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

Anonymous Referee #1

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The authors study four different simulations of the NEMO model. From these runs they relate the variability of the AMOC intensity to density field 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" 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.

I do not recommend this work for publication as it is.

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 Hasselman (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.

2) The authors acknowledged the fact that convection could modify the AMOC intensity (through propagation of fast waves along the western boundary). However the 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 modified the density is maybe possible but less straightforward. They need to explain their choice.

3) AMOC-Ek is never defined even if it is central for the study... My guess is that the authors remove the Ekman layer form 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.

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).

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, p.629 l.7-12, p.629 l.15-21, p.630 l.14-2, and l.7-9 p.631.

6) Most of the Figures and Caption are not easy to interpret. Please see minor comments for more specific problems.

Minor comments: p.625 l.24-27: I cannot see that in the Fig.2. p.627: Use full name when describing experiment, i.e. R07 and G70 should be replaced by ORCA1-R07 and ORCA025-G70. p.628 l.16: Please defined GSNW. p.629 l.13: "correlations" please defined correlation of what with what. Equations p.632: Please use dash (as in the text) instead of "minus" sign when needed. 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 figures and the author should take advantage of panel title to explain what is plotted (e.g. name of experiments, field plotted). Fig.2: The 8 panels should have the same latitudinal and zonal extent (extent are wider for panel a and b). Fig4: Labels and names of the experiment are inconsistent. Please also use the full name of the experiments (including reference to ORCA grid type). 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). 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.) Fig.6 captions: "PC1" is not clear enough, please explain PC1 of what.

References: Arzel, O., T. Huck, and A. Colin de Verdière, 2006: The different nature of the interdecadal variability of the thermohaline circulation under mixed and flux boundary conditions. J. Phys. Oceanogr., 36, 1703-1718. Frankignoul, C. and K. Hasselmann, 1977: Stochastic climate models, part II. Application to sea-surface temperature anomalies and thermocline variability, Tellus, 29, 289-305. Hasselmann, K., 1976: Stochastic climate models Part I. Theory, Tellus, 28, 473-485. Huck, T. and G. K. Vallis, 2001: Linear stability analysis of three-dimensional thermally- driven ocean circulation: application to interdecadal oscillations. Tellus, 53A, 526-545. Sévellec, F., et al., 2009: Nonnormal multidecadal response of the thermohaline circulation induced by optimal surface salinity perturbations. J. Phys. Oceanogr., 39, 852-872. Simonnet, E., et al., 2005: Homoclinic bifurcation in a quasi-geostrophic double-gyre circulation. J. Mar. Res., 63, 931-95. Stone, P. H., 2004: Climate Prediction: The Limits of Ocean Models. American Geophysical Union Monograph, 150, 259-267.

Interactive comment on Ocean Sci. Discuss., 10, 619, 2013.