Reviewer 1

Dear Irina et al.,

0. 'No' above does not strictly mean 'No' but something in bewteen 'No' and 'Yes' and in many cases something closer to 'Yes' but the system doesn't allow me to express that. Also I have revealed my identity by posting a comment as CC by mistake; I don't mind. [It took me a while to get this review submitted through the regular(?) channel. A very confusing system; NPG used to be much easier in the past.]

Dear Javier, thank you for the overall positive feedback on our paper and for your thoughtful comments, all of which have been implemented in the revision. Our point-by-point answers are below. Best. ~Irina/Larry/Michael

1. My main concern with this paper is the lack of relevance of the flow considered for ocean dynamics. There's anecdotal reference to observed behavior. But this flow does not represent the solution, neither exact nor approximate, to any known dynamics.

We probably could have done a better job of stating the philosophy behind this work, namely that it is curiosity driven and aimed at elucidating some of the basics of attraction and attractors of microplastic-like particles in idealized, 3D situations. As far as the model is concerned, the field of dynamical systems is replete with examples where kinematic models have proved very helpful, and the message there seems to be that geometry is what matters the most. This has certainly been our experience working with the rotating cylinder model, where most of what one learns about resonant Lagrangian structures occurs in velocity fields that reproduce the same general structures as exact solutions. And it is particularly helpful in time-dependent cases, where one has to strobe both in time and in space over very long time periods, to have analytically specified velocity fields and to be able to tune parameters to reproduce different types of behavior. We have tried to motivate this better in the introduction as follows:

p. 6: "…we have elected to analyze the movement and aggregation of individual particles using a Maxey-Riley framework in connection with an idealized, analytically-prescribed, 3D vortex flow that qualitatively resembles the geometry of the circulation in some ocean eddies but is not a solution to any dynamical oceanographic equations of motion. As shown by Pratt (2014) and Rypina et al. (2015), kinematic models that reproduce the correct geometry are able to also reproduce the important Lagrangian features of the flow. Even in our simple flow, aggregation is non-trivial, often with multiple attractors present, and the lack of attraction in some circumstances. Thus, we wanted to thoroughly explore this simple example before investigating more realistic oceanic flows."

And then later on p. 7:

"In order to reach a better understanding of what leads to attraction and attractors in 3D flows, we explore a canonical example in geophysical fluid dynamics, namely the flow in a rotating cylinder. This flow resembles some of the characteristics of ocean eddies, including a horizontal swirl and an overturning component in the vertical, but is much less complex than any realistic oceanic eddy. Specifically, we use a simple analytically-prescribed phenomenological velocity introduced by Rypina et al. 2015. The Lagrangian properties of this circulation have been previously studied (Fountain, et al. 2000; Pratt et al. 2014; Rypina et al. 2015) allowing us to begin to investigate inertial particles from an established base of knowledge. A prior theory (Haller and Sapsis, 2008) governing the movement of particles with high drag indicates that accumulation is favored for slightly buoyant particles in flows dominated by vorticity, and this also motivates our choice of background flow. Identification of the attractors that can arise in this flow field, evaluating their reach and domains of attraction, and clarifying the circumstances that lead to their formation are the primary objectives of this work. Although motivated by the problem of marine microplastics, this study is mainly a curiosity-driven research aiming to develop a basic understanding of the circumstances that lead to aggregation of finite-size particles in 3D flows. The hope is that with such basic understanding in hand, one could later start investigating aggregation phenomena in more complex and more realistic ocean mesoscale and submesoscale eddying flows."

2. Furthermore, the periodic time dependence considered and the ensuing emergence of invariant tori is largely of academic interest rather than a representation of observed behavior.

Oh, please be reasonable. You can't just begin with a flow with arbitrary time dependence and hope to come away with anything. Even the time-periodic case is quite involved, as we show. You need to start simple and build from there. Of course the ocean is not time periodic, but neither is it laminar nor homogeneous nor with horizontal boundaries and a flat bottom. You could make a long list of reasons how this work differs from the real ocean. The idea here is to actually establish some baseline knowledge, and if this is what you mean by "academic", then bring on the academics!

