

ANSWER TO REVIEWER

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acknowledgement

We are grateful to the reviewers for thorough review which has helped to considerably improve the manuscript. Some of the reviewers remarks are directed towards our English and the style of the paper. We do not really know how to proceed to satisfy the reviewers demands in this respect. We are not native English speakers and we are very well aware of our limits in this respect and we are certainly aware of the fact that we do not have style when writing in English. We tried to do our best to reformulate along the reviewers comments.

The reviewers comments are reproduced in blue and our answers are written in black and the changes added to the manuscript are given in red. The corrected manuscript is attached.

Answer to Reviewer # 1

1. Introduction

The section needs rewriting both to make it a better introduction and to improve the English - both will help the reader. However having said a good copy editor is required I will usually ignore this aspect in the rest of the review.

Paragraph one jumps from global western boundary currents to the Brazil and Somali Currents, but seems to know little about them. For example the North Brazil Current is just one part of the full sub-tropical gyre in the North Atlantic and the Somali Current is driven by winds which are strong only where the East African Jet leaves the coast off Somalia.

We choose not to discuss the detailed current systems connected to the SC and the NBC but refer the reader to published results in the text as follows:

We refer the reader to Richardson et al. (1994); Garzoli et al. (2003); Fratantoni and Richardson (2006) for a detailed discussion of the NBC and the surrounding subtropical gyres in Atlantic Ocean. A detailed description of the circulation of the northern Indian Ocean, of which the SC is the most energetic part, is given by Schott and McCreary (2001); Schott et al. (2009); Beal and Donohue (2013); Beal et al. (2013).

Paragraph one also discusses the slope of the coastline and then ignores this factor in the rest of the paper.

Not mentioning the influence of the inclination of the coast line could appear as if we were not aware of it or that we considered it not to be important. We now added the sentence:

The important influence of the inclination of the coast line will not be addressed here.

and further below:

In the same spirit several idealized numerical studies with slanted western boundary noted the crucial importance of the slanted boundary in gyre generation (see *i.e.* Cox (1979); McCreary and Kundu (1988)).

Page 755, line 4, refers to "a large number of numerical work". This needs citations.

Now cited.

Page 755, lines 5-15. This is a new subject which could do with a new paragraph. Placing the subject (idealized western boundary currents) at the end of a long sentence does not help the reader. In fact there is a huge literature on idealized western boundary currents - what you may have left out is the effect of time dependence.

Yes this new subject has been wrote now as a new paragraph. The long sentence has been improved as follow:

A detailed determination of the vorticity balances, fluxes and stability of idealized low latitude turbulent WBCs have been performed by Edwards and Pedlosky (1998a), Edwards and Pedlosky (1998b) on the deep WBC and by Fox-Kemper (2005) on the dynamics of single and multiple gyres in a barotropic constant depth β -plane model.

Page 755, line 22. I presume you are not really saying that the Munk theory is both a laminar flow and inertial flow theory. This could do with some basic citations as well as those concerned with stability.

Now corrected. Now cited

We now rewrote the introduction along the lines of criticism of the reviewer.

2. The Model

Page 756, line 12 and elsewhere. The regular use of the equals and other mathematical signs in the main text is distracting. Except in exceptional circumstances they should be removed.

They are removed and we added the table 1 to give the values.

Page 756, line 18. This could do with a sentence on the gravity wave speed and also possibly the range of Rossby wave speeds to expect. The authors should explain what water properties of the Indian Ocean 'inspired' them.

The gravity wave-speed is now given in the table. We are not certain to understand the second part of the question. The depth of the thermocline and the density difference due to the variation of temperature varies in the region considered, the values we choose are typical for the area.

Page 757, line 4-6. Quantities, C1, C2, rho and eta remain undefined. It may be clearer to put the equations first, then define the terms and finally specify the size of the basin and other constants.

We now removed the coefficients C1 and C2 and give the forcing explicitly as suggested by the reviewer, ρ and η are now defined.

Page 758, lines 4-5. The use of (c1,c2)=(x,0) or (0,y) is messy. Why not move the constants to equations 6 and 7 and call one the Monsoon Forcing and the other the Trade Wind Forcing?

See our answer above.

The paper needs to explain and justify the dependences on x and y? It would help if a figure was used to show the pattern of vectors.

The paper already has too many figures and we prefer to put figures that show results. The analytical formula of the forcings are given in the text. We added the schematic winds in Fig. 1 of this answer. If the reviewer insists we will of course add them to the paper. The wind-stress forcing paragraph has been rewritten to take account of the remarks as follows:

As in McCreary and Kundu (1988), the model ocean is forced by wind fields that are composed of one or more patches of the form

$$\tau = \tau_o X(x) Y(y) s(t).$$

Values of the wind strength τ_o are specified below for each type of wind forcing. The offshore and the alongshore structures, $X(x)$ and $Y(y)$, are also described.

The wind-stress implemented in Eqs.(1) and (2) is discriminated into Monsoon Winds forcing

$$(MW) \begin{cases} \tau_x = 0, \\ \tau_y = 0.35 \cdot [\exp(-4(\frac{x}{L_x})^2 - 0.2)][1 - \exp(\frac{-t}{t_c})]. \end{cases}$$

and Trade Winds forcing

$$(TW) \begin{cases} \tau_x = 0.4 \cdot [1 - \exp(\frac{x}{L_x})][\exp(-4(\frac{y}{L_y})^2)][1 - \exp(\frac{-t}{t_c})], \\ \tau_y = 0 \end{cases}$$

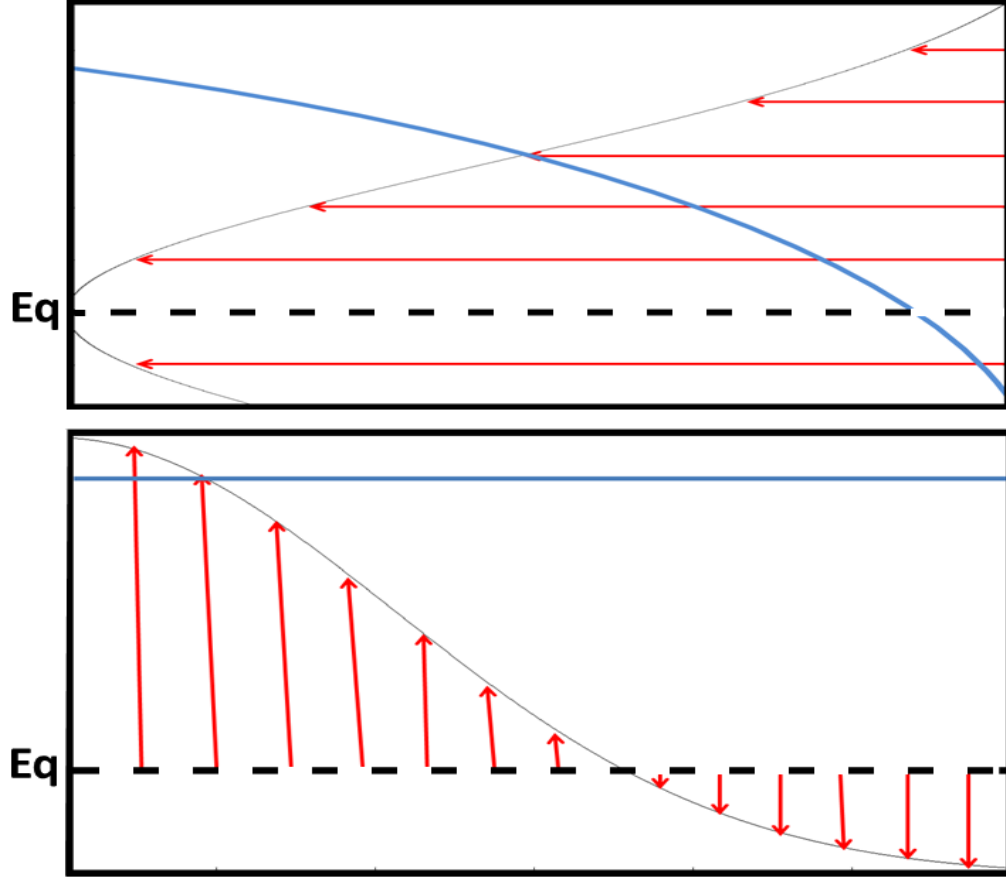


Figure 1: Schematically illustrations of the offshore and alongshore structures for TW-forcing (top) and MW-forcing (bottom). The eastward decrease of magnitude of the TW-forcing is represented in blue line (top) as described in the Eq. 10. The magnitude of MW-forcing is kept constant with longitude, it is represented in blue line (bottom) as described in the Eq. 9.

