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Interactive comment on "Reconciling the north–south density difference scaling for the Meridional Overturning Circulation strength with geostrophy" *by* A. A. Cimatoribus et al.

Anonymous Referee #1

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This paper seeks to reconcile scaling theories for the strength of the MOC based on interhemispheric density contrast with the constraints of thermal wind balance on the east-west density contrast in the Northern hemisphere. This point borders on trivial, since the validity of geostrophic and hydrostatic balance for the large-scale flow is widely accepted. Nevertheless, there is sufficient confusion in the literature to justify publication of this straightforward paper, which makes its point very clearly. The authors argue that there is a tight link between the buoyancy of the ACC region and the buoyancy of the eastern boundary, and they verify this hypothesis with numerical experiments. It will be useful to be able to cite this result. The writing is concise and the figures are clear. But there are a few issues that are in need of revision.

My main criticism is that "the MOC" is never actually defined, resulting in many ambiguities. Presumably Psi refers to the Eulerian mean meridional overturning streamfunction. I think that the actual equation for Psi should be written down in the paper. This issue is relevant because there are in fact many different possible definitions and choices regarding "the MOC." It is not stated whether the Psi diagnosed here includes the eddy-induced transport from the Gent-MicWilliams parameterization; this must be clarified. Most importantly, the thickness-weighted circulation in isopycnal coordinates (rather than height coordinates) can provide a different, and in many cases, more physically relevant measure of meridional transport. For example, even for a steady flow such as Fig. 1, if the meridional flow is returned at the same depth but at different densities, the two diagnostics will give very different pictures. Among the works cited, Wolfe and Cessi and several others used isopycnal averaging. Therefore, a clear discussion of these issues seems necessary in order to connect with other works. How different is the Eulerian-mean overturning from the isopycnal mean? Additional diagnostics may be required to address this point, unless the authors can cite another paper where the issue was assessed in a similar model.

On a related note, I would have appreciated some discussion of what might happen in a higher resolution (eddy resolving) model. It seems likely that thermal wind balance would be easier to violate in the presence of strong mesoscale turbulence in western boundary currents. Such turbulence can lead to significant reynolds stresses that could potentially drive meridional mean flows. The eddies can also contribute directly to the thickness-weighted overturning via bolus fluxes. These are known to be very important for the ACC. Are they completely irrelevant for the Northern hemisphere?

My final major comment regards notation and organization. I found it very difficult to keep track of the different variables and scalings. Equations 1, 2, 3, and 4 are all basically the same; the only difference is the subscripts. I recognize that this is somewhat inevitable. Perhaps in its final form, with the figures in line with the text, it will be easier to read. One solution might be to put all the variables in a table, so the reader can easily refer to them.

A few small comments: p. 6 line 10: I think it would be useful to employ the word "entrainment" to describe this mixing process. p. 6 line 27: Restoring on a monthly time scale? Is there a seasonal cycle in this model? Is it relevant to the MOC? Fig. 4: The contours are quite hard to see. Consider changing the color scheme.

Interactive comment on Ocean Sci. Discuss., 10, 2461, 2013.