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Interactive comment on “Mechanisms of AMOC variability simulated by the NEMO model” by V. N. Stepanov and K. Haines

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Received and published: 17 May 2013 The authors study four different simulations of the NEMO model. From these runs they relate the variability of the AMOC intensity to density change and wind change (especially link with the NAO). They suggest two different timescales linked to a "fast" and a "long" adjustment of the ocean, of 1 yr and 4 yr, respectively. The long adjustment being related to the deep water formation. However, the authors speculate that both variabilities are forced by wind changes (only the subsequent adjustment, and timescale, is related to ocean dynamics). Given the importance of AMOC in northward heat transport in the North Atlantic, this work is an important topic of the actual research of ocean dynamics. My main concern is the forced paradigm that the authors have inexplicitly chosen for explaining "everything"

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about the ocean variability. Also, even if one considers only exogenous variability, it seems that the authors have ruled out the possibility of the variability being forced by heat and/or freshwater ocean surface fluxes. We do not mean to imply that all ocean variability is completely due to wind variability and we agree there is lots of variability caused by internal ocean variability that the ocean is able to generate itself, however in a forced ocean model there is also lots of variability that can be directly related (correlated) to the applied forcing. The aim of the paper is to demonstrate that wind variability can lead to AMOC variability at two different timescales linked to a "fast" (≤ 1 year) and a "slow" (~ 4 years) adjustment of the ocean, and that these responses do depend on model resolution. Although we really do not separate direct mechanisms of buoyancy forcing (i.e., the effect of heat and freshwater surface fluxes) impacting the AMOC (this has been done e.g. by Robson et al (2012)), the buoyancy forcing will also be changing with the wind stresses and the correlations we detect are still clearly due to forcing. The main focus of the paper is the different responses of the models at different resolution. We have tried to describe our aims clearer now in the text.

Specific comments: 1) The authors seem to consider the ocean as purely slave to wind forcing following the pioneer work of Hasselmann (1976) and Frankignoul and Hasselmann (1977). They acknowledged the existence of internal timescale for adjustment, but no "sustained" variability seems consider. I do not think that a study in this purely forced paradigm is legitimate. In the last twenty years there is a lot of experiment suggesting that the ocean itself is able to generate variability (e.g. Simonnet et al., 2005). For example, p.627 l.16-24 the authors suggest that the only difference is based on model parameterization. I agree that it is one difference, but internal variability (that could be out of phase) or deterministic chaotic behaviour could also explain the differences. Potentially these two could even explain more differences that model parameterization. I suggest the author to read Stone (2004) to a more extensive discussion of this issue. In general, the absence of discussion of such well-known fundamental behaviours shows a lack of thorough study of the topic and of clarity of the manuscript. We do completely agree with this comment and the text was modified to

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extend the discussion.

2) The authors acknowledged the fact that convection could modify the AMOC intensity (through propagation of fast waves along the western boundary). However they did not study at all the role of heat or freshwater fluxes. I feel that these two fluxes could impact density and thus convection... Their suggestion that the wind actually modifies the density is maybe possible but less straightforward. They need to explain their choice. It is true we have not separated out the effects of buoyancy fluxes from the direct dynamical effect of wind stress. We make this point more clearly in the paper now. It would require different sensitivity experiments to do this properly and indeed other work eg Robson et al (2012) and further work underway by Robson is addressing this issue. However the relationship between changes in the winds and changes in the AMOC are clear from these runs, the associated changes in density fields give some indication of the exact mechanisms involved, but the key point is the difference in model responses for different model resolution which we have tried to bring out most clearly

3) AMOC-Ek is never defined even if it is central for the study... My guess is that the authors remove the Ekman layer from their calculations. If it is the case it should be clearly discussed, since it could strongly affect the results. Removing the Ekman layer does remove surface boundary currents and so partially, but significantly, affect the Gulf Stream, for instance. Also the direct impact of the wind is still present through the deep return flow of the surface Ekman transport. The AMOC-Ek was defined on p. 621 l. 22-23, but may be not clear enough, the text has now been expanded. It is the AMOC (i.e the transport in the top ~1000m) without the Ekman component and the seasonal cycle. We do not remove the entire upper layer but remove an Ekman transport derived directly from the wind stress. Thus we removed all directly forced variability at short time scales since the aim of the paper is to consider variability at interannual and longer time scales. It is not clear what the reviewer means by "Removing the Ekman layer ... significantly, affect the Gulf Stream". The direct Ekman driven currents are removed but the indirect impact of wind forcing on the geostrophic Gulf

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Stream is still present of course.

