

## ***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: 22 May 2013 The manuscript describes the AMOC variability in a set of forced ocean-only experiments. In particular, the role of the wind forcing, and the variability of the upper ocean density field are analysed. The topic is relevant, and the set of experiments can be the basis for new insights. In its present form, the manuscript is lacking a clear focus. I therefore recommend publication after a major revision. My main problem with the manuscript is its focus: - the abstract (2nd part) suggests that the analysis of the wind forcing and the upper ocean density variability is the main focus - the introduction suggests that emphasis will (also) be put on the resolution. - the summary & discussion suggests that the authors explained AMOC variability as a whole. We have addressed the comments below in both the introduction

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and discussion sections to bring out the motivation of the paper more clearly. We also make clear that this does not explain all AMOC variability of course, but the mechanisms do show where strong relationships to surface forcing can be found in the model results.

1. I strongly recommend to focus the introduction, essentially formulating a clear hypothesis/ question that will be addressed. The authors should pay close attention that their ocean-only setup comes with strong inherent limitations (of course also strengths, as the same model with defined forcing is used at different resolutions). However, I have not found an acknowledgement or critical discussion of this limitation. Nor have I found a reasoning what the set of experiments can describe and what not. The introduction and the abstract have been modified emphasising that we have investigated the impact of wind variability and model resolution on the AMOC variability at 26.5oN at different time scales using ocean-only setup model. Sets of experiments are now described in section 2. The strengths of the ocean forced runs means that the surface boundary conditions are controlled so multiple experiments with the same forcing can be studied.

2. It would be potentially interesting to describe the effects of different forcing and resolution, but at present the authors mix all of these aspects. The main aim of the paper is to demonstrate that wind variability can lead to AMOC variability at two different timescales linked to a "fast" ( $\leq 1$  year) and a "slow" ( $\sim 4$  years) adjustment of the ocean but that these responses are sensitive to model resolution in different ways. We have tried to describe the motivation more clearly in the new paper.

3. After a clear focus has been formulated, I see potentially very interesting results emerging here. It would also add a 'narrative' to the correlation analysis (in section 4). Also, section 5 should be disentangled with respect to what the authors actually found in their analysis, and what they infer, or found justification in the literature for (e.g., the last paragraph on p 629). Again, a focus on a single aspect (forcing or resolution) would make this potentially interesting. The paper is not about forcing OR resolution but

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as stated above we use the common forcing, which shows clear correlations with the AMOC variability, and compare the responses in models of different resolution. We look at the density distributions and correlations as well as the AMOC itself because they drive the geostrophic circulations and they provide some insight as to why the different resolution models have different AMOC responses to the same forcings. Hopefully this is stated more clearly now in the new paper.

4. Apart from an open discussion of the limits of an ocean-only setup, I am also missing a critical discussion of the particular model used in the analysis. Also, I think the last section is entirely missing a critical discussion of the results. To me, the section has the wrong title, as it is merely the final part of the results: it is at minimum unusual to present new figures in the summary and discussion part (p. 631, l. 22 and p. 632, l. 22). While not impossible, I see no justification for this here. This model setup is well covered in the literature because it is the NEMO modelling framework that is being used throughout Europe both for scientific and operational work. We have added some more references to literature that has described previous scientific studies with the model. The last section has now been rewritten as requested in order to include more discussion of the results

Minor comments (going in page order): p. 621, l. 6: references listed here do not appear in the list of references; please check all references carefully. Done, the references are now added.

p. 621, l. 21: how exactly is AMOC-Ek defined? Also, the entire paragraph is quite technical and probably better placed in the methods part. The AMOC-Ek was defined on p. 621 l. 22-23, but may be not clear enough, the text has now been clarified. It is the AMOC (i.e the transport in the top ~1000m) without the Ekman component (defined from the wind stress) and with the seasonal cycle removed.

p. 622: section 2 and 3 could be combined, and the relevance of the setup/ experiments for the subsequent analysis should be brought out. We agree and we have

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combined these sections in the new paper.

Please also note the supplement to this comment:

<http://www.ocean-sci-discuss.net/10/C275/2013/osd-10-C275-2013-supplement.pdf>

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