

Interactive comment on “Frequency-dependent effects of the Atlantic meridional overturning on the tropical Pacific Ocean” by L. A. te Raa et al.

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This paper investigates the response of Tropical Pacific to unforced variations in the Atlantic MOC and to a forced shutdown of the Atlantic MOC. In the first case no significant response is identified, while in the second case a significant response is found. The latter apparently occurs when the MOC crosses a certain threshold. The results are interesting, well presented, and have implications for understanding AMO teleconnections. I recommend publication subject to some minor revisions (listed below). I hope that the authors find my comments useful.

Noel Keenlyside

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- It is claimed that the Tropical Pacific response to AMOC fluctuations is frequency and amplitude dependent. I would argue that neither of these is really shown. What is shown is that the Tropical Pacific response is insignificant to low amplitude relatively high-frequency variations in the AMOC, but significant for a sustained weakening of the AMOC. I believe this would be a more reasonable statement to make in the abstract. However, I would recommend that the authors explain in a little more detail how and why the two responses differ in the North Tropical Atlantic. For instance, following the paper of Chang et al. 2008, it would be instructive to show that the northern branch of the Atlantic STC becomes visible at the point that the SST in the Caribbean change. If this is the reason for the nonlinear behaviour, then it could also depend on the strength of the STC (associated with the surface winds) and hence would be quite model dependent. The statements on frequency are a little harder to justify, and so without further experiments (or appropriate analysis) I feel it is better to make clear the statement is speculative.
- I would like to see some discussion on the realism of the unforced AMOC variability. During the 20C, the AMO has had an expression in the North Tropical Atlantic, and this has been associated with shifts in the ITCZ (affecting Sahel rainfall and Atlantic Hurricane activity). It is not clear how closely the SST in this region are related to changes at higher latitudes, and whether other factors have contributed to SST changes in this region. Nonetheless, there are studies (e.g., Zhang, GRL, 2008, Coherent surface-subsurface fingerprint of the Atlantic meridional overturning circulation) that indicate the changes in this region during the 20C are AMOC driven. As in the observations the SST change does not seem so non-linear, it calls into question the realism of the model. Thus, I would like to see some discussion on these aspects, as they are a potential caveat on the

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results.

Minor points

1. Section 1, pg. 479, Lines 16-18, Although we may infer that natural AMOC variations, for example from SST, are smaller than the potentially large changes resulting from a shutdown of the AMOC, there are insufficient AMOC observations to say this. Please modify the sentence accordingly.
2. Section 2, pg. 480, L 10, A more appropriate reference to Keenlyside et al. 2005 would be Jungclaus, J.H., N.S. Keenlyside, M. Botzet, H. Haak, J.-J. Luo, M. Latif, J. Marotzke, U. Mikolajewicz, and E. Roeckner, 2006: Ocean circulation and Tropical Variability in the Coupled Model ECHAM5/MPI-OM. J. Climate, 19 (16), 3952-3972. This model version was closer to the that used in the AR4. Also, Oldenborgh et al. 2005 is not listed in the references
3. Sec. 2, pg. 480, L 12. I believe the atmospheric model has 31 levels.
4. Section 3, pg 481, L 3, This AMO definition is not the most common one, and not that used in the cited references. Thus, it would be useful to indicate the observed SST ranges for this index.
5. Sec. 3, pg. 481, L 2, it would be useful to explicitly state that removing the ensemble mean leaves the unforced variations, which are of interest here. It might be useful to add this information in sec. 2.
6. Section 3, pg 481, L8-10, I assume the period of 20 years was obtained from spectral analysis. Please indicate on what this statement is based, as only one ensemble member is shown in fig 1a. Also, I would qualify the statement about the observed AMO periodicity, to indicate this is an estimate based on the short

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- instrumental record. From this we still can't say if there is a definite periodicity in the AMO.
7. Section 3, Pg. 481, L10. A better reference for Keenlyside et al. 2008, would be Jungclaus et al. 2005, as this study looks specifically at AMO like variability. However, the variability in this model was somewhat longer (70-80yrs).
 8. Sec. 3, pg. 482, L1-9, The statements about the NAO link are not well supported. The SST pattern in the North Atlantic does not appear to resemble the NAO tripole. Furthermore, it was my understanding that there was a tendency for El Niño to be associated with negative NAO in observations (Brönniman, Rev. of Geophys., 2006) and in some models, and in particular the MPI model (Mueller and Roeckner, GRL, 2006). Thus, I am not sure how why the correlation should be positive, and why the authors claim so strongly that this simply a model artefact. This is not an important aspect of the paper, but the statements are not justified and appear somewhat in contradiction to the literature.
 9. Sec. 3, pg 483, L 11, I think you refer to the publication Wu et al. J Clim, 2008 Global Teleconnections in Response to a Shutdown of the Atlantic Meridional Overturning Circulation.

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