On p. 35 we have added the following paragraph in response to your comment:

"Our rotating cylinder model is much less complex than any real ocean eddy in many respects, including the assumed quasiperiodic time dependence and the absence of decay and interaction with the surroundings. Understanding aggregation in a simple periodic flow seems like a reasonable first step towards understanding aperiodic, interacting, and decaying oceanic eddies. This approach is common in applications of dynamical systems theory to oceanography and meteorology. For example, arguments relating to the increased stability of jets due to the strong Kolmogorov-Arnold-Moser stability near shearless trajectories have first been developed for spatially-periodic and time-quasiperiodic flows and tested using idealized toy models, before exploring these ideas in more realistic oceanic and atmospheric settings (see Rypina et al., 2007 and Beron et al., 2008; 2010). Note also that our results are applicable to quasi-periodic disturbances with finite number of frequencies rather than just periodic disturbances (we only show numerical simulations for the time-periodic case for simplicity), and a quasiperiodic function might potentially be useful for approximating temporal variability in some oceanic flows, especially those with pronounced peaks in the spectrum."

3. If this cylindrical vortex flow were to be of geophysical interest, then it should first be 'embedded in the ocean,' which will require one to appropriately redefine the Coriolis and centrifugal acceleration terms; cf eg Ripa (1987).

The method we used to transform the Maxey-Riley set of equations into a rotating frame of reference is the very standard replacement of the velocity and acceleration terms as written out in item #4 below. We did not make this explicit in the original manuscript because it is quite standard, but the new version includes the transformations (right after Eq. (1)) and we also cite the Ripa paper. We had also originally checked to make sure that our results agreed with Beron-Vera et al. (2019), which they did (with the exception of the centrifugal term, as discussed below).

4. But if the authors do not wish to do 3) I believe the way the MR eq is modified to be written in a rotating frame - a seeming novelty? - can be substantively simplified. There appear to be too many Coriolis 'forces' involved. The MR reads (in the authors' notation except for the use of the Stokes time, \tau)

\dot v = 3R/2 Du/Dt + \tau^{-1} (u - v) + (1 - 3R/2) g

(cf Haller & Sapsis, 2008). In a frame rotating with angular velocity \Omega one just must replace

\dot v --> \dot v + 2\Omega\times v + \Omega\times \Omega \times x (*a)

and

Du/Dt --> Du/Dt + 2\Omega\times u + \Omega\times \Omega \times x (*b)

These simple replacements lead to (3) after rearranging.

Please see your answer to #3 above.

5. It should be \dot x = v_p(x,t) in (3); otherwise one doesn't have a closed system for (x,v_p). But there are x, x_p and r? What is, in particular, r (in the definition of g_r)?

Thank you for pointing out inconsistencies in our notation. We agree that (2) and (3) must represent a closed system for position $(\vec{x})$ and velocity $(\vec{v})$ of the particle. So introducing $r$ is unnecessary as it is simply equal to $\vec{x}$.

6. Beron-Vera et al. do not ignore the centrifugal force. They work in a GFD setting in which the gravitational and centrifugal forces balance one another on a horizontal plane (tangent to the planet). The resulting gravity (which defines the vertical direction) is in general a function of longitude and latitude. As is common in GFD this dependence is ignored, assuming spherical geometry, but the fundamental balance above is kept.

To quote Beron-Vera et al. (just before their eq. 18) “We first account for the geophysical nature of the fluid by including the Coriolis force. This amounts to replacing (13) and (14) with….”. So there is no discussion of effective gravity, geopotential surfaces, etc. at this point, and some readers are going to naturally wonder what happed to the centrifugal acceleration. In fairness, the authors do get to this question in their Appendix A, though centrifugal acceleration, effective gravity, and geopotential surfaces are not mentioned explicitly.

