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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 #1

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This is a review of " water Mass transformation in the North Atlantic over 1985-2002 simulated in an eddy-permitting model" by Marsh, Josey, Nurser, de Cuevas and Coward.

General comments:

This article addresses and actualizes the topic of water mass transformation using the most recent climatologies and model configurations available. The redaction is clear, structured and concise. The language fluent and the article pleasant to read. In a limited number of pages the article encompasses a substantial amount of work. The descriptions are meticulous, the scientific methods and assumptions valid but not always clearly outlined. The results are notable and support the different interpretations and conclusions. It is extremely commendable that numerous references are made to related work or data, in order to validate the results, at different stages of the presented work. Nevertheless, this work could be greatly improved if a more complete description and discussion of the mixing concepts, crucial topic of the study, would be performed.

Scientific issues:

There should be more description of the model (z-levels, "Bryan-Cox", Kpp...) without indirect reference to some older publications. Some attention could be given to the closure scheme since it is a central focus of the paper.

The residual method using the water mass transformation diagnostic is not sufficiently explained. Indeed Walin [1982] and Speer and Tziperman [1991] are quoted but i would also expect, for instance, mentions to the following: Niiler and Stevenson [1982], Garett et al. [1995], Speer et al. [1997], Marshall et al. [1999]. Spending more time on the description of the theory the authors might clarify the ambiguity that appears all throughout their article about "mixing".

Initially, there is a lack of description of what the term "mixing" encompasses. Moreover terms like "Water mass consumption", "water mass transformation", "diapycnal mixing", "subduction rate" are too often used as synonyms all along the text, when they are not, and especially when they haven't been described thoroughly.

Also very frustrating is the lack of description of how much of mixing is due to model intrinsic variability or "spurious mixing". I can't understand why it is not mentioned in the text. It seems also that it should be quantified. Griffies tests (Griffies et al, Spurious diapycnal mixing associated with advection in a z-coordinate model [1999], and some other references where Nurser is one of the contributing author...) should be performed in the given configuration, or the results explicated if they have been done. The model is described as eddy-permitting, but in the higher latitudes the grid resolution is much bigger than the internal deformation radius. One expectation is then the eddy processes to be poorly resolved, and there could be description of that.

Furthermore, adding to the lack of explanation of the different processes potentially involved in the "mixing", there seems to be a confusion between the surface and the interior mixing when these should be treated separately. Mixing in the surface layer and in the ocean interior corresponds to different ocean dynamics associated with much different time scales. To reiterate, this is where a more complete description of the theory is needed in aiding the reader to associated the various mechanisms involved (diapycnal advection, diapycnal mixing, isopycnal mixing, cabbeling, eddy effects, ...) with the observations. The article focuses on interior mixing (the Walin [1982] formulation assumed negligible mixing in the upper ocean), when almost everything might happen in the surface layers. For instance, the article indicates very quick responses in the STMW consumption, compared to the variations of the forcing. We might here consider that most of the signal have been mopped in the the surface layer. It is different with the SPMW where a 4 year lag appears. in that case, are interior processes involved or it is rather corresponding to a timescale of recirculation within the eastern subpolar gyre as explained in the text? A full description and separation of the processes should be done, and in this case, a significant concern could be identifying the rate controlling process for diapycnal transformation in the upper ocean. Do the mesoscale eddies define the rate at which net diapycnal advection would happen or does it primarily depend on the rate at which diapycnal mixing occur?

More generally I would encourage the authors to decompose the mixing term that they define in equation 2, and analyze the contribution of the different terms with time, in the different boxes, in the interior/surface layer... Gmixing= Gdiapycnal + Geddies + Gspurious + Gtidal + Gcabbeling + ... and further determine which processes are dominant for water mass transformation rates with simulations whose sensitivity to mixing in the upper ocean and air-sea interaction is explored systematically.

In the abstract, located on the first paragraph and last sentence it reads: "transformation rates due to mixing are then obtained as the difference between net and surface transformation rates". Why not to complete the study by calculating directly the mixing

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from the model outputs and comparing the results with the residual method?

Is it possible to detail the significances and influences of the unsteadiness mentioned and quantified in the article in the density ranges corresponding to the mode waters? (role of diapycnal advection due to time dependent deepening and shallowing of the mixed layer).

There is mention in the discussion of an observed 10 year variability. Is it Internal to the model, due to the ocean, the atmosphere? Which dynamics are associated with this variability?

Some other questions to consider: How are the outcrop distributions taken into account, why is the North Atlantic current position so biased in the OCCAM results?

Technical corrections:

Page 66, lines 10 to 12 (The "mixing-driven" destruction...). This sentence is irrelevant here and comes with no link with what precedes.

Page 68, line 6. Typo (repetition of sigma layer indices).

Period mean transports across sections: since the transports have been divided per T-S classes, one could expect more statistical comparisons. This would avoid some ambiguities encountered in the discussion. For instance : page 71, line 20: northward transport have shifted toward lower temperature (this is true for the core, yellow dots) and higher salinity (this is not true for the core, yellow dots, but for the small northward transports, red dots). This would also get rid of vague terms like "the majority of northward transport" (page 72, line 4), "substantial northward transport" (page 72, line 23).

Page 72, line 25. "on the EEL half of the amalgamated section". Which half?

Page 73, line 21: The density of northward flows progressively increases across Figs 4a-4e. Say: "the density of northward flows progressively increases with latitude".

Page 75, line 16: broadly Page 76, line 5: broad Page 78, line 23: broad. Is "broad" a scientific term for quantification?

Page 76, line 26: the transformation rates found using unadjusted SOC are in better agreement with OCCAM in a few limited cases, for example at low density in the mid-latitudes box. This is not obvious to me. Isn't there better examples?

Page 79, line 2: Is figure 8 really showing the densest water masses in each region? This is not in agreement with the legend.

Page 79, I wish the water masses would have been previously (introduction?) and more precisely defined.

Figure 5: why not to include the Labrador sea curves (the trend is important) as it is done figure 6?

Figure 7: could you use the same projection for the maps?

Figure 12: in the legend, Atlantic instead of Atalntic.

Interactive comment on Ocean Science Discussions, 2, 63, 2005.