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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.

R. Marsh et al.

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We agree to more clearly state (in our Introduction) the questions we set out to answer. There are two leading questions: (1) What are the long-term (1985-2002) mean effects of surface forcing and mixing on water mass transformation, in the North Atlantic? (2) Given considerable interannual-to-decadal variability in surface-forced water mass transformation (based on existing literature and confirmed in our study), how does mixing respond to this variability? Given that we use a model, and that we address a real time period, a third question necessarily arises: (3) How realistic is the model in representing the processes of transformation and transport of water masses? As suggested by the reviewer, a fourth question arises on the subject of variability: (4) How is variability in surface-forced water mass transformation related to indices of climate variability (such as the NAO-index), and what are the consequences for the transport

and storage of heat in the ocean? Full consideration of this question would involve a further study that is beyond the scope of our current analysis. However, in revising the paper we will attempt a preliminary investigation of this issue.

In terms of highlighting what is new and original, we will more closely examine the literature, including the recommended work of Perez-Brunius on different processes of SPMW formation and another paper (Hall et al. 2004) on interannual variations in Gulf Stream heat transport simulated over 1979-99 in the 1/6 degree CLIPPER model. We accept that one of our main findings - the formation of intermediate waters through mixing - has been previously obtained through an inverse analysis study. However, we believe it is important to support the inverse analysis results with diagnosis of a state-of-the-art model like OCCAM. We will therefore stress this point while highlighting original new results, such as significant correlations between interannual variability in water mass transformation rates and ocean heat transports.

We agree with the reviewer to remove our assertion that variability in the SPMW mixing-driven consumption rate lags that in the surface-forced formation rate, although we will retain discussion of the consumption of anomalous LSW in the early 1990's in the northeast Atlantic. We will take care to discuss this latter event, as we would need a longer simulation, sampling several LSW transients, in order to make a definitive statement about any time lag between anomalies in the formation and consumption rates of LSW.

We thank reviewer 2 for these insights, and we look forward to improving the paper accordingly.

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