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Interactive comment on "On the role of domain aspect ratio in the westward intensification of wind-driven surface ocean circulation" *by* Kaushal Gianchandani et al.

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We thank the referee for helping us improve the quality of our paper. However, the referee's (single) major comment is completely irrelevant to our analysis of Stommel's model. Furthermore, the recommendation to reject our manuscript completely ignores our (unchallenged) analysis of Munk's model.

While it was an enjoyable exercise to revisit the general solutions of these classical models, I'm afraid that I am unable to recommend the manuscript for publication as I believe the results are misleading, at least in the parts of

parameter space of most relevance to the ocean (and it is debatable that these models are of any quantitative relevance beyond their substantial conceptual value).

We believe that the values we selected for the non- dimensional parameters in Stommel's and Munk's model fall within the relevant ranges of values of the dimensional parameters (see our detailed response to minor comment #6). We agree that our paper has two foci - one "conceptual" and the other "quantitative" (i.e. related to the world ocean).

Major comment:

The solution (3) to the Stommel model is indeed the most general, but this form rather obscures the essential physics in the physically-relevant limit of small α (i.e. the boundary current width is much smaller than the basin width). The authors erroneously state that in this limit, the solution becomes linear in x and can satisfy just one boundary condition. However, a more careful expansion of the exponential terms leads to a more complete solution.

I prefer to see this by assuming α is small and hence $\frac{\partial^2}{\partial x^2} \gg \frac{\partial^2}{\partial y^2}$. Thus (1) is well approximated by:

$$\alpha\frac{\partial^2\psi}{\partial x^2} + \frac{\partial\psi}{\partial x} \approx \sin(\pi y)$$

the solution to which is

$$\psi \approx (x - 1 + e^{-\alpha x})\sin(\pi y)$$

This solution consists of the Sverdrup (1947) solution in the basin interior – the first two terms in brackets on the right-hand side – and a Stommel (1948) western boundary current correction – the third tern in brackets on the right-hand side.

Mathematically, the dropping of the $\alpha\frac{\partial^2}{\partial x^2}$ term in (1) means that the particular integral is instead formed by balancing $\partial\psi/\partial x$ against the wind stress curl on the righthand side. However, the same result can be obtained through a careful treatment of the limit of small α in the two exponential terms in the more general solution.

The implications are that the western boundary current transport is:

- **approximately equal (and opposite) to the Sverdrup gyre transport**
- **independent of the linear drag coefficient, α;**
- **independent of the basin aspect ratio, δ.**

These conclusions are at odds with those stated in the manuscript. I do accept that in the case that α becomes larger, the boundary current transport, and indeed the entire nature of the solution changes, but it is hard to see what relevance this has to a large-scale ocean basin.

Response: The assumption, $\frac{\partial^2}{\partial x^2} \gg \frac{\partial^2}{\partial y^2}$ that underlies the referee's comment, implies that the general solution of the associated homogeneous equation is y-independent i.e. the y-dependence of the solution is identical to that of the inhomogeneous forcing term. In our model, this assumption translates to $\frac{1}{\delta^2} = 0$ in the Laplacian $\nabla^2 = \delta^2\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2}$ [given by (2) in the manuscript]. While this y-independent limit can yield a fast-flowing WBC in Stommel's model, it completely undermines the role of basin's aspect ratio (δ) in determining the transport of the WBC, which is the main sermon of our paper (as is evident from the paper's title). For large scale circulation, typical values of $\delta < 0.5$ yield $\frac{1}{\delta^2} > 1$ i.e. setting $\frac{1}{\delta^2} = 0$ is inconsistent with the intended "quantitative" applications. The alternative is to employ the general solution

of equation (1) without assuming y-independence of the Laplacian, which is precisely what we did in our paper.