In the Atlantic the Trade Winds decay towards the east. In our calculations the decay towards the east also prevents the vanishing of the surface layer.

2.4 Numerical Implementation

Page 758, lines 11 onwards. The paper needs more details about the numerical scheme. In particular which Arakawa grid was used and what is the justification? What advection scheme was used? What are its energy and enstrophy conserving properties? If the u and v velocity points are separated, how does the Coriolis scheme conserve energy?

The paper says that a second-order Runge-Kutta scheme was used. What is its stability properties? As I understand it such schemes do not conserve energy and this may explain why such a short time step was needed.

page 759, Lines 1-4. The gravity wave and the largest advective speeds reported in the paper lie near 2 m/s. With a grid size of 2500 m, a time step of 90s, corresponds to a 'speed' of over 25 m/s. This section says that the short time step is due to the high vorticity (velocity?) in the boundary layer. If this is really true then it should be easy to show that there are large velocities (of order 10 m/s or more) in the boundary layer and the paper should do this.

In the present paper we consider the dependence of the dynamics on the viscosity parameter (Reynolds number). A large number of today's ocean models is constructed using schemes which are known to produce stable solutions even when the explicit viscosity parameter is vanishing. The spatial schemes are often up-wind to increase stability and the grid geometries are such that the variables are not collocated (Arakawa B-, C- or D-grid). These grids need some spatial interpolation which increases stability. This leads to numerical schemes which are artificially stable and many of them are integrated with a vanishing explicit numerical viscosity. In a real turbulent flow, with a finite resolution, viscosity is needed to stop a direct cascade of energy or enstrophy which would otherwise pile up at the smallest scales resolved (see Frisch et al. (2008)), this is even more true in a forced model. This zero-explicit-viscosity models are unphysical and it is not clear which mathematical equation they solve as the underlying (zero-viscosity) equations have no solution. This is even more relevant in a forced model. A zero viscosity flow is also inconsistent with no-slip boundary conditions. The above shows that a realistic numerical model should blow-up if integrated above a certain Reynolds number. This is what our model does. Stability is not to be confused with precision. We use a second order scheme in space-and-time and to take advantage of the high-order we use fine resolution in space-and-time. Indeed high-order schemes run at coarse resolution perform often worse than lower-order schemes. In our calculations the boundary current is always resolved by more than 7 grid-points and the CFL is always lower than 0.2 (considering $u_{current} + u_{wave}$). In calculations of turbulent flows by the engineering community, a CFL of 0.3 is often used. It is a short time step that allows the full benefit of a high order scheme. This has to be compared to many integrations of ocean models which are run at the CFL limit with zero viscosity and which have a grid-scale close to the thickness of the boundary current.

The short time step used in all calculations is for numerical accuracy and it is no surprise that for the more accurate model, having a shorter time-step, the integration

converges whereas the less accurate models blow-up for the lowest viscosity used. Runge-Kutta methods are extensively used in numerical calculations of large variety of fields. The present scheme was tested and successfully employed in Wirth (2013) for example. We successfully tested the code on a mid-latitude gyre. For the higher values of viscosity analytical solutions are approached. For the lower values of viscosity, anticyclones appear as that observed in nature and our results compare well to previously published calculations performed by different researchers with other models. For the lowest values of the viscosity, bursts and dipoles appear. These structures appear also in observations and laboratory experiments. None of the dynamics and structures in our model are exceptional, but our model allows to study their detailed spatio-temporal evolution and their statistics, due to the high resolution in space, the short period between successive snapshots and the long integration time.

We added:

using an Arakawa A-grid,

The scheme was successfully tested changing resolution in space and time in Wirth (2013).

We now clarified the sentence discussing the vorticity in the bursts by adding the meaning of high vorticity which is the viscous sub-layer.

4.0 Results

Page 760, lines 3-21. This is written as though no previous work has been written on the response to the ocean to wind forcing. The Trade Wind example has the curl of the wind stress differing north and south of the equator - so two gyres are formed. With the Monsoon it does not change sign so there is only a single gyre. As the problem is on a beta plane the equator would not be expected to affect the width of the western boundary current in any way.

In the same way as the underlying Ekman transport is not affected by viscosity - most people would not expect the large scale circulation to change much. Thus the fact that it is (qualitatively?) unchanged is more a validation than a result of the model.

Yes, the reviewer is right, we now added the word validation to the title of this subsection. We also superimposed the velocity field on the figure 1. We improved this subsection by adding:

Before proceeding to the more complicated non-linear solutions, it is useful to review the familiar, linear solution which can be determined from a combination of *Sverdrup theory* [Sverdrup (1947)] and *Stommel theory* [Stommel (1948)] with a *Munk-layer theory* [Munk (1950)] and *inertial-layer theory* [Charney (1955)]. It provides a useful baseline to compare the more complicated non-linear flows for the two sets of experiments. Shown in Fig. 1a and Fig. 1b are layer thickness variation (η) contours and horizontal velocity arrows from the laminar experiments MW1000 and TW1000, after the spin-up time, at $t = 2000$ days. Velocity vectors are plotted every 20×50 grid points in the x and y directions. These figures show the classic Sverdrup interior solution with a Munk or inertial boundary-layer (discussed in detail in the next section). The velocity vectors are small in regions where the flow is weak and difficult to distinguish in the figures. In these regions, the sense of the geostrophic flow can be inferred from the η -field.

Page 760, lines 24-26 and following. The analysis reported throughout the paper is mainly

concerned with vorticity balances. However unlike turbulence in a pipe, western boundary currents are themselves the end limit of a physical process in which Rossby waves effectively concentrate energy at the boundary. In Munk's theory, i.e. at high viscosities, there is then a balance between the longshore pressure gradient and viscosity which is as equally important for the physics as the balances in the vorticity equation. At low viscosities there is a similar alongshore balance which is equally important, and which might give useful insights into the generation of bursts reported later in the paper.

The point I am trying to make is that by concentrating solely on the vorticity, the paper's approach appears to be rather narrow and it may be missing some key physics.

Velocity vectors are now given in Fig. 1 and Fig. 3. The velocity profiles are shown in Fig. 2. The burst generation resides in the vortical dynamics. The coherent structures are the most visible in the vorticity plots and we explain their genesis and live cycle in terms of vorticity balances. We do not feel that we concentrate solely on vorticity but we use it extensively, as other publications do.

Page, 761, line 2. The paper needs to discuss the validity of this equation - especially the wind stress field (which is presume is constant in an east-west direction) used and provide suitable references. Note also that this solution oscillates, i.e. v changing sign a number of times, whereas the model solutions give single gyres. Real oceans appear to be similar. The discussion of the Munk layer is given in Munk (1950); Pedlosky (1979, 1990), as cited in the paper and is part of today's text-books on ocean dynamics. Please note that the oscillations decay exponentially with the distance from the boundary.

