Response to the comments of reviewer 2 on "Chaotic variability of the meridional overturning circulation on subannual to interannual timescales"

February 27, 2013

First we would like to thank the reviewer for his/her efforts and useful comments. Below we provide a point by point response to his/her comments and concerns and how we intend to address them in a revised version of the paper. The reviewer's original comments are provided in bold fonts followed by our comments in normal fonts.

My main concern is about the experimental setup of the twin experiments. The initial conditions of the second pass (B025, B100) should only differ from the first pass (A025, A100) in the "chaotic" presence of the eddies and in the phases of the involved waves. A time lag of 1 year between the initial conditions of both passes seems adequate. Why do the authors use the end of pass A as initial conditions for B? My concern is that some results are highly influenced by the drift in water mass properties between both passes. For example, the low correlation of the deep water masses in Fig 8 and 9 which also reflects on the ratio of the chaotic/total MOC in Fig 11, 12 and 13. Does figure 8 and 9 look similar in the low resolution runs? Especially changes within the Deacon Cell should be interpreted carefully if not shown in density space.

The simulations A025, B025, A100 and B100 we used in our study are an ensemble of opportunity that was generated in a different context in the framework of the DRAKKAR project. The motivation for the two passes was to have a second pass where the initial model drift is reduced. This is indeed the case: reapplying the 1958 to 2001 forcing starting from the ocean state obtained at the end of the first pass leads to a much reduced initial drift. By then ignoring the first 18 years of the simulations in our study we ensure that there is no strong initial drift that could affect our results.

The reviewer is absolutely right when he/she says that the water masses will have changed at the end of the first pass. However, we feel that this is not a problem for our study. As we say in the abstract the goal of our paper is to provide an estimate of the MOC variability that is dependent on initial conditions. These initial conditions differ in the ocean mesoscale eddy field (for the $1/4°$ model), wave phases, but also in the water mass properties. Even though this would also be an interesting question is not our intention to isolate the impact of ocean mesoscale eddies but to show to what extent the initial condition dependent MOC variability (i.e. "chaotic" MOC variability) differs between a non-eddying and an eddy-permitting ocean model. Our opinion is that the experimental setup - even if it was not initially intended for this purpose - is adequate for what we want. As said before by comparing the last 25 years of the simulations (years 1976 to 2001) we ensure that we avoid the phase of strong initial model drift and that we compare two experiments (passes 1 and 2) that both start from initial conditions that are more consistent with the surface forcing than e.g. the initial conditions at the start of the first pass which is based on temperatures and salinities from the World Ocean Atlas.

Using a lag of 1 year as suggsted by the reviewer means that both model passes would still be diverging for a number of years. To us such as setup seems to be more adequate to assess the model divergence due to different states of the ocean mesoscale eddy field (where ideally we would take even shorter time differences than one year - a few months typically being the decorrelation timescale for the ocean mesoscale eddy field). This is a very interesting question in its own right, however, we feel that it is beyond the scope of our study. What we want to understand here is how different the MOC variability is after the difference between the two model passes has saturated in a statistical sense. We feel that for that purpose our setup is adequate.