4) Forcing of the model. Is there any restoring term forcing the Temperature and Salinity of the model. This is quite commonly used in the NEMO model. However it has significant impacts on the variability of the ocean dynamics (Huck and Vallis, 2001; Arzel et al., 2006; Sévellec et al., 2009). If this restoring term exists, please define it (intensity and location, i.e. only at the surface or also at depth). To prevent drift in global salinity due to deficiencies in the fresh water forcing, a sea surface salinity relaxation to climatology is applied, with a timescale of 180 days for the top 6 m at the ice-free surface, decreasing to 60 days under ice for ORCA025 and correspondingly 36 and about 7 days for ORCA1. The text has now been modified.

5) Parts of the text in section 5 seem strongly speculative and their conclusions are not demonstrated in the paper: p.624 l.24-25, The above mentioned parts of the text have been expanded.

p.629 l.7-12: We have clarified the text. “Use of high-pass filtering of the temperature variability removing timescales greater than 12 months considerably increases the correlations between PC1 and the GSNW index. Therefore we can conclude.” The stronger correlations for high frequency signals is a good indicator that fast boundary processes are involved.

p.629 l.15-21, i.e.: “The correlations between high frequency AMOC-Ek at 26.5N and density at the western boundary show that the density signal from 35N can influence the AMOC: there is a significant correlation between AMOC-Ek at 26.5 N and the western boundary density at 35N from the surface to the deep layers, which is also seen to the south of 35 N below 1000m (Fig. 6g–h). Most of the monthly signals are similar to the annual correlations but correlations are smaller, due to introducing noise at the sub-annual frequency.” This is simply pointing out that the correlated signals are on longer timescales and that monthly data introduces noise which decreases the overall correlations.

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p.630 l.14-2, The parts of the text have been rewritten: “We conclude that in the ORCA025-G70 run about 30% of the total interannual variability in AMOC-Ek transport is due to dense water formation in the Labrador and Irminger seas (it is based on the fact that the standard deviation of interannual ORCA025-G70 AMOC variability after subtraction of the low frequency component decreases from 0.65 to 0.45 Sv), and this variability in turn depends on the wind strength over the subpolar gyre 4 yr before, acting to deepen Labrador Sea convection. Another 35–50% of the AMOC variability (based on the correlation coefficients between $\dot{A}DPa$ and AMOC-Ek obtained from all model runs) is described by January–June sea level pressure differences between high and mid-latitudes over the North Atlantic (in other words to changes in the NAO index).” This partitioning is based on the detected correlations between $\dot{A}DPa$ and AMOC-Ek (with values $\sim 0.6-0.7$) which then imply that the 35-50% variance is related to $\dot{A}DPa$.

l.7-9 p.631 The above parts of the text have been rewritten.

6) Most of the Figures and Caption are not easy to interpret. Please see minor comments for more specific problems. We have tried to improve the presentation throughout.

Minor comments: p.625 l.24-27: I cannot see that in the Fig.2. It is clearly seen now in enlarged Fig. 2g,h as a thin red strip near the western boundary.

p.627: Use full name when describing experiment, i.e. R07 and G70 should be replaced by ORCA1-R07 and ORCA025-G70. Done

p.628 l.16: Please define GSNW. It is this definition of the Gulf Stream north wall GSNW-index and has been described in more detail now in the text

p.629 l.13: "correlations" please define correlation of what with what. Done

Equations p.632: Please use dash (as in the text) instead of "minus" sign when needed. We modified the equation as requested.

Captions: Most of the captions are confusing and should be rewritten to clearly explain

what is plotted. Figures: There are a lot of panels in each figure and the author should take advantage of panel title to explain what is plotted (e.g. name of experiments, field plotted). We have followed the reviewers advice and expanded on the legends to include more details We have checked all captions.

Fig.2: The 8 panels should have the same latitudinal and zonal extent (extent are wider for panel a and b). Done

Fig4: Labels and names of the experiment are inconsistent. Please also use the full name of the experiments (including reference to ORCA grid type). The changes are made.

Fig.5: I suggest that this figure goes first or second and be discussed right at the beginning of the manuscript (as the mean state to “validate” the experiment). We would prefer to leave this figure where it is because the main point of the paper concerns the differences in the variability which are then discussed first. It is not immediately obvious from this figure that the variability sections could be better understood if this was displayed first.

Fig.6c: Hard to read. Try to be consistent between the experiments and the line types (e.g. black for ORCA1, red for ORCA025, dashed for long run and plain for short runs.) Done.

Fig.6 captions: "PC1" is not clear enough, please explain PC1 of what. The caption has been modified (the definition of PC1 was previously only given in the main text).

Please also note the supplement to this comment:

<http://www.ocean-sci-discuss.net/10/C269/2013/osd-10-C269-2013-supplement.pdf>

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