In any case, we have removed the suggestion that the authors did not consider the centrifugal acceleration and replaced it with the explanation on p. 9 and a statement on p. 11 (right after Eq. (4)) that the two results are the same provided the gravity term is properly interpreted. We also mention on p. 10 that Beron-Vera et al. (2019) extend the slow manifold reduction to include lift force.

7. Fountain et al. is not included in the reference list.

We included the reference in the revision.

8. Eq (8) is a modest modification of H&S' eq (31).

In the new text we now mention that the first equality in Eq. (8) is a modest extension of H&S’s Eq. (31). However the remainder of Eq. (8) is not just an extension, but rather a new observation. The new text now reads:

“The first equality in Eq. (8) is a modest modification of Eq. (31) from Haller and Sapsis (2008), and one could probably have guessed that our more general result could be obtained by replacing $Q$ with $Q_a$. The remainder of the equation expresses volume changes in terms of the fluid stresses.”

Further to the issues raised in items #3 and #4 above, we might challenge the reviewer to derive the same result using the equations of Beron-Vera et al. (2019). We think it can be done, but not trivially.

9. Fluid particles cannot deform or split; thus are rigid. A different name should be used to denote inertial or finite-size particles, for example, inertial or finite-size particles!

There are a number of people in our field who use the term “particle” to describe an infinitesimal fluid element, and therefore the term finite-size particle might still seem to refer to fluid to some. And “finite-size” does not capture the essential characteristic property that the object is rigid. So we now use the term “fluid parcel” to describe an infinitesimal fluid element and the term “rigid particle” (redundant as that may seem) to describe a plastic particle. The new version of the manuscript has been edited accordingly. We also clarify both terms at their first use on p. 5: “To avoid confusion, we will refer to infinitesimal fluid elements as “fluid parcels”, and to rigid plastic particles of finite size as “rigid particles”.

10. The derivation of the reduced eq on the slow manifold lacks a prove of the global attractivity of the latter. Fenichel does this (with the modifications introduced in H&S to the nonautonomous case).

True, but since our derivation provides an explicit form for the exponential decay of the solution towards the slow manifold, we think there is value in including our derivation as a Supplementary Material, as it might complement the conventional Fenichel's derivation. It is really a quick way of getting at the same result and something that may prove useful in applications where the existence of the slow manifold is hard to establish.

11. In the Abstract and elsewhere, 'closed contour' should be replaced by 'loop' or 'torus' as contour is more suggestive of a level set of a scalar.

We replaced "closed contour" with "loop".

12. In l.~79 dX_b suggests random variable when everything is deterministic in this ms.

Well, capital letters are used to denote lots of different quantities, many of them non-stochastic. Nevertheless we replaced $X$ with $x_p$ and $dX_b$ with $dx_b$.

13. The comment on divergence in l.~178 seems interesting; however, I couldn't find the counterexample.

We added a reference to the counterexample: "The aggregation of such a concentration would appear to require that the divergence of that velocity be negative (though see an apparent counterexample in fig 1c, presented later)."

14. Finally, all the results relating behavior near (resonance) tori of the flow may be fine, theoretically, but they lack a connection with ocean dynamics. I urge the authors' to tone down the claimed relevance of their results to oceanography in a revised version of the paper.

Agreed. We have toned down our statements regarding possible relevance to oceanographic applications. Please see our answers to 1 and 2 above.

Javier

Reviewer 2

Review of "Aggregation of Slightly Buoyant Microplastics in Three-Dimensional Vortex Flows" by Rypina et al

In this manuscript, the authors analyze the trajectories of idealized but non-trivial particles in a complex analytical flow field, to find attractors in the phase-space.

This is a sophisticated and careful analysis of an interesting dynamical system, and I enjoyed reading it a lot.

