Response to Balu Nadiga's comments on "Chaotic variability of the meridional overturning circulation on subannual to interannual timescales"

February 27, 2013

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

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.

We realize that our model description was not clear enough. We did not use constant buoyancy fluxes in our experiments. Instead NEMO uses the bulk formulae method suggested by Balu Nadiga. In the revised version of the paper this will be clarified.

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.

This is an interesting idea which we have followed up. Based on the difference between the variance of the MOC in the $1/4°$ and the $1°$ models we have calculated the standard deviation of the additional variability that is occurring in the $1/4°$ model compared to the $1°$ version. We illustrate this for the Atlantic in figure 1. When we compare the variability obtained from the difference in variance with the "chaotic" MOC variability shown in figure 13 of our paper we can see that the variability patterns look similar in both cases. However, it is also obvious that the values are higher than the ones we report in our paper. In our opinion this is not surprising. Looking at the difference between two model passes provides an estimate of the MOC component that cannot directly be predicted from the forcing. However, when we increase the model resolution from $1°$ to $1/4°$ not all the additional variability we see for $1/4°$ has to be unpredictable. Some of it will be readily predictable from the forcing, i.e. the overall MOC variability increases not just because of "chaotic" processes such as ocean mesoscale eddies but it can also increase as ocean processes such as wave processes or western boundary currents are better resolved. In the revised version we suggest to add a few paragraphs and a figure similar to figure 1 to illustrate this point.

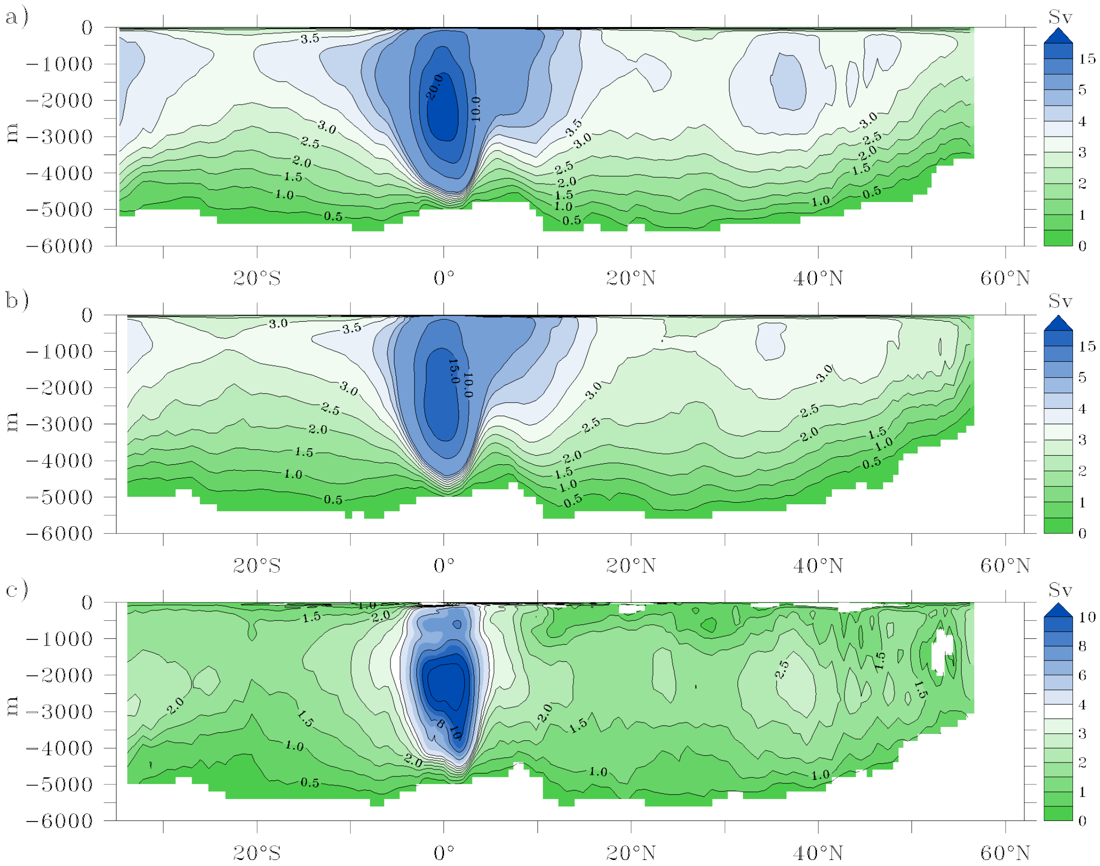

Figure 1: *Standard deviations of the Atlantic MOC in the* 1/4° *(top) and* 1° *versions of the model. The standard deviation of the additional variability occurring in the* 1/4° *model is shown in the bottom panel. It is calculated according to* $\sqrt{var_{0.25} - var_1}$

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.

