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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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We thank David Marshall for this thorough review. Below our replies to his remarks:

1. 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 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.)

Reply: We agree and are going to include Gnanadesikan (1999) and Johnson et al.

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(2007) in both introduction and outlook.

2. Comment: 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 parameterization of eddies, I suspect the model could be reformulated in this manner.

Reply: We are going to include Cessi et al. (2010) as a reference and change the wording to "The buoyancy balance inside the boundary layer is hence implicitly assumed between vertical advection and eddy fluxes (Cessi and Wolfe, 2009; Cessi et al., 2010)." (page 1826, lines 12–14).

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

Reply: Our hands are tied by OS's textual conventions.

4. Comment: 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.

Reply: We are going to change the wording to "Inside the western boundary layer, the buoyancy balance is hence implicitly assumed to be between meridional and vertical advection and eddy fluxes (potentially plus convective mixing)." (page 1827, lines 14–16) and insert an explanatory footnote that reads "A more realistic boundary-layer representation appears feasible, for example one that includes friction in the meridional momentum balance (Robinson, 1970, doi: 10.1146/annurev.fl.02.010170.001453). Nevertheless, to keep the model formulation as simple as possible, we resort to the

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representation given in the text."

5. Comment: 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!

Reply: Noted, no change in paper.

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

Reply: We agree and are going to write out "approximately".

7. Comment: 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.

Reply: We are going to add "assumptions leading to the scaling" in line 3. Otherwise, we feel that our statement is appropriate, because the scaling makes a number of additional assumptions, the justification of which is a priori not clear.

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

Reply: We are going to change to "have indicated" (present perfect), because we refer to a published work.

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

Reply: We are going to change to "... [equatorial thermal wind equations], where all symbols are the same as in Sect. 2.2, might hence successfully be used (Lukas and Firing, 1984)."

10. Comment: Page 1844, Line 3: Longer term, inclusion of a Southern Ocean is

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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).

We agree and are going to change the wording to "Eventually, a representation of the Southern Ocean is highly desirable (Gnanadesikan, 1999)."

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

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