We thank the reviewer for the overall positive feedback on our paper and for the thoughtful comments and suggestions, all of which have been implemented in the revision. The point-by-point answers are below. Best regards. ~Irina/Larry/Michael

1. However, like the other reviewer I agree that the connection to actual microplastics in the ocean is not very clear. While there are some general references to some literature in the introduction section, there is no attempt to reconcile in the discussion section what the results and conclusions mean for microplastic in the real ocean. This really dimities the potential impact of the manuscript, and I thus strongly encourage the authors to reflect on the extent to which the results can be generalized to more realistic scenarios.

We agree that our idealized study presents only the first steps towards fully understanding the distribution, attraction, and aggregation of marine microplastics in real time-varying 3D oceanic flows.

Our simple rotating cylinder flow only qualitatively resembles the circulation within some of the oceanic eddied, including a horizontal swirl and an overturning component in the vertical. However, even in our simple flow, aggregation is non-trivial, often with multiple attractors present and lack of attraction in some circumstances. Thus, we wanted to thoroughly explore this simple example before investigating more realistic oceanic flows.

Our aim was to develop a basic understanding of the circumstances that lead to aggregation of finite-size particles in 3D vortex flows. The hope is that with such basic understanding in hand, one could later start investigating aggregation phenomena in more complex and more realistic ocean mesoscale and submesoscale eddying flows.

In the revision, we have added more info on this to the introduction and discussion sections:

p. 6: "…we have elected to analyze the movement and aggregation of individual particles using a Maxey-Riley framework in connection with an idealized, analytically-prescribed, 3D vortex flow that qualitatively resembles the geometry of the circulation in an ocean eddy but is not a solution to any dynamical oceanographic equations of motion. As shown by Pratt (2014) and Rypina et al. (2015), kinematic models that reproduce the correct geometry are able to also reproduce the important Lagrangian features of the flow. Even in our simple flow, aggregation is non-trivial, often with multiple attractors present and lack of attraction in some circumstances. Thus, we wanted to thoroughly explore this simple example before investigating more realistic oceanic flows."

p. 7: "In order to reach a better understanding of what leads to attraction and attractors in 3D flows, we explore a simple canonical example in geophysical fluid dynamics, namely the flow in a rotating cylinder. This flow resembles some of the characteristics of ocean eddies, including a horizontal swirl and an overturning component in the vertical, but is much less complex than any realistic oceanic eddy. Specifically, we use a simple analytically-prescribed phenomenological velocity introduced by Rypina et al. 2015. The Lagrangian properties of this circulation have been previously studied (Fountain, et al. 2000; Pratt et al. 2014; Rypina et al. 2015) allowing us to begin to investigate inertial particles from an established base of knowledge. A prior theory (Haller and Sapsis, 2008) governing the movement of particles with high drag indicates that accumulation is favored for slightly buoyant particles in flows dominated by vorticity, and this also motivates our choice of background flow. Identification of the attractors that can arise in this flow field, evaluating their reach and domains of attraction, and clarifying the circumstances that lead to their formation are the primary objectives of this work. Although motivated by the problem of marine microplastics, this study is, for now, mainly a curiosity-driven research aiming to develop a basic understanding of the mechanisms that might lead to aggregation of finite-size particles in 3D flows. The hope is that with such basic understanding in hand, one could later start investigating aggregation phenomena in more complex and more realistic ocean mesoscale and submesoscale eddying flows."

pp. 34-35: "We have illustrated the possibility of aggregation of slightly-buoyant particles in 3D vortex flows towards closed loop attractors located subsurface within the interior of the flow. Even in our idealized flow and for spherical particles with fixed radius and buoyancy, aggregation is non-trivial, often with multiple attractors present and/or the lack of attraction in some circumstances.