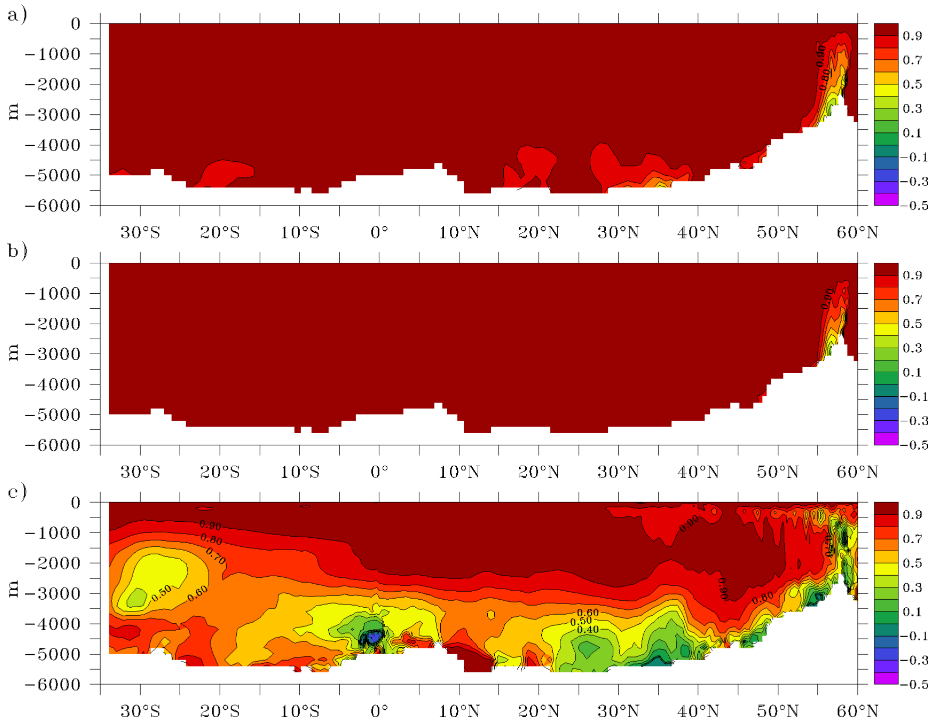

Figure 1: *Correlation between the MOC variability in the low resolution experiments A100 and B100 in the Atlantic.* **a)** *total MOC variability,* **b)** *subannual variability,* **c)** *interannual variability.*

