ANSWER TO REVIEWERS

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We are grateful to the reviewers for their thorough review which has helped to considerably improve the manuscript. We are also glad that our research is appreciated by both reviewers.

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

This is an interesting paper that reports on the small scale turbulence that develops in an idealised western boundary current when the viscosity is low. I have a number of comments, given below, but after these have been addressed the paper should be suitable for publication. Overall the paper is well written and easy to understand. However some copy-editing would be helpful, especially of the Introduction.
Comments on the Text:
1. Equation 8.
The paper needs to explain why this form of the wind stress forcing was used. For example why have a decay with distance from the coast? Why use the factor '4' instead of halving the value of 'L_x'. Why use the factor '-0.2'.

The form of the wind stress forcing is

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

with τ_o the wind strength, the offshore and the alongshore structures, $X(x)$ and $Y(y)$. This form of the wind stress forcing was already used by McCreary and Kundu (1988) in the case of an investigation of the Somali Current. In their paper they studied the response of the ocean to the offshore and alongshore wind forcing. The form of the wind stress forcing was based on this study (please see McCreary and Kundu (1988) for more detailed explanation). Several test experiments were run to have a realistic velocity in the WBC. We choose the factor '4' and '-0.2' to have a strong alongshore wind-stress at the western boundary and an inversion of the wind-stress at about 3500km from the shore, which corresponds roughly to the Indian Ocean during summer Monsoon wind forcing. These two factors lead to an idealized WBC that compares well to the North Somali current during the summer monsoon.
We now added in the manuscript:
The form of the wind-stress is chosen to have a strong alongshore wind-stress at the western boundary (please see McCreary and Kundu (1988) for more detailed explanation) and an inversion of the wind-stress at about 3500km from the western boundary. It corresponds roughly to the Indian Ocean during summer Monsoon wind forcing.
What was the value of t_c and why was that value chosen.
To avoid the initial shock, the wind stress increases from zero as $s(t) = 1 - \exp(\frac{-t}{t_c})$ with spin-up time t_c=180days.
This is now mentioned in the manuscript:
To avoid the initial shock, the wind stress increases exponentially from zero with spin-up time of t_c=180days.
Later the discussion needs a paragraph on whether this particular form of the forcing may have affected any of the conclusions of the paper.
We now added in the manuscript:
This wind-forcing not only leads to a single gyre extending over the entire domain, but also to an almost vanishing zonal velocities. The zonal velocities lead to an inertial boundary

current (Charney (1955), see also Pedlosky (1979)) and have a stabilizing (when westward) or destabilizing (when eastward) effect on the western boundary current. This behavior is subject of a future publication.

2. Page 2467, line 25.

"... asks for a short time step.". Was the short time step just a guess (as the odd phrase implies)? If not how was it chosen? For example were tests made with shorter time-steps to check to solution had converged?

We use fine resolution in space and time to take advantage of the second-order convergence. We also wanted to make sure, to accurately represent the non-linear structures (bursts) and their evolution, tests with a time step of 60s showed the same structures and chaotic behavior. In the CFD research on turbulent boundary-layers usually O(10) grid points are used to resolve a structure and Courant numbers (CFL) of order 0.3 are often used. We emphasize, that such structures we observed and their behavior appear in observations and fine-resolution numerical experiments.

The sentence is now changed to:

To insure a correct representation of this intermittent, rapid and violent process and its nonlinear evolution, a short time-step was used.

3. Page 2468, line 7.

"The highest viscosity experiments ... converged towards a laminar dynamics."

a) This implies more than one experiment was carried out. Explain.

We didn't mention it in the manuscript but experiments with viscosity ν=2000,4000 were carried out. Their solutions show qualitatively the same laminar dynamics for all $\nu > 1000\text{m}^2\text{s}^{-1}$ (with increasing viscosity the western boundary layer dynamics converges towards the corresponding analytic Munk-layer solution).

b) Do you mean "converged with time towards"?

Yes (so it also converges to the corresponding analytic Munk-layer solution with increasing viscosity), we now added in the manuscript:

with time

c) How did you convince yourself that the solution had converged?

The EXP1000 is laminar during the integration and no significant time evolution is observed in the second half of the 5000days integration. All the turbulent experiments reached a statistically stationary state after 2000days. To checked the convergence, we done sub-sampling, that is, we considered average over 2000-3000days, 3000-4000days and 4000-5000days. We didn't see a significant difference. To increase the statistical significance of the statistics, we chose the averaged over 3000-5000days for the results presented.

d) What do you mean by "maximal average"? e) Average over space, latitude or time?

Average over time of the meridional velocity, we wanted to say maximal value v_0 of the time-averaged meridional velocity.

