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Interactive comment on "Dynamics of turbulent western boundary currents at low latitude in a shallow water model" *by* C. Q. C. Akuetevi and A. Wirth

Anonymous Referee #2

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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.

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.

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.

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.

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?

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.)

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?

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?

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.

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

the these boundaries are artificial. Does moving the boundaries further way change the solution? If not, why not?

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

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?

12. Page 763, line 23: Should "a few tenths of kilometers" be "a few tens of kilometers"?

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

14. Section 4.4: The two quantities introduced in this section (lambda_1 and lambda_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 lambda_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 lambda_2 in Bofetta and Ecke (2012). The authors might think about putting in a more accessible reference for lambda_2.

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.

16. Page 767, lines 23-27: Couldn't the appearance of <u'ˆ3> 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?

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

manuscript is typeset in.

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

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?

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

Interactive comment on Ocean Sci. Discuss., 11, 753, 2014.