Even on the short timescales of interest here it would not be enough to look at the barotropic components of the MOC variability to understand the precise nature of the chaotic MOC variability. We have actually started to look at the chaotic MOC variability in the different components of the MOC (barotropic, geostrophic, Ekman). The chaotic variability found in barotropic component actually exceeds that found for the MOC. However, the same is also true for the geostropic component. This is not unexpected given that that there is compensation between the barotropic and the geostrophic components of the MOC (e.g. Kanzow *et al.*, 2007). We agree that looking at the different components of the chaotic MOC variability is of interest. However, as we say in the discussion we feel that this would be a separate study in its own right. Given the length of our manuscript we feel that this should be left for a future study. The main goal of our paper is to provide an estimate of the amplitude of the initial condition dependent (i.e. "chaotic") MOC variability and to what extent the MOC variability is a predictable response to surface forcing.

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

When we presented our results at conferences or during seminars the audience found the box-model a useful way of illustrating the idea behind the work. This was our motivation for including it in this manuscript. We still think that the box model makes the later results obtained with the NEMO model more accessible and therefore believe that it should be kept in a revised version of the manuscript. However, as detailed below we would make some modifications to the box model in response to Balu Nadiga's comments.

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.

We agree that the factor of $\sqrt{2}$ only appears as a difference in the box model. This was the actual motivation for introducing the box-model in the first place as in our opinion it illustrates nicely that by just taking the difference between two model passes (both subject to a noise of unknown amplitude as is the case in the our NEMO simulations) one is likely to overestimate the amplitude of the "chaotic" variability. This happens because the variance of two independent samples is the sum of the variances of the two samples. In the revised version we will make sure that we explain this more carefully.

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'

Balu Nadiga is right when he says that the "ocean noise" is externally input without any dependence on initial conditions. For the revised paper we suggest to replace the white "ocean noise" ζ_1, ζ_2 with a noise that depends on the actual values values of ρ_1 and ρ_2. This new noise is obtained using the logistic equation $f(x) = rx(1-x)$. Depending on the value of r the iteration of f(x) leads to either periodic or non-periodic series of numbers between 0 and 1 (if $0 < r < 4$). We set the parameter r to a value where the Ljapunov exponent is positive, i.e. where the iteration of the logistic equation does not lead to a periodic series of numbers. The ocean noise $\zeta_{1,2}$ is calculated by iterating the logistic equation 100 times:

$$\zeta_1(t) = f^{100}(x_1(t)) - 0.5, \tag{1}$$
$$\zeta_2(t) = f^{100}(x_2(t)) - 0.5, \tag{2}$$

The values of x_1, x_2 depend on the value of ρ_1 and ρ_2 according to

$$x_1(t) = 0.5 + \rho_1(t)/20, \tag{3}$$
$$x_2(t) = 0.5 + \rho_2(t)/20. \tag{4}$$

The parameter r is chosen so that the Ljapunov exponent is positive, i.e. where the iteration of the logistic equation does not lead to a periodic series of numbers. The values of x_1 and x_2 at the end of the iteration are then scaled and added to ρ_1 and ρ_2:

$$\rho_1 = \rho_1 + 5x_1, \tag{5}$$
$$\rho_2 = \rho_2 + 5x_2. \tag{6}$$

In a revised version of the paper we would replace the old figure 4 with a new figure showing the box-model results with the new formulation (figure 2 below) and would modify the text accordingly.

Even with the "chaotic" component now depending on the logistic equation there is still a factor of approximately $\sqrt{2}$ between the amplitudes of the "chaotic" variability in one simulation and in the "chaotic" variability inferred by taking the difference between two model passes. The case shown in the figure 2 is just one example but we have computed many other realisations where we added small