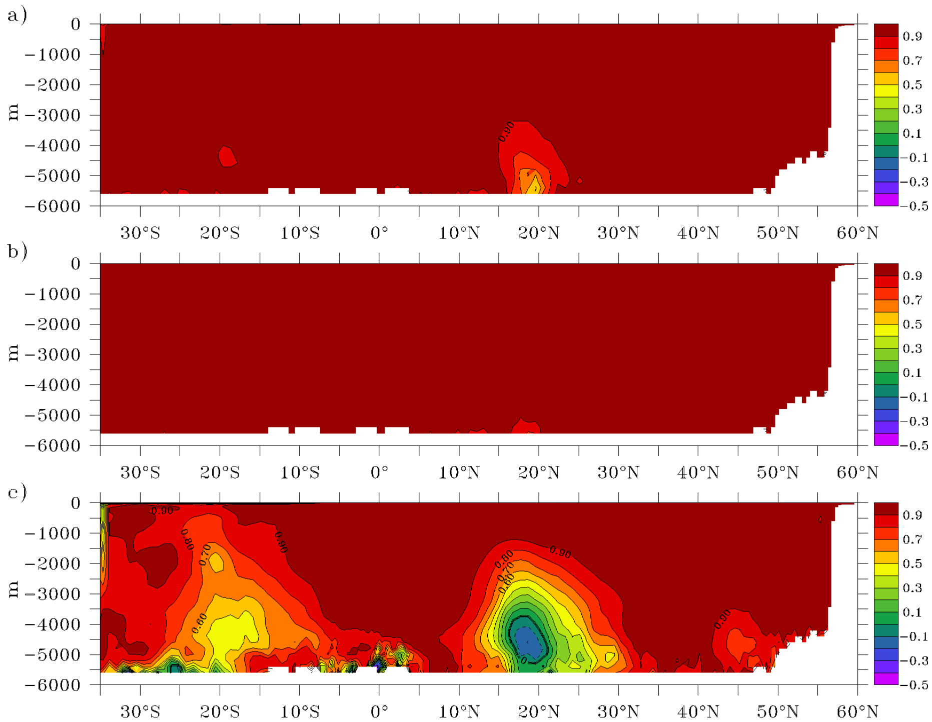

Figure 2: *As above but for Indo-Pacific.*

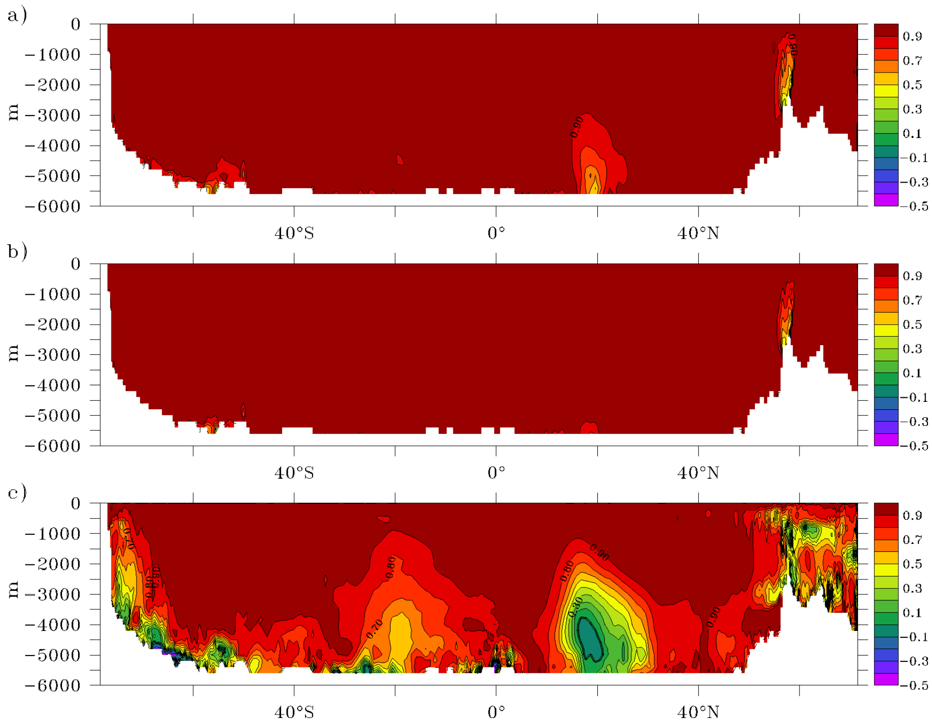

Figure 3: *As above for global ocean.*

In a revised version of the paper we will add the correlation figures 1-3 for the low resolution model and discuss them in comparison to the correlations shown for the eddy-permitting model in figures 8 and 9 of our paper and to the figures added to answer Balu Nadiga's comment about the chaotic variability in the low resolution model.

The most obvious difference compared to the results from the high resolution models is the higher correlation found for the total and subannual MOC variability between the two model passes. For the eddy-resolving model the correlations for the total chaotic MOC variability essentially reflect the chaotic variability found on subannual timescales. For the low resolution model on the other hand the correlations found for the total chaotic MOC variability reflect the variability found on interannual timescales.

Also, in response to Balu Nadiga's comments we will clarify that ocean mesoscale eddies are not the only source of "chaotic" MOC variability. In the high resolution model the impact of ocean eddies is likely to account for most of the chaotic MOC variability found on short (i.e. subannual) timescales. This is illustrated by the much larger chaotic MOC variability found on subannual timescales in the eddy-permitting model compared to the low resolution model (see figure 2 in the reply to Balu Nadiga). The overall chaotic variability is also larger in the eddy-permitting model than in the low resolution model.

Chaotic variability on interannual timescales only accounts for a small fraction of the total chaotic MOC variability in the eddy-permitting model. For the low resolution model in contrast the interannual variability dominates the variability away from the equatorial regions. For the equtorial region the dominant variability occurs on subannual timescales on both the low and high resolution models (see also response to Balu Nadiga). However, despite being largest at the Equator in terms of absolute values the chaotic variability at the Equator only accounts for a small fraction of the total MOC variability (which is also reflected in the very high correlations found between the model passes in the the equatorial region.

