

## ***Interactive comment on “Observed and simulated estimates of the meridional overturning circulation at 26.5° N in the Atlantic” by J. Baehr et al.***

**J. Baehr et al.**

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*Thank you very much for your constructive review. We reply to each of the comments below.*

### COMMENTS:

1. In the 4th point in the conclusion section the authors note that there is little temporal correlation between the ECHAM5/MPI-OM solution and the observed MOC. As the authors noted in a previous section, this is completely expected, since the ECHAM5 solution is a free running coupled model, and there should be no correlation between the time sequence of events on that simulation and observations. I strongly suggest that this bullet conclusion be deleted (or at least modify it to remove the ECHAM5 state-

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ment), as should the corresponding sections of the main text that present and discuss this point. Given this issue, please clarify why there is utility in using the ECHAM5 results in the Taylor diagram in Figure 8. Perhaps I have missed the point.

*Indeed, it is expected that we find little correlation between the coupled model MOC and the observed MOC. This seems obvious for those who frequently work with coupled models. We still think that it is worth making this statement, as in our experience, it's sometimes a new thought for those who don't work with coupled models frequently. For the same reason the coupled model results are included in the Taylor diagram, as it illustrates the small correlations (for all components and for the entire ensemble).*

*Not least, we also think that it is appropriate to consistently summarize (and contrast) the results for both models in the conclusions.*

*Therefore, we have kept the statements in both the text and the conclusions.*

2. On p. 1350, lines 10-13, it is stated that ": : : the correspondence between the level of variability in MOCHA/RAPID and ECHAM5/MPI-OM increases the confidence in the estimates of detection times for MOC changes gained from such a model". I am confused by this statement. The time scales for MOC changes induced by radiative forcing changes (which I assume is what is referred to for detection times) are of the order of decades. In order to have better confidence in the ability of models to detect such changes, we would need to have confidence in the models simulation of variability on decadal time scales, not on intraseasonal time scales as analyzed here. I suggest that this statement be deleted - I do not think the analysis presented here supports that in any way. If I am wrong in this assessment please add text to clarify.

*With 'detection times' we refer to changes in the MOC on any timescale, but particularly interannual to decadal timescales. Further, we do not attribute any cause for these potential changes. What we tried to express here is that the simulation and in turn detection of long-term (e.g., decadal) MOC changes is more likely to be close to*

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*the observations when the short-term (e.g., intra-seasonal) variability is appropriately simulated in the model than not.*

*We have modified the sentence: 'Within the limitations of the currently available observations, the correspondence between the level of variability in RAPID/MOCHA and ECHAM5/MPI-OM increases the confidence in the estimates of the period it takes to detect a change in the MOC based on such a model'*

3. For Figure 2 it would be useful to add panels showing the differences in the profiles, since these differences are discussed in the text but have to be inferred by visually comparing these profiles.

*Such panels would contain exactly the same information as already plotted. The only gain of such additional panels would be an easier reading of the magnitude of the differences. These numbers, however, we do provide in the text. As figure 2 has already eight panels we think additional eight panels would not aid clarity here.*

4. Since there is extensive discussion of the differences between the two models and the observations in terms of the profiles of temperature and salinity, it would be useful to comment on hypotheses (if they exist) on the reasons for the model biases.

*On the reasons for these model biases, we can only speculate. In both models, the parameterization of the mixing is likely to be erroneous. Additionally, the surface fluxes in the coupled model, and the forcing fields used in the assimilation introduces large uncertainties.*

5. The hydrographic characteristics at 26.5N are better simulated in ECCO than in ECHAM5, and yet the time-mean of the simulated MOC is better in ECHAM5 than in ECCO (bullet point 2 in the conclusions). I did not see a discussion of this, but it would

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be useful.

*In the coupled model, there seems to be a fortunate cancellation of deficiencies in the simulation of temperature and salinity. The resulting zonal density gradient is actually stronger than suggested by the observations at intermediate waters, while weaker below 2000 m. This in turn results in comparatively strong northward and southward transports, i.e., a strong MOC.*

*In ECCO, deficiencies mostly occur in the intermediate waters at the western boundary. Some of these deficiencies are canceled. Although the resulting zonal density gradient has a similar sign than the observations, it is weaker at intermediate waters and stronger below 2000m than the observations suggest. This in turn results in comparatively weak northward and southward transports, i.e., a weak MOC.*

Figures

- Figure 4 ... why is just one realization used for ECHAM5?

*With more than one realization, the individual lines are hardly distinguishable and the figures are too crowded. However, the Taylor diagram (figure 8) and the tables give the range of variability between the different realizations.*

- Figures 5 Need labels on figures for which is a,b,c,d as used in caption.

*Labels are placed in the lower left corner (as there is no space in the top left corner).*

Typos

- p. 1340, line 7 "were" should be "where": *corrected.*

- p. 1341, line 16 "availably" should be "available": *corrected.*

- p. 1346, lines 14-15 Confusing grammar; please clarify: *corrected.*

- p. 1347, line 15 Should "and" be "are"?: *corrected.*

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