Page 761, line 7. If you go to the original paper I think you will find that Munk does not base the theory on quasi-geostrophy. Both the Munk and Stommel solutions are in geostrophic balance away from the western boundary current but they obtain a solution by throwing out all the non-linear terms and do not start by assuming an approximately geostrophic solution.

Yes the reviewer is right, we change it in the manuscript:

Munk-layer theory solution is in geostrophic balance away from the WBC. The solution is obtained by assuming a vorticity balance between the advection of planetary vorticity and lateral viscous dissipation and neglects variations in the layer thickness which are important in our reduced-gravity model (see section 4.6) at low latitude.

Page 761, line 13. One of the most important criticisms I have of this paper is that the model is not validated properly. It uses an unknown grid, with unknown advection and Coriolis schemes, an unknown method for specifying boundary conditions and a time stepping scheme which does not conserve energy. In addition, being a real ocean model, it probably contains at least one programming error not known to the authors.

All that is provided in the way of validation is figure 1, which cannot be compared with anyone else's results and figure 2, which is very unclear and to me at least is very dubious.

The scheme used is identical to the one of Wirth 2013 ("Inertia-Gravity Waves generated by near Balanced Flow in 2 Layer Shallow Water Turbulence on the β -Plane", *Nonlin. Processes Geophys.* **20**, 25–34, 2013). All the results that we obtain with our model compare perfectly to published results (Munk-layer, inertial layer, anticyclone size and strength). The smaller scale structures as bursts and dipoles are observed in the ocean

(see Beal and Donohue (2013)) and in laboratory experiments (see Van Heijst and Flor (1989); Robinson (1991)). So, we like to say that none of the results of our model comes as a surprise. The new approach of our research is the high resolution and the large statistics (long-time integration with many snapshots) which allow the analysis we performed. Please see also our answer to the reviewers remarks on the numerics section. Furthermore, concerning the validation, we successfully tested the code which reproduces well the mid-latitude gyre.

If we consider first the Munk solution. As the wind stress field is known, it should be possible to obtain an analytic solution for the whole domain. As the wind stress varies with latitude, this should show a latitudinal dependence - but the red lines appear to be unchanged. It should also be different for the Trade Wind and Monsoon solutions. It isn't.

Secondly an inertial solution is plotted. An equation (what is u_I ?) and references are given but, especially as the inflow result is important elsewhere in the paper, the section needs more on how the equation is derived and, especially, the limits for which it might be valid.

The Monsoon Winds forcing does not vary with latitude. In the Trade winds case (inverse) inertial effects become important and the Munk layer solution is strongly modified. No analytical solution is available. We plotted the analytical Munk and inertial layer solutions to allow the reader to compare them with the model solutions.

We now added: $u_I(y)$ is the westward zonal velocity just outside the boundary layer, responsible for the inertial effect (see Pedlosky (1979, Sect. 5.6)).

And when all this is done, in figure 2 the lines do not lie close to each other, there is no quantitative measure of the error and I was not overly convinced by the explanation of the differences. It needs more.

The plot of analytical Munk-solution is done with the same arbitrary amplitude at all the latitudes, just to have the best fit with the two high viscosity experiment solutions. In the two forcing experiments only the Trade Winds forcing experiment exhibit a westward zonal velocities (inertial effect) just north of the equator. So the amplitudes of the inertial solution are chosen to best fit it at different latitudes near the equator.

Page 761, lines 9-26. This is a discussion of the vorticity balance which is not supported by any of the results presented here. Later in the paper the distribution of the different terms is plotted but the statements made here need more justification. I think that this section really needs a (large, clear) figure which justifies the statements made. In addition I would suggest that the important terms are discussed before the smaller terms.

We didn't show the vortex stretching because as we said in the text:

"We found the vortex stretching to be important very close to the boundary but decreases rapidly before the meridional velocity reaches its maximum (not shown), but does not lead to substantial deviations from the Munk-layer and inertial-layer solutions as can be verified in Fig.2".

The vorticity balance and fluxes in idealized (shallow-water models) are discussed in detail in the papers by Edwards and Pedlosky (1998a), Edwards and Pedlosky (1998b), Fox-Kemper and Pedlosky (2004), Fox-Kemper (2004) and Fox-Kemper (2005), we compared

our results to theirs and found no significant differences. This discussion is to introduce the fact that our laminar solutions reproduce well the other already known results of the laminar vorticity balance.

Coherent structures

Page 762, line 16. How does the speed of the eddies compare with the speed of the boundary current?

We did not find a mechanism nor a relation which connects the two quantities. Furthermore, for the lower viscosity values the dynamics is turbulent and it is difficult to define eddy speed and the speed of the boundary current.

Page 762, line 20. How does the size of the eddies compare with the width of the current or the distance between the coast and the current maximum?

Same as for the previous question: it is difficult to clearly define eddy-sizes and width of the boundary current in a turbulent dynamics. That is why we introduce the turbulent length scales in Section 4.4.

Page 762, line 24. Again how does the speed compare with the speeds in the boundary current.

We give some values of the velocities which compare well with observations. Again it is difficult to talk about speeds in the boundary current as it ceases to exist, it is composed of a succession of eddies. So one could say that the fluid speeds in the eddy are the speeds of the boundary current. In the turbulent case we even have counter currents as explained below.

Page 763, line 6. The discussion here is vague. It needs either some good figures or some other analysis to illustrate how random-like behavior develops with lower viscosities. Terms like lower and lowest viscosities, used here and elsewhere in the paper, need to be replaced by more specific definitions.

Page 763, line 11. This gives an interior velocity of 2 m/s, the value given earlier. In the next paragraph it is 2.4 m/s. Why the difference?

Yes the reviewer is right. It is a typographical error, we corrected it by replacing 2m/s by 2.4m/s in the text.

Bursts

Page 763, line 26. "simulations with the strongest velocity and vorticity gradients...". Not a surprising result but ... Which simulations are these? What are typical values? What are typical values in the cases where the detachments are weak or non-existent.

In the high viscosity experiments only a signature of onset bursts without detachments, forming pairs of positive and negative vorticity which slip rapidly along the boundary. In the experiments with viscosities $\nu = 500\text{m}^2\text{s}^{-1}$ and lower the detachments are observed. We added in the manuscript:

(with viscosities $\nu = 500\text{m}^2\text{s}^{-1}$ and lower)

Page 764, line 1. "the southward part detaches...". The southward part of what. What happens to the rest of whatever.

Yes the reviewer is right, that is the south part of the viscous sub-layer detaches (not the southward part), and the north part continues to flowing northward along the boundary as can be seen in Fig.4. We corrected this in the manuscript:

... south part of the viscous sub-layer ...

The north part of the viscous sub-layer continues to flow northward along the boundary. Page 764, line 3. "... and meridional velocity are negative". The figure is unclear, it needs to be improved, but what it does show is a northward boundary velocity every- where.

Please see Fig. 2 for this answer, which shows the Hovmöller of the meridional inversion in the viscous sub-layer of MW300 and TW125 experiments..

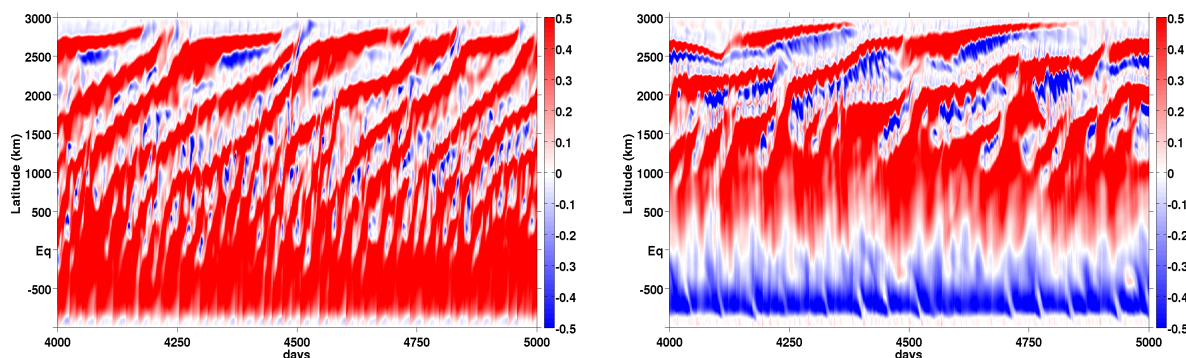


Figure 2: *Hovmöller of near wall meridional velocity v in the viscous sub-layer of MW300 (left) and TW125 (right) experiments.*

Page 764, lines 9-11. A key point but not really justified by the work reported. You need to show that a lower resolution grid does not produce similar instabilities.