As illustrated in figure 3 the correlations for the Deacon cell are clearly higher in the low resolution model than for the eddy-permitting model suggesting that the MOC in this region is more predictable from the forcing in the coarse resolution model. The reviewer suggests to study the Deacon cell in isopycnals but given the length of the paper we feel that such an analysis is beyond the scope of our study and that discussing the differences in terms of correlations is sufficient.

Ideally the twin experiments should have exactly the same atmospheric forcing, i.e. all sea surface temperature and salinity values used for the bulk formula / restoring should be the same in both passes (for example using mixed boundary conditions). Additionally the sea surface velocity dependence of the wind stress should be switched off. A more careful experimental setup would strengthen some of the results which are not understood yet, e.g. the high chaotic MOC variability at the equator or the still surprisingly high values in the non-eddying experiments in some areas.

The atmospheric fields passed to the bulk formula are identical for both passes. The only differences in forcing are due to a weak salinity restoring used in NEMO, as well as to the wind stress which depends on the relative velocity difference between the 10 m winds and the surface ocean velocities. Even though this means that the forcing experienced by the model in the two passes is not exactly the same it is very similar. This is clearly reflected in the high correlations found between both passes in the surface layers where the correlation is 0.9 or higher at all latitudes and on both subannual and interannual timescales (for both high and low model resolutions). This shows that differences in the forcing cannot be the cause for the differences between the two model passes we describe: a difference in e.g. the wind stress would have the largest expression via Ekman transports in the surface layers which should be reflected in the near surface values of the chaotic variability and of the correlations between the model passes 1 and 2. Finally, we would like to mention the reservations of Balu Nadiga about using constant fluxes. We're somewhat confused about the reviewer's suggestion to use mixed boundary conditions (i.e. restoring for temperatures, prescribed freshwater fluxes). Firstly, the fluxes would not be the same between the two passes either (differences in the restoring fluxes for temperature) and secondly mixed boundary conditions have been shown to be excessively sensitive to changes meaning that most likely we would markedly increase differences between the model passes which would lead to an overestimation of the "chaotic" MOC variability. We feel that using the bulk formula is the better option here. We did briefly mention this issue of slight differences in surface forcing and in a revised version of the paper we will add additional explanations in the method section to further clarify this point.

As shown in the reply to Balu Nadiga the chaotic variability is smaller in the low resolution model than in the high resolution model and (apart from the Equator) is mainly confined to the longer (interannual) timescales. The chaotic variability is largest at the Equator. However, compared to the very large Equatorial variability simulated in NEMO it is actually small and only accounts for a small fraction of the total variability (the Equator is one of the regions with the lowest ratio between total and chaotic MOC variability). As already mentioned under the previous point we will more carefully describe the chaotic variability found for the low resolution model in the revised manuscript.

Minor comments:
- p. 3195, only gravity waves are usually considered as internal waves

Ok - we will clarify this.

- p. 3213, a discussion to which latitude A/B025 could be regarded as "eddy-permitting"

Eddies are formed up to high latitudes. For example there is still clearly eddy-activity in the Antarctic Circumpolar Current at 60°N. However, at 1/4° the model cannot resolve the dynamics of these eddies. Nevertheless, the eddy activity mostly occurs at the locations suggested by satellite altimetry, even if eddy kinetic energy is lower than the observations suggest (e.g. Penduff *et al.*, 2010). In the revised paper we will provide more details about the ability (and limitations) of NEMO 1/4° to generate eddies.

References

Penduff, T., M. Juza, L. Brodeau, G. C. Smith, B. Barnier, J.-M. Molines, A.-M. Treguier, and G. Madec (2010). Impact of global ocean model resolution on sea-level variability with emphasis on interannual time scales. *Ocean Science*, *6*, 269–284.