Our rotating cylinder model is much less complex than any real ocean eddy in many respects, including the assumed quasiperiodic time dependence and the absence of decay and interaction with the surroundings. Understanding aggregation in a simple periodic flow seems like a reasonable first step towards understanding aperiodic, interacting, and decaying oceanic eddies. This approach is common in applications of dynamical systems theory to oceanography and meteorology. For example, arguments relating to the increased stability of jets due to the strong Kolmogorov-Arnold-Moser stability near shearless trajectories have first been developed for spatially-periodic and time-quasiperiodic flows and tested using idealized toy models, before exploring these ideas in more realistic oceanic and atmospheric settings (see Rypina et al., 2007 and Beron et al., 2008; 2010). Note also that our results are applicable to quasi-periodic disturbances with finite number of frequencies rather than just periodic disturbances (we only show numerical simulations for the time-periodic case for simplicity), and a quasiperiodic function might potentially be useful for approximating temporal variability in some oceanic flows, especially those with pronounced peaks in the spectrum."

2. For example, how would the results change if

    a. the eddy is also translating (advecting) and/or decaying;

Translation with a constant velocity will not affect the results, as one might go to the translating reference frame where the eddy would be stationary. Considering decaying, as well as merging or splitting of eddies, on the other hand, would be much more complex, because these processes might change the basic geometry of the flow and are also truly aperiodic in time. These phenomena are not accounted for by our idealized model and will need to be explored separately later. This is now explained on p. 36 of the revision:

"Real ocean eddies are also decaying in time and are usually moving (translating) rather than stationary. Translation with a constant velocity can be handled by considering the flow in a moving frame of reference, but decay and interactions will likely change the geometry of the circulation and make the flow truly aperiodic. Our simplified model cannot account for these effects, which will need to be explored separately later."

b. the particles experience biofouling (see also the highly relevant work at https://aslopubs.onlinelibrary.wiley.com/doi/full/10.1002/lno.11879);

Biofouling is certainly one of the important processes that can affect the behavior of microplastics and thus will need to be taken into account for the realistic prediction of microplastics in the ocean. In the revision, we state that we do not account for biofouling, acknowledge its importance, and provide a reference to Kreczak et al. (2021):

"Marine microplastics can have complex non-spherical tangled-filament shapes, change their physical and chemical properties in time due to aging and photo- or chemical-decay processes (Andrady 2011), are subject to biofouling (see recent relevant work by Kreczak et al., 2021), and may interact leading to the formation of clusters. None of these effects were considered in this paper, and all will need to be taken into account for the realistic prediction of marine microplastic evolution and re-distribution in the ocean."

c. the particles are non-spherical;

Adjustments to the coefficients within the Maxey-Riley equations can be made to account for elliptical shapes (see, for example, DiBenedetto et al, 2018a and references therein). However, real microplastics often have complex tangled-filament-like shapes which are poorly represented by an ellipsoid, and no corrections for tangled filaments are currently available. This explanation has been included on pp. 8-9 of the revision.

d. the particles are negatively buoyant?

Our theoretical results apply to both positively and negatively buoyant particles ($R = \frac{2\rho_f}{\rho_f + 2\rho_p}$ is >2/3 for the former and <2/3 for the latter). Thus, according to Eq. (8), buoyant particles tend to accumulate in regions with positive $Q_a$, i.e., in eddies, whereas dense particles aggregate in strain-dominated regions. Since we are interested in vortex flows reminiscent of ocean eddies, we focus on buoyant particles. This has been explained on p. 13 of the revision:

"For dense particles, contraction occurs in areas dominated by strain, and it has been shown that aggregation of heavy particles can occur in strain-dominated filaments that arise in particle-laden turbulent flows, though the considered particle-to-fluid density differences tend to be quite large (see Brandt and Coletti, 2022 for a review). In our study, we will focus on vortex flows reminiscent of ocean eddies, and on lower dimension objects within eddies that can act as attractors for buoyant particles."

Furthermore, I have the following minor comments

- line 41: is it also important to state that these particles are spherical?

Yes, added "spherical."

- line 59-64: these statements here need more references. For example, are concentrations really largest at the sea surface?