Almost identical ejections are present in the realistic simulations with the NEMO in the Drakkar configuration. They appear at 1/4 degree resolution but are more pronounced in the 1/12 degree resolution (see Fig. 3 of this answer taken from Akuetevi et al. (in prep.)). These ejections are also present in observations through the cold wedges. The same is true for the offshore transport by the dipoles clearly present in the 1/12 degree resolution (but not well in the 1/4 degree). These dipole signatures are already observed by Beal and Donohue (2013) (their Fig. 6) and called "flanking cyclones". Ejections and the subsequent offshore transport by dipoles are observed in the ocean and captured by all fine resolution numerical models we are aware of.

Page 764, line 13. I think you need a definition or at least a few words making clear what you mean by laminar here. I usually associate it with a region which is not turbulent and in which the stream lines follow the boundary. Are you saying that the viscous layer is turbulent?

Yes, the viscous sub-layer is subject to strong intermittent ejections which penetrate up to the boundary. For the high viscosity experiments, signatures of "pre-bursts" are observed but the viscous sub-layer does not detached. So in these cases pairs of positive and negative vorticity anomalies are observed near the boundary which slip rapidly along the boundary. Detachments of the viscous sub-layer only occur for the lower viscosity experiments. The same is true for the viscous sub-layer in engineering fluid dynamics, where strong variations in space and time of the shear at the boundary are observed.

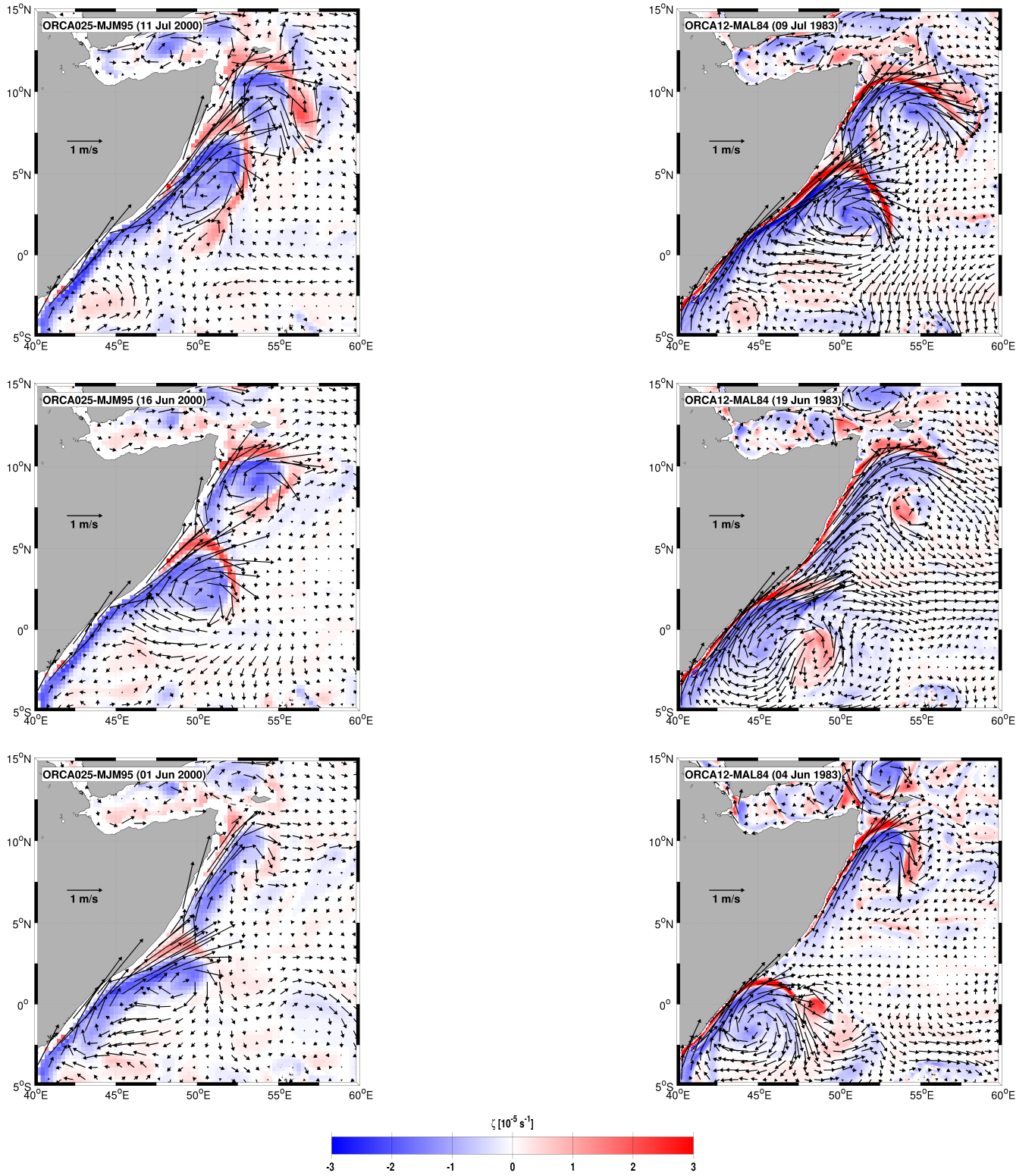


Figure 3: From bottom to top: sequence of occurrence of the bursts (created by the Southern Gyre and the Great whirl) and their subsequent development into dipole and the first two trajectories of the cyclone through the surface currents (vectors; $\text{m}\cdot\text{s}^{-1}$) and the relative vorticity (ζ ; color shading; s^{-1}) from ORCA025.L75-MJM95 (left panels) and ORCA12.L46-MAL84 (right panels) experiments. Landmasses are shaded gray.

page 764, line 16. It is not clear whether T2 is an average over time and distance of flow reversal regions or is an average over time of the number of reversals in the region. The phrase "percentage of bursts" is a bit unclear and to me seems melodramatic. Something like the "fraction of time with flow reversal" might be both more accurate and more informative.

Yes the "fraction of time with flow reversal" is more informative than "percentage of bursts", so we now changed it in the manuscript:

... fraction of time with flow reversal ...

Page 764, line 28. This does not appear to make sense - the result of the paper using confusing terminology. Bursts are partly a response to adding the non-linear terms and so including 'inertial effects' but this sentence says otherwise.

The inertial effects stabilize the western boundary-layer as proved in the section 4.4 and as can be seen in the Fig.5b of TW125 experiment. In the lower viscosity TW experiments, just north of the equator the extended boundary layer is nonexistent, proving the stabilize effect on the western boundary-layer. Further north of this region the inertial effects are absent so there are creation of large anticyclones which create bursts. Hence the sentence is right.

Scales of Motion

Page 765, lines 10 to 20. The problem here is really a matter of style. The start is OK - the paper is going to discuss two key quantities. It then gives a rather messy equation, gives it a name, adds some references - but only at the end, after the reader has lost interest, does it say what it is trying to do.

