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Comment

Interactive comment on “A simple and self-consistent geostrophic-force-balance model of the thermohaline circulation with boundary mixing” by J. Callies and J. Marotzke

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This is a very nice paper which describes a simple model of the meridional overturning circulation in a rectangular basin, formulated in terms of buoyancy, continuity and momentum balances at the western and eastern boundaries. The main strength of the paper is the predictive power of the model, despite its relative simplicity. I am therefore happy to recommend publication in Ocean Science.

I have only minor comments for the authors to consider in producing a final manuscript.

General comment: The review of previous literature is heavily focused on the work of Marotzke and coauthors. In particular, some reference of the single-layer pycnocline

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model of Gnanadesikan (1999, doi: 10.1126/science.283.5410.2077) would be appropriate, especially in the context of neglected Southern Ocean processes. (The relation of the Gnanadesikan model to western, eastern and interior dynamics is discussed in Johnson et al., 2007, doi: 10.1007/s00382-007-0262-9.)

Section 2.2: The authors might also usefully refer to Cessi et al. (2010, doi: 10.1175/2010JPO4426) which presents an alternative and, to my mind, more coherent explanation of the effective boundary condition as applying to the residual, mean plus eddy-induced, velocity. Even though the present study does not include an explicit parameterisation of eddies, I suspect the model could be reformulated in this manner.

Page 1823, line 26: Be consistent in use of Section or Sect.

Page 1827, lines 13-16: Please explain the statement that the implicit balance is between the effect of Rossby waves and meridional and vertical advection (and convection), due to w_w being constant across the boundary layer. I do believe that there is a Rossby wave balance implicit in these arguments, but I am less sure this is the most useful way to think about the western boundary dynamics. I would also hope, and expect given the success of the present model, that the most basic dynamical balances would not rely on the constancy of w_w across the boundary layer.

Page 1834, footnote 1: While I am not overly concerned by this point in the present context, I am surprised to learn that the authors are using a numerical advection scheme that can introduce such an effect. It is refreshing, however, to see numerical convergence being tested in the appendix!

Page 1835, line 9: No need to abbreviate "approximately".

Page 1838, lines 1-3: These results are very nice indeed! However, if splitting hairs, I feel that the statement that the numerical solutions justifies the assumptions in the simple model is the wrong way around; rather the results of the simple model are consistent with the full numerical solutions.

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Page 1843, line 25: "Indicate" (present tense).

Page 1844, equation (41): Please explain the equatorial thermal wind equations and provide a reference.

Page 1844, Line 3: Longer term, inclusion of a Southern Ocean is surely essential? This is not intended as a criticism - the closed interhemispheric basin is sufficiently challenging and interesting - but there is lots of evidence to suggest that the dynamics is substantially different with a Drake Passage (e.g., Gnanadesikan, 1999).

Interactive comment on Ocean Sci. Discuss., 8, 1819, 2011.

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