We now corrected it in the manuscript:

... maximal value v_0 of the time-averaged meridional velocity in the boundary current ...

f) How did you estimate the Munk-layer thickness.

The Munk-layer thickness is estimated by the characteristic boundary layer thickness $\delta_M = (\nu/\beta)^{1/3}$ where ν and β are model parameters and thus obtained before running the

experiments.
We now wrote in the manuscript:
... and the Munk-layer thickness $\delta_M = (\nu/\beta)^{1/3}$ (where ν and β are model parameters and thus obtained before running EXP1000) at $y = +1500$km is ...
4. Page 2468, line 18.
"Lower viscosity experiments converge to a statistically stationary state".
Also in 3000 days?
Yes, we checked it. It is always the larger structures that take longest to adjust. Smaller structures have shorter characteristic time scales. This is also the case for small scale boundary layer turbulence. The time-scale of 3000 days is very conservative, for a rather small basin at low latitude.
5. Page 2469, line 22. " It decreases rapidly with distance from the boundary."
a) Give the distance over which vortex stretching was important.
At about $\sim$20km.
We now added it in the manuscript:
It decreases rapidly with the distance from the boundary ($\sim$20km, not shown), before the meridional velocity reaches its maximum.
6. Page 2469, line 25
"The meridional velocity is close to the Munk-layer solution"
Give some numbers, i.e. at least one comparison.
We now added in the manuscript:
The analytic solution of the Munk theory, for $\nu = 1000\text{m}^2\text{s}^{-1}$ vanishes at $\frac{2\pi}{\sqrt{3}}\delta_M$=133km. The laminar experiment EXP1000 has a vanishing meridional velocity (width of the boundary current) of around 150km (not shown).
7. Page 2470, line 26.
"..., with an almost motionless core."
Make it clearer that this refers to what you describe as rings and not eddies.
Yes, the formulation was ambiguous, we now changed it to:
In the literature, eddy or ring are often used interchangeably to denote the same object. A closer inspection (not shown) of the velocity field shows that they are eddies in almost perfect solid-body rotation. They are not vortex rings with an almost motionless core (eye).
8. Page 2472, line 7.
"... the dynamics in the viscous sub-layer is not laminar".
You need to explain what property indicates that it is not laminar.
We now added in the manuscript:
Please note that the dynamics in the viscous sub-layer is not laminar, as bursts, that is, intermittent and violent detachments of the viscous sub-layer occur intermittently. This feature ...
9. Page 2474, line 20.
Presumably the size of the Extended Boundary Layer increases during the first part of each run. Does it reach a fixed limit or is it still increasing with time at the end of the experiments?

The zonal extend of the extended boundary layer increases during the first part of each run but then stabilizes, this happens way before the period used to analyze the data (from 3000 to 5000days).
We now added in the manuscript:
The zonal extend of the extended boundary layer increases during the first part of each experiment but then stabilizes.
10. Page 2476, lines 1-25
Figure 5 implies that the advection term (TRVA) is transporting vorticity into the boundary layer where it is lost to friction. If this transport down gradient - from a region of strong fluctuations to a quieter region? If so is there also a transport off-shore?
The transport is varying between down-gradient and up-gradient. We tried many approaches to find a parameterization based on values and gradients of various quantities (vorticity, tke, ..) but did not succeed.
11. page 2480, line 17 to 20.
An important point made here is that viscosity values like 1000 (as used in EXP1000) give unrealistic boundary currents. However it reads as if the paper is reporting a conclusion reached earlier when in fact this is the first time the problem has been discussed.
One possible way of dealing with this is to include some lines near the end of section 4.6 which points out that EXP1000 is actually unrealistic.
What we want to say, is that this is a direct consequence of the above results. Including some lines in 4.6 means discussing twice the same thing.
We now added in the manuscript:
A consequence of the results presented above is that, choosing ...
Comments on Tables and Figures:
Figure 1.
Nice figure, and on a screen I can zoom in to see the details, but on a printed page it will not show much of the vectors. I suggest that you add an insert with an expanded view of part of the boundary current.
The purpose of this figure is to introduce the domain and the basic features of the circulation, that is a strong current along the entire western boundary and weak recirculation in the reminder of the domain, as predicted by ocean circulation theories. The structure is close to the Munk boundary-layer which is given in the formula 9. In a previous version of the present manuscript a detailed comparison of the structure at different latitudes to the Munk layer solution was given.
Figure 5
On the printed page, the colours (especially black, grey and dark blue) are difficult to distinguish.
We now changed the colors.
Figure 7
Caption indicates that the symbols refer to three different latitudes, whereas the figure shows 5, 5, 4 and 4 different values for the four experiments.
Now corrected.