How about working the other way round, starting with where you are trying to go - i.e. in the first case representing the scale of the turbulent generating flows - and then discussing how you intend to get there, i.e. introducing the equation and justifying why that is the correct way.

None of the authors is a native English speaker and we are very well aware of our problems with the English language. The two scales are used in papers dedicated to turbulence. They are extremely useful when spectral analysis is not adapted due to spatial inhomogeneity (existence of a boundary). We introduce the scales, to warn the reader, that they apply only to turbulent dynamics. We then give the results and explain them. After that we give an interpretation for turbulence. In the end we mention the consequence of the results for numerical simulation of GFD-problems involving boundary currents. This seems to be a logical sequence.

Page 766, line 6. This seems to be a long winded irrelevant discussion. If you start with any velocity field with regions of zero velocity and zero gradients, any measures like eqns 10 and 11 are going to have both zeros and infinities. So what have you learnt?

This part has two purposes. As these scales are "new" to oceanography we wanted to make sure that they are well understood and it is interesting to note that even in the fully turbulent experiments, traces of laminar flows remain away from the boundary in the scale analysis (explaining the oscillations).

As turbulence generally spreads out it would be much more sensible to consider aver-

of the measures over some sensible area around each point.

Put another way - if you are sensible the measures might be useful for analyzing any flow. (See line 9).

The dynamics and all the quantities strongly depend on the distance from the boundary. The dynamics is strongly inhomogeneous along this direction. So variations along x contain information and are not due to insufficient statistics. We instead performed simulations over a long time to get statistically reliable results using time averages.

Page 766, line 11. This highlights the problem of using a description like Taylor scale instead of something that has a more obvious physical basis or usefulness. The use of a parameter which is infinite in regions where its value is unimportant seems ridiculous. It gives the impression that the analysis is not really getting to grips with the underlying physics.

The Taylor scale and the dissipation scale analysis are used in many publications on turbulence and explained in text books and review articles as cited.

Page 766, line 15. The clearly defined boundary seems very suspicious. What has stopped the eddy field extending further? We know the ocean is full of eddies - the Rossby waves transmit eddy energy everywhere. The model Rossby waves should be doing the same. Thus the existence of a boundary indicates that either the model has not run to equilibrium, or it has some bug in it, or there is some key physics which you have missed.

We carefully verified that the results do not change significantly when half the analysis period is used so we are very confident that our averaging period is sufficient. The larger Rossby waves have a westward group-velocity. The large values of the scales correspond to the absence of turbulence away from the boundary, where the viscosity is large enough to damp the turbulent motion. A rather sharp boundary (in a statistical sense) between a turbulent and a non-turbulent zone is observed in almost all applications of turbulence. An example are turbulent jets, where a discrete separation between a turbulent and a non-turbulent zone is observed and an angle of the spread of the jet can be defined. In our case we can even explain that turbulence stops when the dissipation scale equals the energy containing scale. Indeed, in the energy (and enstrophy) cascade the dissipation scale depends on the energy (and enstrophy) flux (and the value of the viscosity) if the flux weakens the scale increases (and if the viscosity decreases the scale decreases). In our example the only source of turbulence is in the boundary. In the real ocean turbulence in the interior of the basin is generated by the instability of zonal jets, interaction with topography and small scale time dependent wind forcing frontal dynamics due to outcropping of isopycnals and 3D turbulence in the mixed layer. all these processes are excluded by construction in our model set-up. We now added in the manuscript:

A sharp boundary between turbulent areas and a laminar environment is observed in many instances when turbulence arises from a single process such as turbulent jets, planetary boundary layers, gravity currents and stratified turbulence.

Page 767, lines 1-7. Until the previous point is addressed, this discussion of the width of the eddy intense region is not useful.

Moments of the velocity field

Page 767, lines 11-27. I sometimes feel that people study moments because they are there. Does the section add much to the paper? OK maybe don't cut it but if anything shorten it.

The section adds to the paper as in the disc model the third order moment vanishes. As said in the text it is a signature of the bursts and dipoles and represents a “non-local transport”, that is not down-gradient. The third order moment is a key quantity in many closure models and is often supposed to vanish (quasi Gaussian closures). In parameterizations of the convective planetary-boundary-layer the non-local transport is the key quantity. We are convinced that this section is essential to attempts for the parameterization of boundary layer turbulence.

Vorticity fluxes

Page 768, line 13 and following. It would help the reader if the paper used the acronyms RVA, PVA, TRVA, STR and FRIC as little as possible and instead tried to use more descriptive phrases within the text. Thus instead of saying “the FRIC dominates ...” how about “friction dominates” or “the friction term dominates” as you would in normal speech.

This might solve another problem in that the fluxes are really vectors, taking vorticity from one region of the model to another. Thus instead of saying that some ω is large the paper might be more effective if it said that the velocity field is advecting vorticity released by the beta term and advecting it into the viscous boundary layer.

We now changed that in the manuscript.

Page 769, lines 1-3. The paper needs to briefly justify these definitions of the width of the two sub-layers possibly with reference to plots of the quantities concerned. Readers cannot read your mind. Also given that large fluxes of vorticity may be involved, it is not obvious that such simple definitions are always valid.

We now added in the manuscript:

According to the shape of the different terms in Eq.(16) (shown in the Fig.7), we estimate...

Page 769, lines 3 to end of section. This section is over-long. The key point has been made more than once earlier in the paper. Choose what you want to keep and shorten it.

Figures 7 and 8 are extremely dense. Indeed all the results of our work are condensed in Fig.8, which can not be understood without understanding Fig.7 first. A repetition of previous results is essential to show that they are indeed visible in Fig.8 in a concise way. A detailed discussion of the different aspects are thus imposed, we would not like to omit a single one. Again we do not say that a shorter discussion is not possible, it is just we did not find a way to discuss it in a shorter way without omitting an important result.

Estimation of eddy viscosity

Page 770, line 10 to end of section. I like the idea of this section but again it seems to be hard work. The argument needs to be tightened up and the text shortened.

None of the authors is a native English speaker and we are convinced that the reviewer would have written it in a more concise way. We think that the arguments presented are clear and we do not know how to write it shorter without losing important information.

Page 771, line 5. Placing units within an equation like this is messy and unprofessional. It is simpler just to say " ... using SI units ...".

We do not agree, a physical equation needs units. This is also important as it shows that the kinematic viscosity is used, rather than the dynamic viscosity. It also shows, that a viscosity is composed of a velocity scale multiplied by a length scale. This compares to the molecular counterpart of a free mean path of a particle and the average particle velocity.

Page 771, lines 5 to 15. Given the statement at the end of the discussion that the result is 'based on the Prandtl formula', this seems to be the wrong way round. The paper first gives the result and only later discusses Prandtl. If you obtained the result and only made the connection with Prandtl later then say so and do not give the opposite impression in the conclusion.

Yes, at the end of the paper we write 'We propose here a numerical value, based on the Prandtl formula'. But in the derivation (section 4.7) we proceed in an empirical way. We first observe the affine correlation, thus recovering Prandtl's formula and connecting a free parameter to the eddy size. Which means we do not suppose the Prandtl formula to be valid from the start. In the discussion the eddy viscosity calculation is just summarized in one sentence, so much more concise. We still think that this presentation is wright way round, other choices are clearly possible.

Discussion and Conclusions

Page 771, line 13. They key result of this paper is not that "we have not observed that the vanishing of the Coriolis parameter at the equator plays a special direct role in the dynamics of western boundary currents". Come on - as well as sending everyone to sleep that is lecture room stuff for undergraduates - the beta-plane-101.

Personally I think your key up-front result concerns the scales and viscosities at which bursts occur and the resulting propagation of eddy pairs into the ocean interior. So I suggest that this goes first.