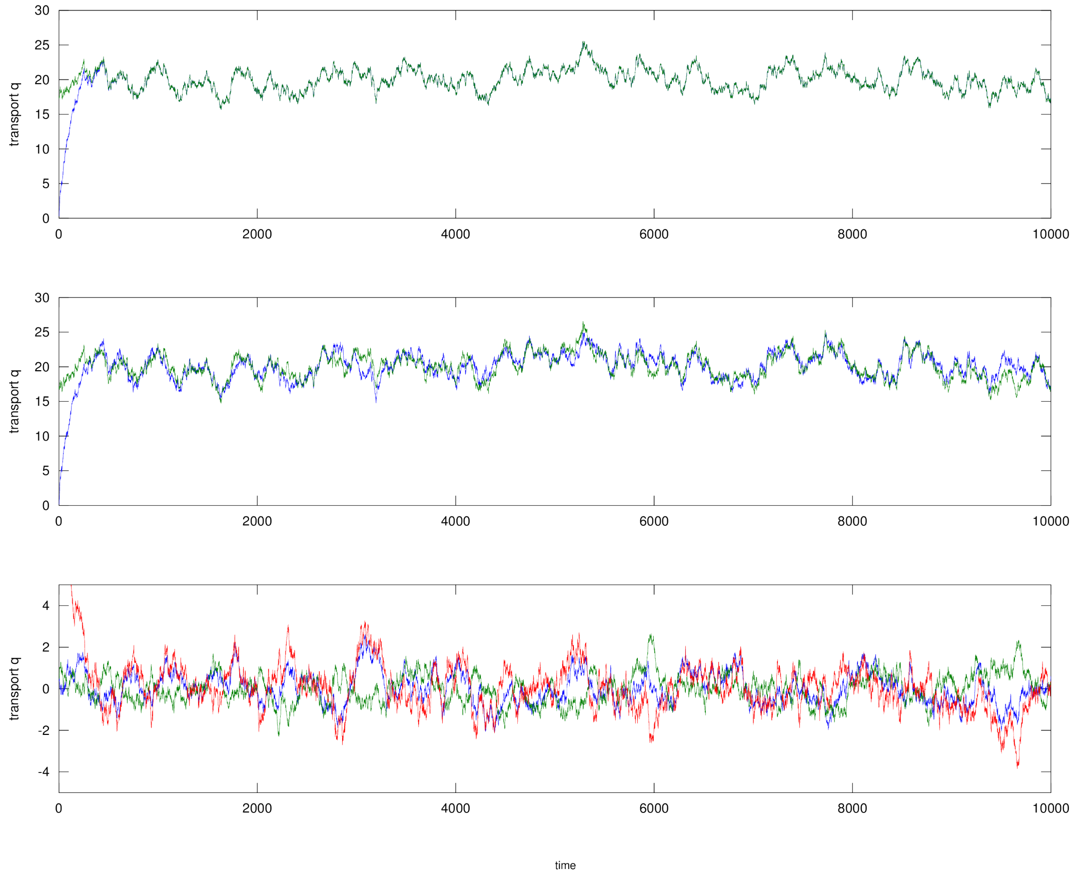

Figure 2: *Results from box model with ocean noise obtained using the logistic equation. Top: two model passes without "chaotic" ocean variability (spinup: blue, second pass: green). Middle: two model passes with "chaotic" ocean variability (blue: spinup, green: second pass). Bottom: differences between the spinups (blue), the second passes (green), and between the spinup and the second pass for the case with added "chaotic" ocean variability (red).*

perturbations ϵ to x_1 and x_2 i.e.

$$x_1(t) = 0.5 + \epsilon + \rho_1(t)/20, \quad (7)$$

$$x_2(t) = 0.5 + \epsilon + \rho_2(t)/20. \quad (8)$$

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

This was an oversight and we will add the forcing term in the equation. In the calculations the surface forcing was always included.

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

We agree that eddies are not the only contributors and also in response to a comment made by reviewer 2 we will make this clear in the revised version of the paper. We suggest to include a figure illustrating the amplitude of the "chaotic" MOC variability in the experiments A100/B100 and explain the differences with the results found for A025/B025:

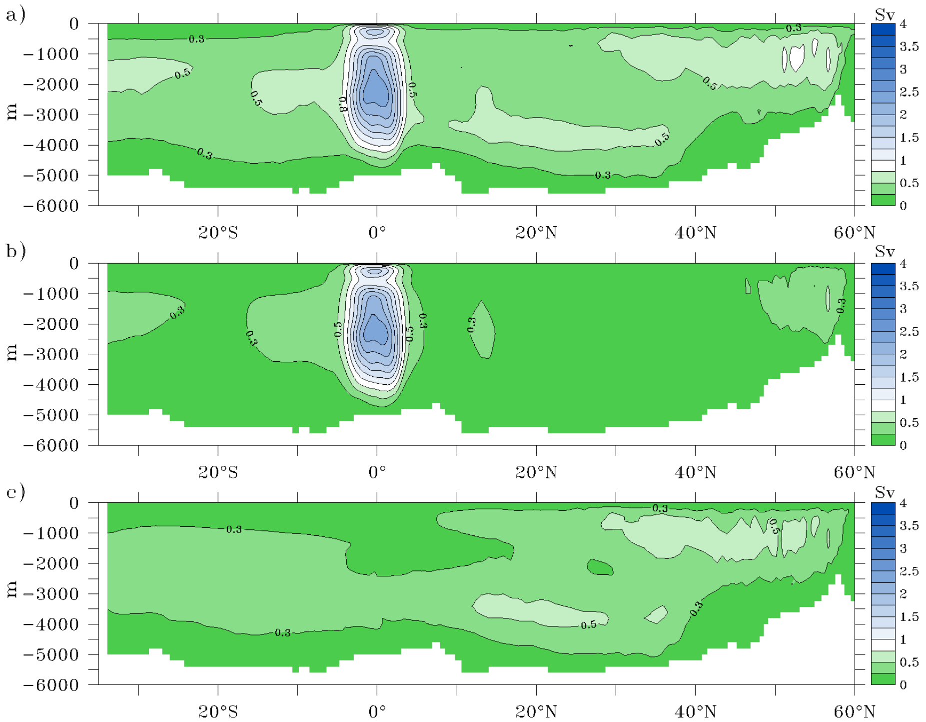

Figure 3: *Chaotic MOC variability in the Atlantic for the* 1° *model.* **a)** *Total chaotic MOC variability,* **b)** *subannual chaotic MOC variability* **c)** *interannual chaotic MOC variability.*

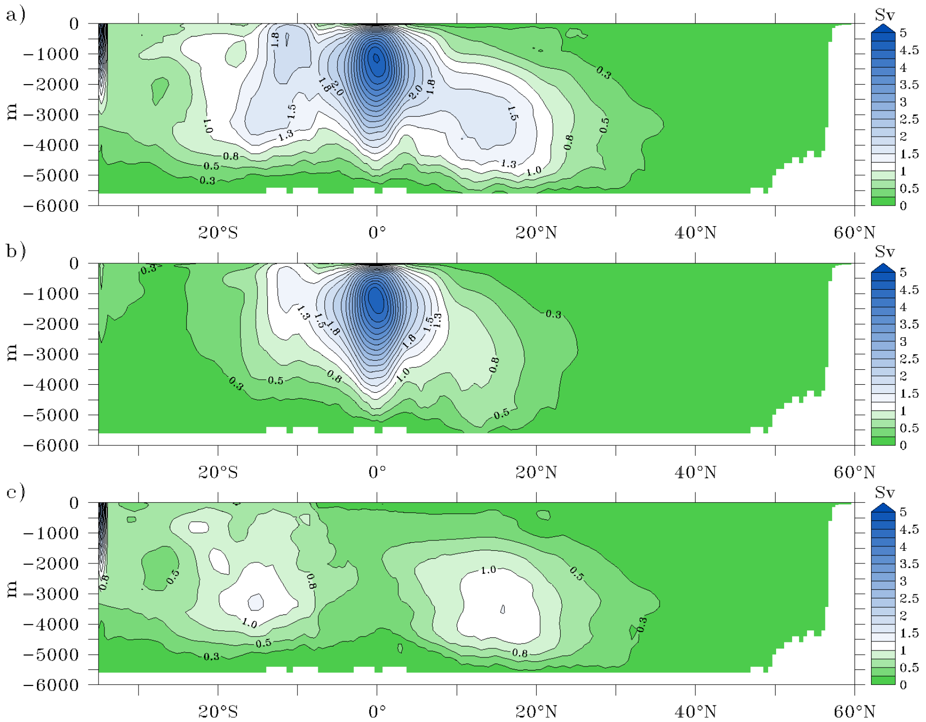

Figure 4: *As above but for Indo-Pacific.*

In contrast to A025/B025 the largest contribution to the "chaotic" MOC variability away from the equatorial region is found on interannual timescales. The chaotic MOC variability at the Equator is confined to the subannual timescales for both the low and high-resolution versions of the model. The overall chaotic MOC variability is smaller in A100/B100 and our results suggest that almost all the variability increase found for A025/B025 occurs on subannual timescales. The amplitude of the interannual variability is comparable for both resolutions with the low resolution even having a higher interannual "chaotic" MOC variability in some regions (e.g. north of about 30°N in the Atlantic, and in the deep Indo-Pacific between 10-25°N.

In the revised paper we will discuss the differences and similarities between A100/B100 and A025/B025 in more detail. Main additions will be to stress that most of the chaotic variability increase in A025/B025 is confined to subannual timescales. We will also revise our statement in the discussion where we suggest that the impact of mesoscale ocean eddies affects the chaotic MOC variability on interannual timescales. Even though the chaotic MOC variability on interannual timescales is higher in places for A025/B025 (especially around 1000m depth for many latitudes of the Atlantic), the picture is not as clear as our previous statements suggest. This will be rectified in the revised paper.

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...'

Point taken. This will be rephrased in the revised version of our paper.

References

Kanzow, T., S. A. Cunningham, D. Rayner, J. J.-M. Hirschi, W. E. Johns, M. O. Baringer, H. L. Bryden, L. M. Beal, C. S. Meinen, and J. Marotzke (2007). Flow compensation associated with the meridional overturning . *Science*, *317*, 938–941. doi:10.1126/science.1141293.