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Interactive comment on "Water mass transformation in the North Atlantic over 1985–2002 simulated in an eddy-permitting model" *by* R. Marsh et al.

Anonymous Referee #2

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Water mass transformation in the North Atlantic over 1985-2002 simulated in an eddy permitting model by R. Marsh and co authors.

General comments

In this paper, the authors examine water mass transformation in the North-Atlantic over the period 1985-2002 in the OCCAM eddy permitting model. They use a well established water mass transformation diagnostic (Walin 82, Speer and Tziperman 92). From the model output they compute net advection across the boundaries, unsteadiness, and surface fluxes. From those values, they deduce water mass transformation due to mixing. Values are discussed and compared to Lumpkin and Speer. The paper addresses relevant scientific and interesting questions; however no substantial conclusions are reached. The authors should go more deeply in the analysis and interpretation of their results. They should clarify the questions they want to answer and precise how their work relates with what is already known on water-mass transformation in the North-Atlantic (see Perez-Brunius et al for instance). The authors mention that the formation rates present interannual to decadal variability but the authors do not really go further than that. One can wonder whether this variability is linked to the NAO, what are the consequences of such variability on the circulation, heat storage, etc. They should put some efforts in highlighting what is new and original. Indeed, the main result is that mixing leads to formation of intermediate waters which is not very different from Lumpkin and Speer' results. Finally, the authors say that SPMW consumption rate can lag anomalies in the surface formation rate by up to 4 years, but this is not proved. It is just a hypothesis and that should not be in the abstract.

Specific comments

0. p1, 2nd paragraph in the abstract: The authors provide comments on spatial and temporal resolution in the OCCAM surface fluxes but this is discussed nowhere else in the model. That should not be in the abstract.

1. p4, 1st paragraph: The forcing frequency should be specified.

2. LSW box: Why not defining page 6 a LSW box? Why has this box a separate status compared to the other boxes? In this paragraph, it is not evident for me why "in the case of LSW, we consider mixing of the dense variety ($26.7<$sigma0<27.2) in the Mid-Latitudes Box". Why do the authors consider this box for the LSW? Why do they authors consider this density range when LSW is associated with sigma0>27.8 in the following paragraph?

3. Plots presented in Section 4 The authors should be more precise on what is plotted in the various plots of this section and should clarify whether those are cumulative flux or not. They should clarify their definition of "water masses formation rate" and relate it to the equations given Section 3. To improve the clarity of the paper, they should also

relate calculations and plots to the parameters introduced section 3.

4. Surface fluxes (Abstract, Section 4.3 and Section 5, p17) The purpose of the comparison of the water mass transformation rates obtain from OCCAM with that obtained from the SOC climatology is not clear to me. I would think that the aim of such comparison would be to validate the OCCAM forcing fields. Actually, the comparison mainly leads to conclusion about problems in the SOC forcing fields and most of them were apparently documented in previous papers (Josey et al 1999 and Grist and Josey 2003). According to this section, LSW transformation rates peak at 15Sv at sigma0=27.4 which is probably too large by about 50%, but this point is neither discussed nor used in the paper to explain or comment some of the results or comparison with other formation rates. By the way, when considering last paragraph page 11, I suppose the author mean “a maximum formation rate of LSW” and not “a minimum”.

5. The paragraph at the end of p13, beginning of p14 could be clearer. I don’t understand why the density range in Fig8a varies from 25.6 to 27.6 and not from 23 or at least from sigma0=24. Indeed, according to Fig6, water mass destruction occurs from sigma0=24 to 25.6. Text describing Fig 8b could be clarified.

6. Section 4.4, P16, 2nd paragraph: The authors hypothesize that strong consumption rate can lag 4 years behind a peak in formation rate. That may be true but one occurrence of such event does not prove it. In addition, other peaks in formation rates are not always followed by a peak in consumption rate. The authors must be careful in their conclusions, and this hypothesis shouldn’t be written in the abstract.

7. Section 5, P18, end of 1st paragraph: The ECCO project provides interesting and original results in data assimilation and state estimation. However, it seems to me that the ECCO papers are not the relevant papers to cite here to highlight that realistic fluxes and eddy-permitting resolution are better for the large scale circulation and potentially for water mass transformation than less realistic fluxes or lower resolution...Isn’t it something obvious?

Technical corrections

1. p3, last line: Analysis Centre climatology.

2. Colorbar: in most of the plots (Fig. 2 for instance), the same “orange” color is used for two different values in the colorbar. For a better clarity of the plots, the colorbar must be modified.

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