As this is "Discussions and Conclusions", you also need to discuss where such events should occur in the real oceans and if possible cite the evidence. It worries me that despite a large amount of satellite data they are not a well known feature of western boundary currents.

If they do not exist then is it an effect of continental shelves or rough continental slopes? How might these stabilize the viscous layer?

Your other key point is your estimate of the effective boundary layer viscosity due to small scale motions. Readers might ask whether this is valid for more complex 3-D ocean models so you should consider this.

The point about needing high resolution everywhere in ocean models is also well made - but really it only supports what those who go to sea have known for many years, i.e. that scales in the ocean are small almost everywhere.

Page 772, lines 18-19. This comment about the equatorial instabilities appears to be a new idea. Was it reported earlier in the paper?

The equatorial wave guide and the resulting time variability is part of text books. In the "discussion and conclusion" it is important to note that the wave guide dynamics

influences also the western boundary layer dynamics. Page 772, line 21. "The western boundary layer does not exist for high Reynolds number flows." It may not be stable but most people would say it does exist.

The sentence is now changed to:

Without the stabilizing inertial effects, the transport of PV towards the boundary area, the western boundary layer does not exist as a continuous flow for high Reynolds number flow.

Page 772, line 23-24. "Its boundary layer structure can only be recovered in a average sense." Do you mean its laminar structure, its simple analytic structure or something similar?

The sentence is now changed to:

A laminar boundary layer structure can be recovered in an average sense.

Page 772, line 26 and following. Style again - you discuss viscous, advective and extended and then jump to one, three, two. Why not say: the thickness of the viscous layer increases with viscosity, that of the extended region decreases and the advective region stays essentially unchanged.

We now changed that in the manuscript.

Page 773, lines 8-9. This needs decent references. It could do with a comparison between a burst and dipole formation in your model and say a satellite photograph.

The bursts dynamics is well documented in engineering fluid dynamics. In oceanography they occur when the resolution allow it and so it still poorly documented. Nevertheless their signature are already observed in sea surface height by Beal and Donohue (2013) and in NEMO (Drakkar configuration) by Akuetevi et al. (in prep.).

Now cited.

Page 773, lines 10-12. I thought that meso-scale eddies promote biological productivity all over the ocean. If you know of evidence that this only occurs in limited regions where boundary current generated dipoles occur then give a good reference.

Of course meso and sub-meso scale activity promote biological activity in many places. But in the Indian Ocean the burst plus offshore transport are important processes. we added the sentence:

The above is an example of how meso and sub-meso scale activity can increase biological activity.

Page 773, line 18. What evidence have you presented that "A rough boundary introduces a lower bound for the thickness of the boundary"?

The sentence is now deleted.

Page 773, line 27. Are you saying that the resolution of your model results is insufficient? What we were saying is that for the turbulent boundary-layer, the viscous sub-layer is thinner than the prominent Munk-layer on which spatial resolutions of todays simulations are based. We reformulated that in the manuscript:

The here presented results prove, that for the turbulent boundary-layer, the choice of spatial resolution based on the Munk-layer theory is far from being sufficient.

Page 773, line 28. The lower values of what?

The reviewer is right, we reformulated that in the manuscript:

For the same viscosity, the viscous sub-layer of the MW forcing experiments is thinner than in TW forcing experiments (see Fig.8), which explains why the experiments of the MW forcing were only possible down to $\nu = 300\text{m}^2\text{s}^{-1}$ while the experiments with the TW forcing converged down to $\nu = 125\text{m}^2\text{s}^{-1}$. Page 774, lines 5-8. I do not see why the gap (difference?) is a measure of the complexity of the numerical calculations? Your computer code did not get more complex.

In this discussion you need to remind the reader what your symbols mean. Even better do without symbols.

We replaced “gap” by “difference” in the manuscript.

I know what you mean by grid refinement at the boundary but to make it clear you should give references.

We now added:

..., that is using a finer grid closer to the boundary than further away, ...

There are a lot of works which used a grid refinement, but we choose not to give references as it would appear as criticizing of the works referred and grid refinement might well be appropriate in other applications.

Page 774, lines 10-11. What does ‘involved degrees of freedom’ mean?

As answered above, we would like to say, the involved degrees of freedom of the coherent structures and turbulent flows created at the boundary. To explain simply, we changed now the paragraph to:

The extension of the extended boundary-layer, the area over which turbulence is present, grows with decreasing viscosity whereas the size of the structures decreases. Therefore, higher viscosity runs require a higher resolution over a larger domain, increasing the complexity of the calculation.

Page 774, line 20. The confusion introduced by the use of the term ‘Inertial Theory’ has been discussed previously.

page 774, line 24. Which velocity profile?

The zonal velocity profile, we now added it in the manuscript.

Page 774, line 26. I do not agree. Equation 15 is a parameterisation - you have related viscosity to some velocity.

Yes the reviewer is right, we now corrected that in the manuscript:

Our determination of the eddy viscosity in section 4.7 via the Munk formula is a parameterization as we related eddy viscosity to the maximum fluctuating velocity.

Page 775, line 8. How could using fine resolution and low viscosity not generate eddies (i.e. ‘perform large-eddy simulations)?

We now reformulated the sentence as:

In other words, our findings discussed above suggest that, one can either use fine resolution and a low viscosity ($\nu < \approx 300\text{m}^2\text{s}^{-1}$) to simulate the turbulent boundary or, one can use coarse resolution and a high viscosity ($\nu \approx 6000\text{m}^2\text{s}^{-1}$) and recover the time-averaged boundary layer dynamics. Using viscosities in the interval $300\text{m}^2\text{s}^{-1} < \nu < 6000\text{m}^2\text{s}^{-1}$ leads to a wrong time-averaged boundary-layer dynamics.

Page 775, line 13. Low eddy viscosities might be expected to lead to a narrower viscous

layer. However this reads as if (a) you have made all the necessary tests and (b) it affects all the boundary layer measures you have discussed in the paper.

We now added: **Our calculations suggest, that ...**

Page 775, lines 16-18. This is an author talking to fluid dynamicists. Explain what the Froude number measures physically.

We now added: **that is the fluid velocity is higher than the speed of the gravity waves**

Secondly if the present model cannot reproduce such phenomena, what relevance is it to the real ocean?

Yes, the shallow water model is based on the hydrostatic approximation, which is no valid for small scale processes (smaller than a few km)

Thirdly isn't the use of a 'constant depth model' equally artificial and irrelevant to the real ocean?

Yes, it is even more irrelevant as it does not fully include vortex stretching.

Tables

Table 1. This needs a more detailed explanation of the terms so that it can be understood independently of a detailed reading of the text.

Done!

Table 2. This needs an explanation of the term 'scaling exponents' - maybe with reference to an equation in the main text.

The scaling exponent is nothing other than the slope, so we replace scaling exponents in the caption text by:

Slopes of ...

Figures

These appear to be high resolution bitmap images. It is fine for the 2-D plots but the associated text looks unprofessional. You would do much better to output the plots as simple postscript files and then use programs like Adobe Illustrator to clean up the layout and text and generate the pdf files. Line plots would be clearer and need less bandwidth if simple postscript was used to create the pdf files (which are then essentially the postscript plus a few extra commands).

Figure 1. Clean up text in the figure. Replace eta in the figure by 'sea surface elevation'. To be clear you could use 'sea surface elevation (eta)' in the caption. The figure should explain what TW1000 and MW1000 mean.

We now superimposed the instantaneous velocity fields.

Figure 2. Almost unreadable. The figure needs to concentrate on the western boundary region and use colours which cannot be confused. On my printer the black and dark blue are almost the same. The axes text needs to be larger and the latitudes should be included within each sub-panel. The highly distorted inner box looks a mess.

Done!

Figure 3. A good figure but the order 180, 195, 200 - differs between the figures and the caption. The axis text needs improvement. Explain in the caption that MW is the lowest viscosity run with Monsoon winds parallel to the coast.

