Ocean Sci. Discuss., 10, C251–C252, 2013
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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 #2

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.

1. I strongly recommend to focus the introduction, essentially formulating a clear hy-

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pothesis/ 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.

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.

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 find 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.

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.

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.

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.

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

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