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Ocean Science Discussions

Interactive comment on "Impact of hydrographic data assimilation on the Atlantic meridional overturning circulation" *by* G. C. Smith et al.

Anonymous Referee #2

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The aim of this work is to assess the impact of assimilating hydrographic data on the AMOC and its different components. The study is based on a hindcast ocean only simulation that is compared to an assimilation integration and to observed data provided by the RAPID array at 26.5° N. A large set of experiments is analyzed in a very comprehensive way to determine the impact of assimilation in correcting errors associated with model biases. The observed data are only assimilated in the upper 2000m due to the absence of data below. The authors demonstrate quite conclusively that the assimilation has a large effect on improving the subtropical gyre circulation. This is a very interesting result full of promise for model developments and subsequently for the understanding of the mechanisms of the North Atlantic circulation. The authors also show that the corrections hardly affect the AMOC, and explain it by the lack of assimilation in the deeper layers. I am concerned about this point which is the main conclusion of

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the manuscript. In my understanding, the data are assimilated only during three years, which means the ocean below 2000m is not adjusted. So even if there is no doubt that we need to assimilate data below 2000m to get the right water mass properties and thus see potential changes in the AMOC, correcting the upper layers might also affect the deeper layers in a weaker extent, if we look at simulations that are adjusted. This a challenging problem as the Argo data are only available for 5 years so we might not have enough data yet to see the effect on the deep ocean and thus on the AMOC. The manuscript is well written, logically organized and adequately illustrated. It provides a first step toward the development of an ocean assimilation system and highlights the need for deep observed data to constrain the circulation. I recommend it for publication but major revisions are required. The authors should discuss more carefully their conclusions on the lower NADW, and be aware that their experiments can only show to some extent the effect of assimilation on the AMOC, as the gyre circulation might be adjusted after 6 months but the overturning is not. The manuscript is also too dense and too long and could be shortened to clarify the message. In particular some experiments should be described more briefly and some figures could be omitted.

Detailed comments are given below :

-The overturning is referred to as the AMOC, the RAPID array, the RAPID AMOC and the overturning. It can be confusing in some parts of the manuscript.

-p.2672, l.12-13 : ORCA 0.25 is 1/4 ° resolution and not 1/3. It is correct in the rest of the manuscript.

-p.2681, l.9 : show instead of shown

-p.2684, l.1: It would help the reader to have a short description of Baehr et al. (2009) study.

-p.2685 : The influence of assimilation on the AMOC comes too late, while it is the main focus of the study. The previous sections on the different components and the

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transports should be shortened.

-p.2685, l.11-18 : I do not agree with the author's comments on Fig 7d. The seasonal cycle is actually fairly well captured by both CNTL and SYN simulations despite a weaker amplitude. But indeed, there is little difference between the assimilated and the control runs.

-p.2689 : Why does the assimilation start in 1987 for ORCA 0.25 and in 2002 for ORCA1 ? The authors mention that changes in the high latitudes have time to influence the AMOC. Do you actually see these changes ? It would be interesting to describe them as the AMOC might be more adjusted than in the low resolution experiments. The results that are described for the high resolution model are broadly the same than those of the 1° model. This subsection could thus be omitted and the results only discussed in the last section. Also, the effects of the resolution might be larger than those described if we allowed the ocean to adjust, as the deep western boundary current is I guess better resolved in the eddy-permitting model. If this subsection is kept in the final manuscript, the authors should discuss how the improvement of the model with resolution is expected to affect the assimilation.

-p.2704 : The number of tables and figures is consequent. I suggest to combine Fig.1 and 2 and to refer to C07 for more details.

-p.2707, Fig.5 : Can you comment on the differences between RAPID and WOA ? What are the error bars ?

p.2712, Fig.10 : The influence of initial conditions on the overturning in the assimilation run is not clearly described. Are the conclusions on the sensitivity to initial conditions similar with and without assimilation ?

-p.2713 : Fig. 11 could be omitted and the results just discussed.

-p.2714, Fig 12 : It does not appear essential to show all the components of the AMOC. I suggest to keep only panel b) which shows the overturning, and describe panel a) and

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c) in the text without showing the figures as the results are similar to those of the low resolution model.

-p.2690 : The authors should add in the discussion that the results of the present study might be model dependent. A coupled assimilation where atmospheric feedbacks would be allowed should also lead to different and better results.

-p.2691, l.12-13 : I agree that full depth assimilations are needed to accurately represent the AMOC, but I do not believe the present study provides convincing results on that issue because of the short time scales considered.

Interactive comment on Ocean Sci. Discuss., 6, 2667, 2009.