Cover letter: OS-2022-0831

Dear Dr Paldor,

For this revised version I have obtained 3 reviews and you can see that reviewer#3 raises some major points. The manuscript needs a revision to set the work into a better perspective, regarding the earlier work pointed out by reviewer#3.

I hope that you will agree to revise your manuscript to take into account reviewer#3's remarks.

Best regards,

Anne Marie Treguier

Response:

Dear Dr. Treguier,

We are happy that referees #1 and #2 have recommended publication of our work in OS.

As for referee #3 who recommended rejection. We feel that this review is based on a general criticism of the, frequently used, β-plane approximation and Inertial Oscillations there, which is irrelevant to the (wind) forced problem we study. Though one of us (NP) has published over 20 papers dealing with various aspects of GFD in spherical coordinates (including inertial motion, waves and instabilities) it should be recognized that very few readers of OS are familiar with this new branch of GFD and most oceanographers feel much more comfortable with the planar version of GFD (this is current situation which we hope will change in the future). As in many other problems – finding a solution on the β-plane provides a good starting point for finding the spherical solution. We are presently engaged in the initial stages of extending our approach to spherical coordinates, a goal more complex than in Cartesian coordinates.

While the referee acknowledges some novel findings of our work (the effect of the equatorial dynamics on the mid-latitude trajectories) s/he completely ignores the new findings regarding the zonal drift (that can be directed eastward and is independent of the sign of the wind-stress). The referee does not provide an alternate reference for quantifying the time it takes the column of water to reach the equator ($t_{cr} = \frac{1}{2b\Gamma}$ in our work) which is quantified straightforwardly in our approach.

Given the irrelevance of Inertial Oscillations to our **forced** problem and the complete ignorance by the referee of our results regarding several aspects of the dynamics, we find the REJECT recommendation of this referee too harsh and unsubstantiated.

Authors response to report 2 of referee 1: OS-2022-0831

Thanks

Authors response to report 2 of referee 2 – OS-2022-0831

I am satisfied by how the authors addressed my comments and find the presentation much clearer now. My second point below warrants a minor revision I believe, which I apologize for not having flagged the first time around. If it is a simple misunderstanding that the authors can clarify easily, I am happy to see this paper accepted without another round of reviews.

1. P. 8: I am slightly confused about the articulation between text and Figure 4. To be honest, I should have been during my first review, but better late than never? You describe the green curves as the 'monotonic evolution (averaged over oscillation) of x'. First off, averaging over oscillations only gives the low-frequency evolution, which is monotonic for the blue curve, but not for the red curve. So, I would just talk about averaging over oscillations, or phase averaging. Second, does this expression describe <x> (Eqs. 20 and 21)? If so, you might want to just write that, it makes it easier to hunt for the expression.

> ***Done, the text now clarifies that the two green (now black) curves are indeed calculated from equations (20) and (21) and that these curves accurately capture the non-oscillatory part of the numerical solution.***

2. Same place but different question (I think). Why do the green curves in Figure 4 diverge from their respective solid curves near their ends? You never plot them until t_cr, it is as if Eq. (21) is not necessary, even though you refer to the long-term evolution of the trajectories quite a lot before and after. You might want to discuss it here, or refer to further discussion (I don't think there is any, but I might not have read the second version carefully enough).

> ***Done, the caption of figure 4 now clarifies that the two black curves are invalid in the vicinity of $t = t_{cr}$ (i.e. near the equator) since $\omega_0 \to 0$ there, so adiabaticity does not prevail.***

<u>Authors response to report 1 of referee 3 – OS-2022-0831</u>

While this work has merit as a mathematical exercise, its value in understanding the ocean is less clear.

The problem of inertial oscillations on the rotating earth has already been solved cleanly and exactly by Early (2012), following Ripa (1997), see eqn. (16) therein. These equations are simpler than those presented in the paper under review, as well as being free from questionable approximations. Moreover, unlike the equations in this paper which require numerical integration, the solution on the rotating earth have exact analytic solutions, see Appendix A of Early (2012)—which in fact extended earlier work by the first author of this paper. Therefore, I am unclear in principle what a beta plane study might add.

The virtue of using the beta plane equations for inertial oscillations was challenged by Ripa (1997), who showed that they are inconsistent in that they do not improve the representation of angular momentum from the f-plane case, and give an incorrect zonal drift velocity. An improved alternative was suggested therein. In other words, the starting equations are potentially flawed. These issues are not mentioned by the authors.

On a more intuitive level, inertial oscillations are known to arise from the interaction of the departure of the earth from sphericity as a result of its rotation, leading to a component of true gravity that is tangent to the surface of the earth. This has been clearly articulated by Durran (1993) and Early (2012). Thus, one would not expect that simply adding the beta term to f would lead to more realistic representation of inertial oscillations, as they involve curvature and the already-neglected centrifugal force. The use of the beta plane equations for the problem at hand therefore requires rigorous justification.

We fully agree with the issues raised by the reviewer regarding the inconsistency of the β-plane approximation and its geometric oversimplification of Earth's curved surface. However:

1. The community of physical oceanographers is familiar with the planar geometry much more than with the spherical one. Take Rossby waves as an example: probably less than 1% of OS readers know the expression for these waves in spherical coordinates while nearly all of them know it in Cartesian coordinates.

2. We view the present work as a first attempt to extend Ekman's theory beyond the original f-plane set-up employed in 1902! In the last paragraph of section 4 we note that the present work should be extended to spherical coordinates. However, as reviewer 2 noted in

his/her first report: "... but one would have to start with the present analysis.". In our initial examination of the spherical geometry extension indicates that the ideas developed in the present, simple, work can indeed be used in spherical coordinates but their application is much more complicated mathematically (and this highly mathematical future application might not be suitable for publication in OS).

Finally, I had questions about the parameters regimes being explored. To be of oceanographic interest, gamma and other parameters should be chosen to be physically meaningful. In other words, if the solutions presented have particles moving at 1000 km / hour, that would not be very interesting. Therefore it is essential to connect the parameter space of solutions back to physically relevant values.

The present version only employs realistic values in the numerical results and figures 2-5 were modified accordingly. This point is highlighted in lines 200-205. The dimensional values of the free parameters b and Γ were discussed in the previous version as well (see lines 85-89 in the revised version).

The most interesting part of the paper to me was the articulation of the equator as a kind of trapping point via the adiabatic analysis. Despite my concerns, if the use of the beta plane could be justified, either globally or in the vicinity of the equator, and the connection to the spherical solution made clear, I could see it becoming a useful contribution.

We agree with the reviewer that the effect of the equatorial dynamics on the mid-latitude trajectory is novel. However, without our theory one could not have estimated the time it takes a column of water originating at latitude ϕ_0 to reach the equator ($t_{cr} = \frac{1}{2b\Gamma}$). We also view our results of the zonal drift, and in particular its independence on the sign of the wind stress, as novel (we don't know of an alternate study the provides this estimate). In particular, for strong but realistic wind stress or for zero initial conditions our work demonstrates that the zonal drift is directed eastward and not westward as could be wrongly concluded by extrapolating the westward drift of Inertial Oscillations on the β-plane. It is also of value to confirm analytically intuitive arguments that are based on the f-plane theory.