Ocean Sci. Discuss., 11, C109–C121, 2014
www.ocean-sci-discuss.net/11/C109/2014/

Ocean Science
Discussions
Open Access

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

D.J. Webb (Referee)

djw@soton.ac.uk

Received and published: 22 March 2014

I found this a frustrating paper to review. On the one hand it contains some interesting scientific results which should be published. On the other hand it leaves out some key information and is difficult to read. Part of this is due to language but partly it is because the authors are applying ideas from traditional fluid dynamics to the ocean. This is well worth doing but the authors needs to remember that the average reader may know less about the engineering aspects and will almost certainly know more about the actual ocean (and possibly theories of ocean circulation).

In summary the paper should be published but, before then, it needs a major revision

and the services of a good copy editor.

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.

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

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

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

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.

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.

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.

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.

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?

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.

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.

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.

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 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.

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.

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.

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.

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.

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.

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.

Coherent structures

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

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

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

Bursts

Page 763, line 26. "stimulations 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.

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

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 everywhere.

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.

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?

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.

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.

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.

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?

As turbulence generally spreads out it would be much more sensible to consider averages of the measures over some sensible area around each point.

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

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.

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.

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.

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 *!VA 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.

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.

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.

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.

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

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.

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 stabilise 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?

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.

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?

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.

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.

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.

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

Page 773, line 27. Are you saying that the resolution of your model results is insufficient?

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

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.

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

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

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?

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

viscosity to some velocity.

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

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.

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

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

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

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.

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

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.

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.

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.

Figure 4. Text needs to be clearer and smarter. Caption needs forcing and viscosity spelt out. Reference equation in which Taylor scale is defined.

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.

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.

Figure 8. Too small. Unreadable.

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

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