We thank the reviewer for this question and admit that the global spatial distribution of marine microplastics in 3D is not well known. In the revision, on p. 4 we expanded the paragraph in question and included more references:

"Observations of marine microplastics have been conventionally carried out using net tows and mostly occurred at or near the sea surface (van Sebille et al., 2015). However, the density of many types of microplastic particles, including high-density polyethylene, is sufficiently close to that of sea water that suspension within the water column for long periods of time is feasible. For the near-surface microplastics, Kukulka et al. (2010) and Kooi et al. (2016) present observational evidence for the fast decay in concentrations with depth over the top 5 – 20 m of the water column, with the vertical penetration of plastic particles dependent on the wind speed. Pabortsave and Lampitt (2020), on the other hand, show observational evidence for much deeper, below-the-mixed-layer subsurface peaks for three common types of microplastics in the Atlantic Ocean. Processes such as biofouling and bio-geo-chemical or photo degradation might increase the density of the particles and eventually lead to the sinking of microplastics from the surface into the deeper part of the water column (Kaiser et al., 2017; Kreczak et al., 2021; Kvale et al., 2020). Consumption by biomass with the subsequent downward vertical transport is another vehicle for redistributing microplastics from the surface down. For example, Choy et al. (2019) suggest that this mechanism, specifically, consumption by pelagic red crabs and giant larvaceans, was responsible for the subsurface peaks in particles concentrations observed at depths near 250 m in Monterey Bay. Thus, microplastics have been found well beneath the ocean surface, but less is known regarding their spatio-temporal and size/density distributions (Shamskhany et al., 2021)."

- line 72: Is Froyland et al (2014) the most appropriate reference for this statement?

The reviewer is correct, a more appropriate reference for the importance of the 3D circulation is Wichmann et al. (2019).

- line 84: E.g. https://www.nature.com/articles/s41598-020-72898-4 also uses an Eulerian approach

We thank the reviewer for a relevant reference, which has been added to the revision: "In a similar fashion, Kvale et al. (2020) propose an Eulerian model for the biological uptake and the resulting re-distribution of microplastics." Note, however, that Kvale et al. (2000) do not account the inertial (non-water-following) effects in the movement of microplastics (Maxey and Riley framework is never mentioned or referenced there).

- line 100: is there being an attractor a sufficient condition for aggregation to occur? Or do other conditions also need to be fulfilled?

The presence of an attractor is not sufficient for aggregation to occur. Because each attractor is associated with its corresponding basin of attraction, if particles are introduced outside of the basin of attraction, they will not be attracted and will not aggregate towards that attractor. This explanation has been added to the revision on pp. 6-7.

- line 103: added mas is not introduced yet, at this point

We rewrote the sentence in question.

- line 104: how is drag different from inertia?

We rewrote the sentence in question (the distinction between drag and inertia will be explained later after Eq. (1) on p. 10).

- line 129: a motivation for why the lift force, the Basset history force and the Faxen corrections are omitted is missing. This should be motivated

Faxen corrections account for the variation of the flow across the particle and are proportional to a^2 \Delta u. For a particle size that is much smaller than the typical length scale of the flow, these corrections are small and typically neglected (Haller and Sapsis, 2008; Beron-Vera et al., 2019)

The history term, which is an integral along a particle path, accounts for the boundary layer effects that a particle leaves behind. It is typically ignored under the assumption that the chances of other particles crossing that localized boundary layer before it decays are small. Similar arguments were used to neglect this term in Beron-Vera et al., 2019, see also Langlois et al., 2015 and Daitche and Tel., 2011 for more info on the influence of the history term on the behavior of particles.

Finally, the lift force arises when a particle rotates in a horizontally sheared flow. As shown in Beron-Vera 2019, the inclusion of the lift force leads to the next-order, $O(\tilde{\epsilon}^2)$ correction in the slow-manifold approximation, and thus can also be neglected for small $\tilde{\epsilon}$.

This explanation has been added to the revision on p. 9.