Moreover, Bye and Veronis (1979) succinctly established the $\delta-$dependence of the Sverdrup transport. In terms of our scaling this correction can be derived by noting that since $\alpha = \epsilon/\delta^2$, equation (1) of the manuscript implies:

$$\left(\epsilon\frac{\partial^2}{\partial x^2} + \frac{\epsilon}{\delta^2}\frac{\partial^2}{\partial y^2}\right)\psi + \frac{\partial\psi}{\partial x} = \sin(\pi y)$$

where $\epsilon = (r/\beta L_x)$ is a proxy of damping and the non-dimensional width of the WBC. Under the assumption of small damping i.e. $\epsilon \ll 1$, the term $\frac{\epsilon}{\delta^2}\frac{\partial^2\psi}{\partial y^2}$, cannot be neglected in the interior solution when $\delta^2 \sim O(\epsilon)$ i.e. the Sverdrup balance becomes a function of δ in this case. The latter two bullet points that the referee makes lead to the unacceptable result that the strength of the WBC (that is equal in magnitude to the δ-dependent Sverdrup transport) is not determined by either of the model parameters α (or ϵ) and δ!

To appreciate this subtle issue one should compare a square basin, where $\delta = 1$, with a "narrow and long channel-like" basin where $\delta \ll 1$. In a square basin, the classical approach of equating $\partial\psi/\partial x$ to $\sin(\pi y)$ works well since the North-South gradient of the zonal velocity (represented by $\partial^2\psi/\partial y^2$) is small and can be neglected from the interior solution. However, in a "channel-like" ocean this quantity is large and cannot be neglected from the balance of terms in the interior solution. Surely, an examination of the 3 vorticity terms in the interior ($\partial\psi/\partial x$, wind-stress and $\partial^2\psi/\partial y^2$) clarifies that the WBC in the "channel-like" ocean should be weaker compared to a square ocean. Clearly, in the referee's approach there is no difference between the two oceans.

Here, we take the opportunity to thank the referee for his comment. To reconcile our approach with the existing literature, we will re-write the paper with ϵ as the parameter for damping (instead of α). We will also include the aforementioned comparison between a square and "channel-like" basin to further emphasize the conceptual aspect of our study.

Minor comments:

1. I don't understand why the authors estimate the boundary current transport rather than simply calculate the maximum value of the streamfunction which gives the actual western boundary transports. If I follow correctly, the authors also invoke the Stommel scaling for the boundary layer width, but that only holds in the low α limit

Response: The reviewer is right in that a simpler definition could have been used to estimate the transport. However, the definition based on the maximum point of the streamfunction gives a width of about 500 km in 10,000 km basin while our definition gives a width of about 100 km in the same basin. Moreover, using Stommel's scaling to obtain our ad-hoc definition of the boundary layer width [i.e. $\epsilon = r/(\beta L_x)$] makes the paper more accessible to oceanographers that are inclined towards observations or numerical modeling.

The expression for transport [given by (4) in the manuscript] is valid for the referee's definition of boundary layer width as well and the choice of the WBC's width does not alter the conclusions presented in our paper. We thank the referee for bringing this to our attention. In the revised manuscript, we will further emphasize that the results are not sensitive to the choice of WBC's width.

2. It might be helpful to many readers to state the original equations, before nondimensionalising.

Response: We will accept an editorial decision on this matter but since both forms of the vorticity equation – dimensional and non-dimensional – appear in so many textbooks and research papers we thought that presenting both versions is

redundant.

3. γ is the non-dimensional magnitude of the wind stress curl, not the wind stress

Response: We thank the referee for pointing this out. We will correct this in the revised manuscript.

4. In figure 1(d), why is the eastern boundary condition not satisfied?

Response: Figure 1(d), depicts the analytically obtained, non-dimensional streamfunction for Munk's model. In Munk's model the stream function, given by (7), is a function of $|\alpha| = \mu \frac{L_x}{\beta L_y^4}$ and does not vanish identically even for small $|\alpha|$ at either boundary (although the values of the streamfunction at the boundaries are rather small). For large values of $|\alpha|$, the value of the streamfunction at the boundary is no longer close to 0 (as it is for smaller $|\alpha|$) and we see the less than optimal behaviour as depicted in Figure 1(d).

We thank the referee for his input and will emphasize this further in the revised manuscript.

