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Ocean Science Discussions

Interactive comment on "Chaotic variability of the meridional overturning circulation on subannual to interannual timescales" *by* J. J.-M. Hirschi et al.

M. Hecht (Editor)

mhecht@lanl.gov

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Dear Dr. Hirschi,

Referee 2 has provided comments in response to your plan for revision, included below. Please take these points into consideration when you revise your manuscript, along with the original pair of reviews to which you've already outlined a response.

I look forward to receiving your revised manuscript. Of course, let the journal know if you need a bit of extra time. Here, in some sense, you're being asked to respond to a subsequent stage of review.

Yours, Matthew Hecht

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Interactive Discussion

Discussion Paper

The authors provided additional figures which rather support my suspicion that water mass drift matters here. They should be able to provide a physical explanation for the low correlation between both passes in the deep ocean especially on interannual timescale (Fig. 8, Fig. 1-3 of the reply).

Otherwise I have two suggestion for the authors:

Suggestion 1) Redo the experiment as recommended in my first reply. I am sorry for being not precise enough so I will give some supplementary notes.

The one year lag between both passes was suggested to avoid any complication with the seasonal cycle. The second pass could start with one year lag around year 1976 of pass one which would only give $\sim$25 year additional integration. There are of course more sophisticated ways of initializing model ensembles.

I agree with the authors and Balu Nadiga that bulk formulation are the most realistic boundary conditions and that prescribed fluxes underestimate chaotic variability. Realistic boundary conditions will give you the most realistic estimate of the "chaotic" MOC compared to the driven MOC. On the other hand, as shown in the paper, the chaotic MOC variability is model dependent and a realistic estimate seems difficult. If you aim to understand the physics of the internal driven variability, prescribed fluxes are easier to interpret as they rule out forced variability in such a comparison. (My comment regarding mixed boundary conditions was indeed not very precise. The idea was to diagnose all fluxes and prescribe them in an additional run.)

Suggestion 2) Concentrate on aspects and figures which you understand and were water mass drift is unimportant. For example, If you want to stress the high correlation in Figure 8 you should use a different color scale as the eye catcher here are the weak correlation at the bottom. You could omit the figures and discussion on interannual variability (although I would prefer a physical interpretation even if the answer is water mass drift). In the current draft many figures raises more questions than the paper is able to answer,e.g. the chaotic variability near the equator or the chaotic variability

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of the non-eddying model. The additional figures in both replies do not give much additional insights as the physical mechanisms are still unclear.

A final comment: The authors should stress the point that they are discussing the Eulerian Streamfunction. In contrast to the Residual Streamfunction (Eddy+Eulerian) the Eulerian Streamfunction might be not very relevant in the Southern Ocean (Deacon Cell) and in the shallow equatorial cells. A physical interpretation of the differences of Figure 9 of the eddying model and Fig 3 (second reply) in the non-eddying model without knowing the Eddy Streamfunction is difficult. I assume the non-eddying model uses a constant thickness diffusivity in a Gent&McWilliams parameterization. A couple of recent paper show that the Southern Ocean MOC reacts different to wind stress changes in such models compared to eddy resolving models.

Interactive comment on Ocean Sci. Discuss., 9, 3191, 2012.

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