Answer to Reviewer # 2

I reviewed a previous version of this manuscript that was ultimately rejected. This version is much more focused and readable. The English has improved and some of the more confusing aspects of the original manuscript have been removed or clarified. I think the present manuscript presents some interesting results and makes a few good points and could be published after a bit of revision. Major comments:
1. Time step: The reviewers of the original manuscript questioned the short time step used here (90 s—five times shorter than that required by the CFL condition) and this issue has has never been fully resolved. The authors state that the "intermittent, rapid and violent" bursts "asks for a short time-step," but it's not clear from the manuscript why this is so. It seems unlikely that the bursts could violate the advective CFL condition, as this would require unphysically large velocities (on the order of 14 m/s). In the responses to the previous set of reviews, the authors state that they used such a short time step because they wanted the flow to be well resolved in time as well as space; they should add a comment to this effect in the present manuscript.

This is now done, please see answer to referee 1 (his point 2).

2. Scales of motion: The previous reviewers had issues with the definitions of the length scales used in section 4.4 and the present manuscript does not resolve them completely. One issue is that the scales appear to be defined using the full (i.e., fluctuating + mean) fields, rather than the fluctuating fields. For example, the kinetic energy used in defining λ_1 appears to be the total kinetic energy rather than the turbulent kinetic energy. This seems strange considering that these scales are supposed to characterize the scales of the turbulence, not the mean flow. Defining the scales in terms of the fluctuating quantities would alleviate the problem noted below equation (13): "the above scales are not useful for analyzing time-independent flow." If they were defined in terms of fluctuating quantities, the scales would simply become undetermined for steady flow rather than oscillating between zero and infinity as do the solutions in eq (13).
Yes, we have thought about filtering out the time mean signal. Filtering out the time mean is clearly a valid option. There are several reasons why we did not do so: (1) Filtering does not change the message, that the size of the structures decreases faster than predicted by laminar Munk theory, when viscosity is reduced. Indeed, it is the stationary signal that becomes unstable and the first coherent structures arising are of the order of the stationary signal, in size and magnitude. To convince the reader, it is always good to "filter" as less as possible to make ones point. (2) For numerical simulations the resolution of the total signal is important. (3) In the classical (3D) boundary layer the turbulent signal is rather small. In two dimensional turbulence the turbulent signal is often stronger than the mean flow (which makes 2D-turbulence so important and so beautiful to look at). By taking the full signal we want to pass the message, that what we analyze is really important and not a negligible fluctuation around a dominant mean state. In the case where a succession of eddies moves along the boundary, the time-mean signal is produced by the eddies moving coherently along the boundary and the size of the mean is the size

of the eddies. (4) The viscous dissipation of energy and enstrophy (quadratic quantities) acts on the total signal. (5) taking only the fluctuating part does not alleviate the problem when the variability is almost zero, as its calculation will be the quotient of two numbers which are almost zero. The above arguments inclined us to take the total signal rather than only the fluctuating part. We acknowledge that the choice proposed by the referee is also valid.

Minor comments:

1. Page 2462, Line 2: "is" should be "are".

Done!

2. Page 2462, Line 6: insert "the" between "in" and "form".

Done!

3. Page 2463, Line 9: delete "While".

Done!

4. Page 2463, Line 18: Replace "permit to represent the" with "permit representation of".

Done!

5. Page 2463, Line 23: Replace "were performed" with "have been performed previously".

Done!

6. Page 2464, Lines 13 & 14: Insert commas after "Indeed" and "knowledge". Remove comma after "current".

Done!

7. Page 2465, Line 20: Replace "These numbers" with "The values of these parameters".

Done!

8. Page 2466, Line 15: Replace "It" with "This".

Done!

9. Page 2468, Line 14: What is meant here by "scheme"?

It means numerical scheme, we now replace it in the manuscript by "the numerical scheme".

10. Page 2469, Line 3: Insert a comma after "experiments".

Done!

11. Page 2469, Line 9: Replace "spacial" with "spatial".

Done!

12. Page 2470, Line 23: Remove comma following "demonstrates".

Done!

13. Page 2470, Line 26: The comment "One has to mention that in the literature eddy or ring are often used interchangeably to denote the same object." can be replaced with the parenthetical comment "(In the literature, eddy and ring are often used inter- changeably.)"

Done!

14. Page 2471, Line 25: "strongly spatially localized" rather than "strong spatially localized".

Done!