- line 162: known in advance of what?

We have rewritten this sentence as "A chief advantage of the slow manifold reduction is that the $6^{th}$ order system given by Eqs. (2) and (3), in which particle velocity needs to be solved for, is reduced to a $3^{rd}$ order system given by Eqs. (2) and (4), where the particle velocity is explicitly written as a function of fluid velocity and flow and particle parameters (and thus is known)."

- line 174: could the surface then also been seen as an attractor, possible one 'external' to the fluid?

Yes, we have added this observation to the sentence in question.

- line 176: 'similar' instead of 'the like'?

Replaced "the like" with "similar".

- line 198: give examples of these cases?

There are many examples where the pressure gradient provides the major source of stress. In the manuscript we have added:

"In many cases, including quasigeostrophic eddies and gyres, internal waves, and the surface gravity waves considered by DiBenedetto et al. (2018a,b) and all inviscid flows…"

- line 234: which phase space? Physical space?

Yes, phase space is indeed the physical space. To avoid confusion, we have eliminated the mention of phase space and clarified this sentence as: "The area or the patch thus expands as particles are drawn towards the center of the vortex."

- line 246: give references for these 'numerous authors'?

We have added references: "Our model is based on the incompressible flow in a rotating cylinder (Greenspan, 1986), which has been studied in many configurations by numerous authors as a model of ocean circulation (Hart and Kittelman, 1996; Pedlosky & Spall, 2005), ocean eddies (Pratt et al., 2014; Rypina et all., 2015), or industrial processes and engineering applications (Lopez & Marques, 2010 and references therein), and can be easily set up in the laboratory setting (Fountain et al., 2000; Lackey and Sotiropoulos (2006))."

- The manuscript is very sloppy with Equation references. Very often, the word 'Eq.' Misses before an equation is references

We have added "Eq." throughout the manuscript.

- line 283: It would be helpful to start a new subsection here, detailing with fluid trajectories

We have added an opening sentence to this paragraph to streamline the narrative: "We now review the main features of the Lagrangian circulation in the rotating cylinder flow."

- line 285: is there an estimate hat 'a sufficient length of time' is?

Yes, we have clarified as follows: "The typical torus is associated with quasi-periodic trajectories and any such trajectory, followed for a sufficient length of time so that it completes many overturning and azimuthal rotations around the cylinder, will sketch out the torus in 3D."

- line 433: How are the particles integrated? What time-stepping scheme? What integration scheme? Which boundary conditions?

We use the variable-step 4-th order Runge-Kutta integration scheme, which we implemented in Matlab via the built-in function "ode45". In our simulations, the relative and absolute tolerances are set to the value of $10^{-9}$ to integrate particle trajectories (Eqs. (2) and (3)) (our results were not sensitive to the further decrease in tolerance values). Since the flow (Eqs. (9a,b,c)) is prescribed analytically and has no normal flow component at the perimeter and top and bottom of the cylinder, no interpolation scheme is needed and no extra boundary conditions are enforced during the integration. Integration of a trajectory is stopped when a particle got within one particle radius from the cylinder walls or top/bottom. This information has been added to the revision on pp. 25-26.

- line 474: If these are non-dimensional units, they seem like enormous(?) particles, at 0.1% of the size of the eddy?? Or how does the scaling of $d$ work?

The reviewer is correct, in dimensional units, our parameters correspond to a 1 mm (or 0.5 mm in some simulations) particle in a rotating cylinder with a diameter of 1 m.

- In general, it would be helpful if the authors could also provide animations of their simulations

In the revision, we have included one animation showing the aggregation to the single attractor in the steady symmetric case (Experiment 1a in Table 1).

- The manuscript is also somewhat sloppy with math font in text. On some occasions (e.g. line 233, 367, 391), short equations seem to have been written in normal (italic) font, rather than math. This is especially a problem with negative numbers, where the minus kind of disappears.

We have consistently used math font for all equations in the revision.