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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.

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The authors consider the difference in MOC between (the latter 25 years of) two repeated cycles of the same forcing in an ocean model (NEMO) to estimate mesoscale component of MOC variability. This is done at an eddy-permitting quarter degree resolution (A025 and B025) and a coarser one degree resolution (A100 and B100). The findings in the context of these experiments can be summarized as follows:

1. MOC variability is largely a reflection of surface forcing down to significant depths

2. Comparing A025 and B025, the ratio of chaotic (mesoscale) variability to total variability is larger on interannual time scales than on sub-annual time scales and larger in Atlantic than in Indopac

3. At depths where the maximum MOC occurs, chaotic MOC variability is about 30% of the total MOC variability at the eddy-permitting resolution considered and 5-10% at the coarser resolution.

Here are some of my comments and concerns.

1. One of the main restrictions of this study is the use of constant buoyancy flux to force ocean circulation. This is particularly so at eddy-permitting resolutions since mesoscale eddies can significantly affect buoyancy forcing (as noted by the authors). As may be expected, the problem of forcing ocean-only circulation is a common one and has been addressed in numerous publications, as e.g., in Griffies et al., Ocean Modelling 26 (2009) 1–46. A seemingly accepted protocol is the use of bulk formulae to compute the fluxes using the evolving ocean state and specified atmospheric fields.

Notwithstanding the fact that the authors note that their estimate is to considered a lower bound, the point is that the use of constant buoyancy flux consistently underestimates chaotic variability, and this underestimation would have been mitigated with the use of the 'accepted' bulk formulae method to compute buoyancy fluxes. Given this limitation, I'd be cautious about over-interpreting the results.

2. I would have additionally considered differences in the variability of MOC between, say, B025 and B100 to come up with a second estimate of mesoscale related variability of MOC. This would provide a consistency check.

3. Given the sub-annual to interannual focus, I would have devoted more effort to analyzing the barotropic component of MOC variability. In some high resolution N. Atlantic experiments I have done, I see that they can dominate MOC variability at high frequencies.

4. I found the part on the conceptual model not useful.

4a. I strongly suspect that the sqrt(2) factor that the authors find is related to the fact that the two ocean noise processes representative of mesoscale activity appear only as a difference in the conceptual model. For example, the variance of the sum (or difference as in the present case) of two normally distributed independent samples is the sum of variances of the two samples. However, the authors do not provide enough detail in this context to say for certain. In any case, Eq. 6 is sufficient to make the point.

4b. The conceptual model is somewhat inappropriate in that the model is not capable of exhibiting chaos and sensitive dependence on initial conditions (IC). Consequently, the fact that the ICs are different hardly matters and the difference in q (dq) related to the externally-input 'ocean noise'

4c. Forcing is missing in the prognostic equations of the Stommel-like 2 box model

5. The authors do not attempt to explain the non-zero level of 'chaotic' variability in the A100/B100 experiments. If one should attempt this, the results of the experiments may suggest that 'mesoscale and smaller scale processes' may not be the only contributors to chaotic variability. And that subject to the same forcing, the ocean may develop large-scale, quasi-periodic or chaotic oscillations

6. The article says that the chaotic component of climate variability has no predictability (e.g., beginning of 'Introduction'). To avoid confusion, it may be best to further qualify this. Something along the lines of 'beyond a decorrelation time or a time related to doubling of finite-sized errors...'

I would like it if the authors can address some of these issues before I can recommend publication of the article.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/9/C1334/2012/osd-9-C1334-2012-supplement.pdf

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