15. Page 2472, Line 2: "dynamics" rather than "dynamic".

Done!
16. Page 2472, Line 7: Be more specific about what is meant by "the dynamics in the viscous sub-layer is not laminar".
Done! We now added in the manuscript:
Please note that the dynamics in the viscous sub-layer is not laminar, as bursts are intermittent and violent detachments of the viscous sub-layer. This feature ...
17. Page 2472, Lines 10-15: The fact that T1 and T2 are numerically similar does not indicate that "there is only a feeble dependence on latitude," as this agreement could simply be a consequence of the mean value theorem for integrals. If T(y) is the fraction of bursts as a function of latitude, then T1 = T(1000 km) and T2 is the average of T(y) over a range of latitudes containing y = 1000 km. The mean value theorem states that the mean value of T(y) over an interval is achieved as the point value of T(y) for some point in the interval. Thus, the agreement between T1 and T2 could simply be due to a fortuitous choice of the latitude used to evaluate T1. It would be more convincing to show a plot of T(y) as a function of latitude, or at least state that T(y) is nearly constant.
Yes, the reviewer is right comparing T1 and T2 can only be seen as a hint that there is no or only week dependence. We checked that there is no significant dependence on latitude within the range considered. We thus decided to take away the T1 value and use only the more significant (more data) T2 value named T.
We changed it in the manuscript:
To quantify the occurrence of bursts, the fraction along the western boundary in the interval $y \in [+125, +2250\text{km}]$ at which a flow reversal occurs is calculated and then average over time, to obtain the value T presented in Tab.2. For viscosities $\nu = 1000\text{m}^2\text{s}^{-1}$ or larger there are no bursts. Bursts are observed for $\nu = 500\text{m}^2\text{s}^{-1}$ and lower. The fraction of time with flow reversal strictly increases with decreasing viscosity in all the experiments performed and reaches values of around 19% for the lowest values of the viscosity, showing that they are a recurrent dominant feature of low viscosity boundary currents when inertial effects are absent.
18. Page 2473, Line 6: Delete "Using".
Done!
19. Page 2473, Line 8: Delete "which".
Done!
20. Page 2473, Line 16: Do not indent this line.
Done!
21. Page 2473, Line 18: Start a new paragraph after "gradients."
Done!
22. Page 2473, Line 21: Do not indent.
Done!
23. Page 2474, Line 9: Replace "when" with "through".
Done!
24. Page 2474, Line 19: Remove comma after "plateau".
Done!
25. Page 2475, Line 21: Replace "state a time" with "state, the time".

Done!

26. Page 2476, Line 13: The term "advective boundary layer" might confuse the reader into thinking you're talking about an inertial boundary layer. A better term might be "turbulent boundary layer."

The reviewer is right, that there might be a danger of confusing it with the inertial boundary layer, which does not exist in our set-up (the forcing is choose to have only negligible zonal velocities on average). The advective boundary layer is the area where the (turbulent) northward advection of PV (in the time-mean) is significant. Turbulence extends throughout the "extended boundary layer". So calling it "turbulent boundary layer" might lead to confusion with the "extended boundary layer".

27. Page 2478, last line through beginning of next page: It would be easier to follow if the description started at high viscosity and proceeded downwards to lower viscosity; otherwise the comment "once the viscosity drops below values ..." is confusing.

28. Table 1: "extent" rather than "extend".

Done!

29. Figures 3 & 4: Since λ_1 diverges over a large fraction of the domain, it would be clearer to plot the inverse of λ_1. It might make more sense to frame the entire discussion of length scales in terms of their inverses (i.e., wavenumbers) since turbulence is often discuss in wavenumber space.

Yes, turbulent spectra are discussed in k-space. We do not show spectra, which are dominated by the strong gradient at the boundary, hiding the near boundary behavior. The length scales obtained from λ_1 and λ_2 correspond to coherent structures of which the extension is known in physical space. Furthermore we argue also about the numerical resolution needed to resolve the structures, this is again a quantity given in physical space. In the part where the values diverge no turbulence is present. A large part of the ocean community is not familiar with spatial spectral representations and our paper can be understood without this knowledge and thus attain a larger community. We would therefore suggest to keep the scale rather than the wave-number representation, which is more straightforward to understand.

30. Figure 5: Consider changing the color scheme of the lines; they are currently difficult to distinguish.

We now changed the color.

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

Other Modifications

We recently became aware of recent publication (Spall (2014)) looking at western boundary current from different angle and showing the same coherent structures. This paper is now cited in the discussion and conclusion section:

Large anticyclonic eddies creating bursts and a strong dipole are also clearly visible in numerical simulation of Spall (2014) (his Fig.9).

References

Charney, J.: The Gulf stream as an inertial boundary layer, Proc Natl Acad Sci USA, 41, 731–740, 1955.

McCreary, J. P. and Kundu, P. K.: A numerical investigation of the Somali Current during the Southwest Monsoon, Journal of marine research, 46, 25–58, 1988.

Pedlosky, J.: Geophysical fluid dynamics, Springer Verlag, New York, URL `http://opac.inria.fr/record=b1085573`, includes index, 1979.

Spall, M. A.: Some influences of remote topography on western boundary currents, Journal of Marine Research, 72, 73–94, 2014.