Done!

Figure 4. Text needs to be clearer and smarter. Caption needs forcing and viscosity spelt

out. Reference equation in which Taylor scale is defined.

Done!

Figure 5. Replace 'small scale' (often used together as two adjectives) by something less confusing. Caption needs to be made more self contained, for example reference equations in which scales are defined. All the text is bad but the interior box is distorted and essentially unreadable.

Done!

Figure 6. Make scales more readable. Inner box is again essentially unreadable.

Figure 7. Main problem is that all the important information is squashed near the y-axis. It would be better to expand this region as in Fig 6. Axis text hard to read, inner box hard to read and badly distorted.

Done! We also added a panel at $y=+1500\text{km}$ for the MW experiment.

Figure 8. Too small. Unreadable.

Done!

Figure 9. At least this is readable but why is the y-axis text expanded and the x-axis compressed?

We now corrected it.

Answer to Reviewer # 2

This manuscript presents a set of idealized studies of eddying western boundary currents. The context is the tropics, so wind forcing corresponding to trade winds and monsoon winds are considered. The western boundary currents become unstable below a certain value of the viscosity and become fully turbulent at an even smaller value. In the chaotic state, there is a substantial region where eddy vorticity transport becomes important. The authors call this region the "extended boundary layer" and present some results which demonstrates the scaling of the extended boundary layer with viscosity. A pragmatic means for estimating the eddy viscosity in the extended boundary layer is proposed. An interesting feature of this study is that the grid scale is kept fixed at a value appropriate to the lowest viscosity simulation, so changes in Reynolds number can be studied separately from changes in grid resolution. This is a logical and straightforward approach, but one which is rarely taken in ocean circulation simulations.

I agree with the other reviewer that there is material in this manuscript which is interesting and potentially publishable, but the writing is poor, the presentation confusing, and the figures poorly formatted. The manuscript could definitely use the attention of a careful copy editor or proofreader who is fluent in English. The problems with the figures are adequately addressed by the other reviewer; I will not repeat them here. My remaining issues are addressed point-by-point below:

- General comment: For ease of comparison with other studies, it would be better to present the results in terms of a Reynolds number rather than (or in addition to) the viscosity. The manuscript makes references to the Reynolds number, but the particular Reynolds number the authors have in mind is never defined and numerical values are not given.

We defined and gave the Reynold number at the beginning of the manuscript (Page 759, line 12) for the laminar solution as $Re = v_0 \delta_M / \nu$. In the lower viscosity Monsoon Wind forcing experiments the boundary-layer is disintegrated by the turbulent dynamics at all the latitudes. In the lower viscosity Trade Wind forcing experiments, the boundary-layer is a combination of turbulent dynamics at higher latitudes and laminar dynamics at lower latitudes due to the inertial effects. In these cases it is difficult to determine a characteristic length and velocity scale to calculate the Reynold number as the western boundary current is a turbulent-layer or a combination of a turbulent/inertial layer. As a length scale we could choose, the Munk-scale, the inertial scale or the Taylor scale. There are thus several definitions of a Reynolds number possible. Especially in the Trade-Wind forcing, the Reynold number is also dependent on the latitude. Furthermore, the viscosity coefficient ν is a model parameter, it is fixed before the experiment starts, whereas the Reynolds number is result of the calculations it is, to our opinion more appropriate to distinguish the experiments. The reviewer is right when he says that the evolution of a Reynolds number with viscosity should be given. We now added in section "3 Experiments":

The transport in the boundary layer is, to leading order, imposed by the wind forcing over the entire basin, which does not vary within the Monsoon-wind and Trade-wind cases.

This leads to the fact, that velocity times the boundary layer thickness is constant and the Reynolds number scales as the inverse of the viscosity.

1. Section 2.1, The physical problem: The problem is posed on an equatorial beta- plane with the tropics off-center. While tropical circulations are given as the motivation for the study, it's not clear that placing the domain in the tropics makes the problem significantly different than in midlatitudes. The authors state that the vortex stretching term is negligible throughout most of the domain, which makes the reduced gravity system essentially equivalent to the barotropic quasigeostrophic equations. While the geostrophic velocities obviously change sign across the equator, the (barotropic) QG vorticity balance doesn't really care about the equator. The authors should explain, at least qualitatively, how their reduced gravity results are different from results obtained using QG and how their results would different if the model was formulated at midlatitudes.

We agree with the reviewer. It is a result of our experiments, that vortex stretching is not dominant and that the equator, or the low-latitude setting does not play a dominant role within the boundary layer dynamics. It is, however, dominant for the interior dynamics especially for the response of the ocean to the Trade-wind forcing, leading to a strong inertial effect. So the forcing is typical for the low latitudes of the Indian and Atlantic low-latitude dynamics. A direct comparison to mid-latitude case is difficult, as the same forcing would result in different currents in the interior ocean and thus also to different boundary-layer structures. We had mentioned this in the first paragraph of the section "5 Discussion and conclusions". To make it more clear we now added at the end of this paragraph:

To leading order, the vanishing of the Coriolis parameter is important due to its determination of the dynamics in the interior ocean, which then governs the boundary-layer structure.

Please see also our answer to point 8 brought up by this reviewer. In figure 1 it is shown that the variations of the layer thickness are of order 1, which questions the applicability of a QG model. Furthermore QG models never outcrop (by definition) and they can produce results in regimes that do not really exist, where a shallow-water model would have outcropped. The thus obtained results are than purely mathematical but do not apply to the dynamics on our planet.

2. Section 2.1: Why is the domain off-center from the equator (i.e., with the equator at $y = 1000$ km rather than $y = 2000$ km). This introduces an asymmetry to the problem that deserves a comment.

The domain is off-center, as our research is directed towards studying the Somali and North Brazil currents, ranging within the most energetic structures in the world's ocean and both occurring north of the equator. We now added in section "2.1 The physical problem considered":

The domain extends further northward than southward, as our research is directed towards studying the Somali and North Brazil currents, ranging within the most energetic structures in the world's ocean and both occurring north of the equator.

3. Section 2.2: The specification of the forcing in (1) and (2) is bizarre. It would be more straightforward to remove C_1 and C_2 (which are not defined) and simply give two

formulas for the wind stress.

We now removed the coefficients C1 and C2 and give the forcing explicitly as suggested by the reviewers.

4. Section 2.3: Why does the trade wind forcing (6) decay exponentially across the domain? It's typical in these kind of idealized studies to have the zonal stress depend only on the meridional coordinate, so there must be some reason for the zonal decay, but there is none given. Would the results be significantly different if (6) were constant in x ?

In the Atlantic the Trade Winds decay towards the east. In our calculations the decay towards the east also prevents the vanishing of the surface layer (outcrop). For the boundary layer structure only the velocity profile in the western part of the ocean is important.

We now added in section “2.3 The winds-tress forcing”:

In the Atlantic the Trade Winds decay towards the east. In our calculations the decay towards the east also prevents the vanishing of the surface layer.

5. Section 2.4: The implementation of the model needs to be discussed in more detail and the results compared to a test case with an analytical solution or a known numerical solution. The model appears to be home-grown, so we have little reason to have faith in it a priori. This is especially true given the fact (as pointed out by the other reviewer) that the time-stepping scheme not only doesn't conserve energy, but actually amplifies the energy of the system. (I agree with the other reviewer that this is likely the source of the short time step requirement.)

Please see the answer for the reviewer # 1 above concerning this point.

6. Section 2.4: The reduced gravity equations allow the lower layer to outcrop by letting the thickness of the upper layer go to zero (in this case, this would mean $\eta = -H$). How are outcrops handled? If they are not handled, how are they prevented?

