. Particles are assumed to be point-like, with spherical symmetry. The main goal of the paper is to assess the relative importance, in comparison to the combined action of ocean passive transport and gravity, of the Coriolis and inertial

forces.

Starting from the equation of motion in a general flow (neglecting Faxen corrections and Basset history terms) : first the authors discuss how this eq. is modified in the presence of an underlying rotating fluid and get to the dynamical eq. (5), then they discuss how this last can be simplified, with different degrees of approximation, in the limit of very small inertia, St«1, and get to the kinematic eqs. 9-10-11.

The simplified motion kinematic equations are then numerically integrated for particles released in a realistic ocean flow, described by daily averaged currents and vertical velocities obtained from ROMS (Regional Ocean Modelling System) simulation of the Benguela region (Angola).

On the basis of the numerical experiments, the authors conclude that for the sinking motion of the small biogenic particles here analysed, the important ingredients are : the passive advection due to the ocean mesoscale currents and the resolved vertical motion, the action of gravity, plus the action of some eddy diffusivity to describe the effect of unresolved motions.

Finally, the authors comment on the biogenic particle clustering in a 3D incompressible ocean, by estimating the changes in the horizontal density of evolving particle layers moving with the horizontal part of the velocity field only.

GENERAL COMMENTS AND RECOMMENDATION

The manuscript is well-written and the topic is of clear interest both for the physical and biological oceanography communities. However, as it is now, the results seem to me not sufficiently analysed to draw the actual conclusions contained in the manuscript. Hence, in reason of the specific comments reported below, I can not at present recommend publication. In the following I summarise a series of concerns that, I think, the author should consider to improve the manuscript quality and clarity.

SPECIFIC COMMENTS

1) Abstract The sentence “Our aim is to unify the theoretical investigations with its applications in the oceanographic context and considering a mesoscale simulation 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.

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.

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).

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 tempo-

ral) are based on the kinetic energy dissipation rate, \epsilon. This can change a lot i the ocean, both horizontally, and vertically. So how do the authors choose the adopted value (which should be \epsilon= 1.e-6 mˆ2/sˆ3)? Have they obtained it from ROMS stat of the Benguela region? Does it vary vertically and /or horizontally? If \epsilon= 1.e-6 mˆ2/sˆ3, then \eta=1mm and \tau_{eta}=1s : are these the values used by the authors to assess MRG eq. validity?

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).

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_fˆ2)ˆ1/3/\Omega with k_f = 2.pi/10km âĂŤ> Ro= 1; if k_f = 2.pi/100km âĂŤ> 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.

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).

Probably it is justified for deep stratified waters, but this has to be discussed.

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..

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, $\sim$ 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.

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 2 is indeed not really

informative.

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.

MINOR COMMENTS

i) it seems to me that there is a 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.

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.

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