5. I'm really struggling with the numerical and theoretical boundary current transport scalings in figure 3, especially the upper panel for the Stommel gyre. I understand that the authors will state that these results support their conclusions, but they are at odds with the basic dynamics of the low α limit (see major comment above). The explanation in lines 194-189 of what has been done to obtain the theoretical scalings, and why, is confusing (to me at least).

Response: The results shown in Figure 3 highlight the consistency of our theoretical/analytic findings with (dimensional!) numerical simulations. Our response to the major comment above provides the explanation of the consistency between our dimensional numerical simulations and the non-dimensional analysis based on our scaling.

6. Regarding figure 4, are you seriously suggesting that $\alpha = 0.5$ is an appropriate value for the East Australian Current? This would imply the failure of geostrophy, for example.

Response: The parameter $\alpha = rL_x/(\beta L_y^2)$ for each WBC was estimated by substituting $\beta = 2 \times 10^{-11}$ m^{-1}s^{-1}, Rayleigh friction coefficient r = 1/10 (days)$^{-1}$ and the typical dimensions (L_x and L_y) of the basin. Other choices of the Rayleigh friction coefficient do not alter the results. Figure 1 below depicts the results for r = 1/20 (days)$^{-1}$ and the same values of β, L_x and L_y.

We thank the referee for suggesting this. If requested, we will be happy to include the attached figure in the revised manuscript.

7. Following on from point 6, there are numerous other processes that are likely in influence the width of real world western boundary currents ahead of linear bottom drag and lateral friction. These include relative vorticity (Fofonoff, 1954; Charney, 1955),stratification (the deformation radius emerges as a natural length scale), bottom topography (e.g., Hughes and de Cuevas, 2001), eddy fluxes (e.g., Eden and Olbers, 2010).

Response: We agree. However, none of these works addressed the role of basin's aspect ratio in determining the transport of the WBC.

References:
Bye, J. A. T., and George Veronis. "A correction to the Sverdrup transport." Journal of

Physical Oceanography 9.3 (1979): 649-651.

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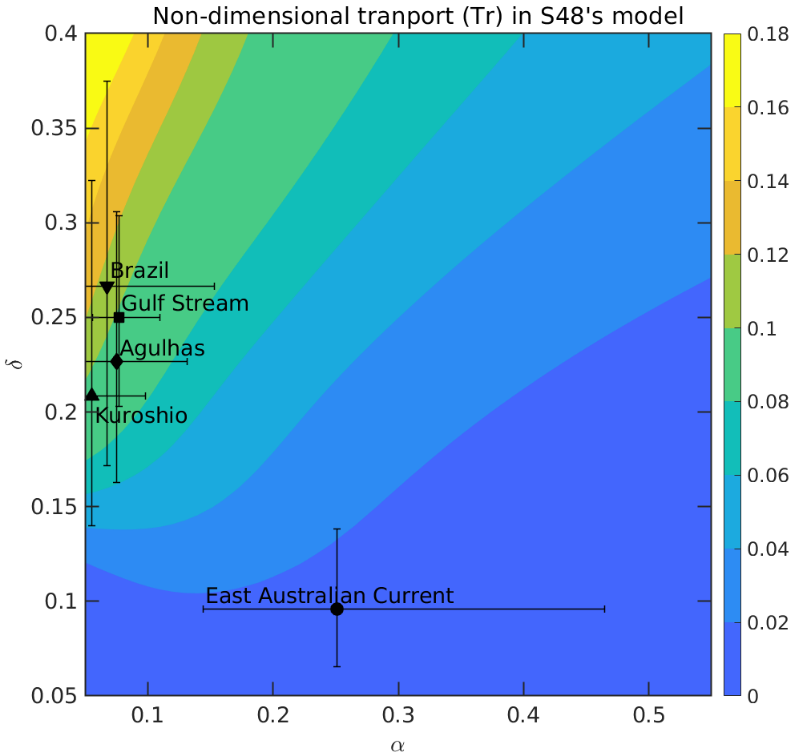

Fig. 1. Alternative figure to panel (a) of Figure 4