The physical parameters are such that in the present dynamics an outcrop does not occur. As stated above outcrop occurred at the eastern part of the basin, but this could be easily handled by reducing the wind forcing in the eastern part, which also corresponds to observations as stated above. When the layer-thickness vanishes (becomes negative), numerical, instability results and the model blows up. In previous research (Wirth, Willebrand & Schott 2002) we found the dynamics to be very dependent on how the model handles the outcropping. And outcropping can create small scale structures mimicking frontal instability. We thank the reviewer for this question because it is indeed important to note, that the present structures were not (artificially) by a numerical procedure to handle outcropping. We now added in section “2.4 The numerical implementation” :

The physical parameters are such that in the present dynamics a vanishing of the fluid layer (outcrop) does not occur.

7. Section 2.4: "We favor fine-resolution rather than high-order schemes?" Why is that? Is this even a dichotomy? Can't you have both high order and fine resolution?

High-order schemes are more computer time consuming than lower-order schemes. So if computer resources are fixed one has to look for a compromise. At the boundary the higher order schemes are usually degraded to lower order (or some analytical boundary layer model has to be conjectured). It is the dynamics at the boundary that is key in our results, so we wanted this dynamics to be sufficiently resolved, rather than rely

on a boundary layer model or an high-order-scheme. Second-order in-space-and-time is standard for ocean modelling. In all schemes, it is a short time-step and a fine spatial-resolution that increases the accuracy of the results, that is why we favor a short time-step and fine-resolution. At coarse resolution, high order schemes often perform less accurate than low order schemes.

8. Section 4.1/Figure 1: Showing the streamfunction instead of, or in addition to, the thickness would be more informative here. Since the solution is steady, the transport streamfunction is well defined.

In the first figure we wanted to show the dynamical variables of the model. One important information not visible in the transport stream function, is that the layer thickness is far from vanishing (see discussion above) but its variability is of the order of the layer thickness. In the transport stream function, the influence of the equator is less visible and it looks like a barotropic geostrophic equation. We now superimposed the velocity field on the layer thickness variation to have all the dynamical variables (u, v, η) visible. We hope to be able to convince the reviewer to stick to our initial choice to show the layer-thickness. Other choices are clearly possible and have other advantages.

9. Section 4.1/Figure 1: It appears that the northern and southern boundaries are playing a large role in setting the structure of the solution. This is troublesome, since these boundaries are artificial. Does moving the boundaries further away change the solution? If not, why not?

The reviewer is right, the northern and southern boundaries are playing a large role in setting the structure of the solution. Moving the boundaries further away means also a larger ocean and a larger area of forcing, especially in the Monsoon-wind case, leading to stronger currents. The meridional extension is roughly this of the Indian Ocean with the southern boundary corresponding to the South Equatorial current.

10. Section 4.2: What is $u_I(y)$ in the line following (9)?

As said above in the answer to reviewer # 1 we now added in the manuscript: $u_I(y)$ is the westward zonal velocity just outside the boundary layer, responsible for the inertial effect (see Pedlosky (1979, Sect. 5.6)).

11. Page 762, line 20: The equatorial Rossby radius is usually given as $\sqrt{\frac{\sqrt{g'H}}{2\beta}}$ (i.e., with a factor of 2 in the denominator under the radical). This would give a Rossby radius of 250 km rather than 350 km. Is this difference important?

It is defined without the factor 1/2 by the American Meteorological Society ([http : //glossary.ametsoc.org/wiki/Equatorial_kelvin_wave](http://glossary.ametsoc.org/wiki/Equatorial_kelvin_wave)). But this difference is not important we just use the fact, that it is order of.

12. Page 763, line 23: Should "a few tenths of kilometers" be "a few tens of kilometers"? Yes the reviewer is right it is "a few tens of kilometers", we now change it in the manuscript.

13. Page 764, line 16: What is special about the location $y = 1000$ km?

The location $y = 1000$ km is chosen to highlight the inertial effect just north of the equator in the TW experiments as there is no burst in this area (from slightly north of the equator up to roughly $y = 1300$ km).

14. Section 4.4: The two quantities introduced in this section (λ_1 and λ_2) would be unfamiliar to most oceanographers and require more explanation. It's not obvious that the Taylor scale is even relevant to 2D turbulence. In 3D turbulence, the Taylor scale characterizes the smallest scales in the inertial subrange, so it's not clear why the difference between the Taylor scale and λ_2 would give you an idea of the range of scales over which turbulence is active, even in 3D turbulence. Is the factor of mentioned in $\sqrt{5}$ in line 16 significant? Also, I had a hard time finding λ_2 in Bofetta and Ecke (2012). The authors might think about putting in a more accessible reference for λ_2 .

The smallest scale in the inertial subrange is characterized by the Kolmogorov scale η and not the Taylor scale λ_2 . The Taylor scale in 3D turbulence is well within the inertial range. In fact, $\lambda_1/\eta \sim \sqrt{Re}$. We choose the review article of Bofetta and Ecke (2012), we do not know where it is used elsewhere. The idea use to calculate the two quantities is simple. As an example let us consider the velocity field $u(y) = A \sin(y/L + \omega t)$ which has the vorticity $\zeta(y) = A \cos(y/L + \omega t)/L$. Using the formula

$$\sqrt{\frac{\langle u^2 \rangle}{\langle \zeta^2 \rangle}}, \quad (1)$$

gives L , the scale of the velocity field. The two quantities are based on this formulation so the factor $\sqrt{5}$ is not significant. Please see also the answer to this section of the reviewer # 1

15. Page 767, line 1-2, Figure 8: There only appear to be two values of the viscosity for which the extended boundary layer width is calculated. A scaling law based on only two points is not very convincing.

Yes, there are only two values of the viscosity but measurements are taken at different locations. We used a very careful formulation (“Supposing a scaling behaviour”, “suggests”) in the text which, as we think reflects our caution of the obtained result: “Supposing a scaling behavior for the extension of the extended boundary layer with viscosity in the MW forcing experiments suggests an exponent close to $-2/3$ [...]”

16. Page 767, lines 23-27: Couldn't the appearance of $\langle u'^3 \rangle$ be explained by the disc model if, instead of being transported in the y-direction, the disc was moving at some angle to the y-axis?

The eddies move principally along the Y axis at the location where the averages were taken. It is clear that strong burst will lead to a positive third order moment as it is seen in the convective planetary boundary layer. We now changed the paragraph to:

The positive value of $\langle u'^3 \rangle$, however, can not be explained by the model of a disc moving in the meridional direction at constant speed, which leads to a vanishing third order moment. It is most likely a signature of the bursts and dipoles, with more intense and localized transport away from the boundary than the recirculation towards the boundary. This agrees with the findings of anisotropic bursts and dipoles dynamics in subsection 4.3.

17. Page 768, lines 27-28: Consider using different subscripts for the inertial and viscous boundary layer thickness. ν and V are very hard to distinguish in the font the

manuscript is typeset in.

We changed “V” to “A” (for advective).

18. Page 769, line 29: What is meant by “inverse inertial”?

We now changed the sentence to:

At higher latitudes the scaling is higher for the TW forcing, showing that the boundary layer thickness decreases even faster with decreasing viscosity, when “inverse inertial” effects are present, that is when the zonal velocity at the outer boundary of the boundary-layer is positive.

19. Page 773, line 14: Is the fact that the western boundary currents are at low latitude significant to this statement? Are midlatitude western boundary currents less turbulent?

We agree with the reviewer, that the qualitative aspects of our findings are not restricted to low latitude western boundary currents, but we have not shown it.

20. Figure 8: The slopes corresponding to the scaling laws in table 2 should be indicated on this figure.

The figure is also extremely dense, presenting almost all the results of the paper. Adding lines makes them unreadable, that is why we added the table.

We like to thank both reviewers again and hope we have replied to their questions.

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