

## ***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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Dear Noel Keenlyside,

thank you for your useful comments. We have addressed your comments as follows.

### **Main concerns**

1. *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 state-*

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*ment 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.*

The difference between the natural variability and sustained weakening is in the frequency and amplitude, so either of these must cause the difference. We adjusted the title to reflect better that we cannot distinguish between the two.

In further analysing our model data, we found similar results as reported in Chang et al (2008). Indeed the changes in the Caribbean are almost simultaneous with the reversal of the North Brazil current, which in this model occurs after 10 to 15 years. Figures of the northwards transport of the subtropical cell between 100 m and 400 m and of equatorial Atlantic SST in JAS have been added to the paper (Fig. 7).

The mechanism described by Chang et al is not instantaneous, it takes time to reverse the direction of the boundary current. From their results it is not clear whether the time for the signal to reach the Caribbean or the strength of the signal is the main factor in the time delay. As you remark, all these factors are most likely model-dependent as well. Lagged SST plots (added to the paper as Fig. 8) suggest that advection with the gyre can also explain part of the time delay. Combined with the clearly frequency-dependent differences between the interannual variability and decadal-scale variability we decided to keep the wording in the abstract as is.

2. *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 finger print of the Atlantic meridional over turning 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 results.*

We could not reproduce the results of Zhang et al (2008) in the observations after subtracting a pointwise regression against global mean temperature. In the SODA reanalysis the first EOF of heat content to 750 m is in quadrature with the AMO index, with very low simultaneous correlations but lag correlations of  $r = 0.9$  at 6–7 years. As the attached figure shows, the model reproduces the EOF well, but the lagged correlations with the AMO index show a somewhat faster time scale and lower correlations ( $r = 0.3$  at  $\pm 5$  years), although it is hard to be definitive given the large uncertainties in the observations. However interesting these results are, we do not think they fit into the subject of the current paper. We therefore did not add this diagnostic to the article.

The SST and land temperature plots look very similar to the observed ones, except that the extension to the subtropics is absent in the model. This may be the cause that the Sahel rainfall is not affected by the AMO in this model (see also Biasutti et al, J. Clim., 2006). Within the large uncertainties the AMO connection to the tropical Pacific SST is well reproduced. The teleconnections to land 2m temperature are also very similar to the observed ones, with a coherent

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signal only in the eastern half of the US, with correlations of  $r \approx 0.3$ . Plots of correlations of the AMO with T2m and precipitation have been added, both for the ESSENCE ensemble and the observations.

## 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.*

We added a distinction between modelled and observed results. In the models this statement is certainly true, hence we added a qualifier ‘modelled’.

Secondly, the amplitude of low-pass filtered AMO index variability over the last 150 years is of the order of 0.2 K in the observations and 0.3 K in the MPI model (after subtraction of the global warming signal or ensemble mean). The cooling in the AMO index induced by a collapse of the AMOC is 2 K in the model. Estimates of the cooling in the Younger Dryas are compatible with this. Based on the almost order of magnitude difference between the natural variability over the last 150 years and a shut-down in the AMO, we think it would be safe to deduce that the AMOC has similar properties in the observations. We added a sentence to indicate that this is deduced from the AMO, not from direct AMOC observations, which are unavailable.

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.*

We changed the reference. Van Oldenborgh et al (2005) is listed under 'v'.

3. *Sec. 2, pg. 480, L 12. I believe the atmospheric model has 31 levels.*

Thank you for pointing out this typo.

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.*

We have added two paragraphs on the difference between our AMO index and the ones used in previous publications: the southern extend (25° N vs the equator) and the subtraction of the ensemble mean or global mean temperature. Our index is very similar to the one employed by Trenberth and Shea (GRL, 2006).

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.*

This has been emphasised in the paragraphs mentioned above, and a sentence has been added to section 2 mentioning this.

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 instrumental record. From this we still can't say if there is a definite periodicity in the AMO.*

This part of the paper has been extended. The spectra of the modelled and observed AMO have been added to Fig. 1, and the uncertainty in the observed spectrum is emphasised in the text.

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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).*

Added this reference. We think the reason we do not see a strong peak at this period may be that we only have 150-yr transient runs, although this should be just enough to pick it up. Maybe the warming trend interferes with the Arctic excitation mechanism described in Jungclaus et al (2005).

8. *Comment 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.*

The correlation between El Niño and winter temperature in Europe described by Brönnimann are very weak ( $r < 0.15$ ). In the observations, the maximum 3-month correlation between the Iceland-Azores NAO index and Niño3.4 occurs in Jan–Mar with a barely significant  $r = -0.19$  and Jul–Sep with  $-0.22$  over 138 years, all other seasons have lower correlations. The annual mean correlation is around  $-0.15$ , depending on the details like the starting month.

These numbers are much lower than the modelled teleconnection, hence we consider this a model artefact. We rephrased this section to mention that the observed correlations are all below 0.2, and cited Brönnimann and an earlier investigation with negative results.

9. *Sec. 3, pg 483, L 11, I think you refer to the publication Wu et al. J Clim, 2008*

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*Global Teleconnections in Response to a Shutdown of the Atlantic Meridional Overturning Circulation.*

Thank you for correcting this error.

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Interactive comment on Ocean Sci. Discuss., 6, 477, 2009.

**OSD**

6, C165–C172, 2009

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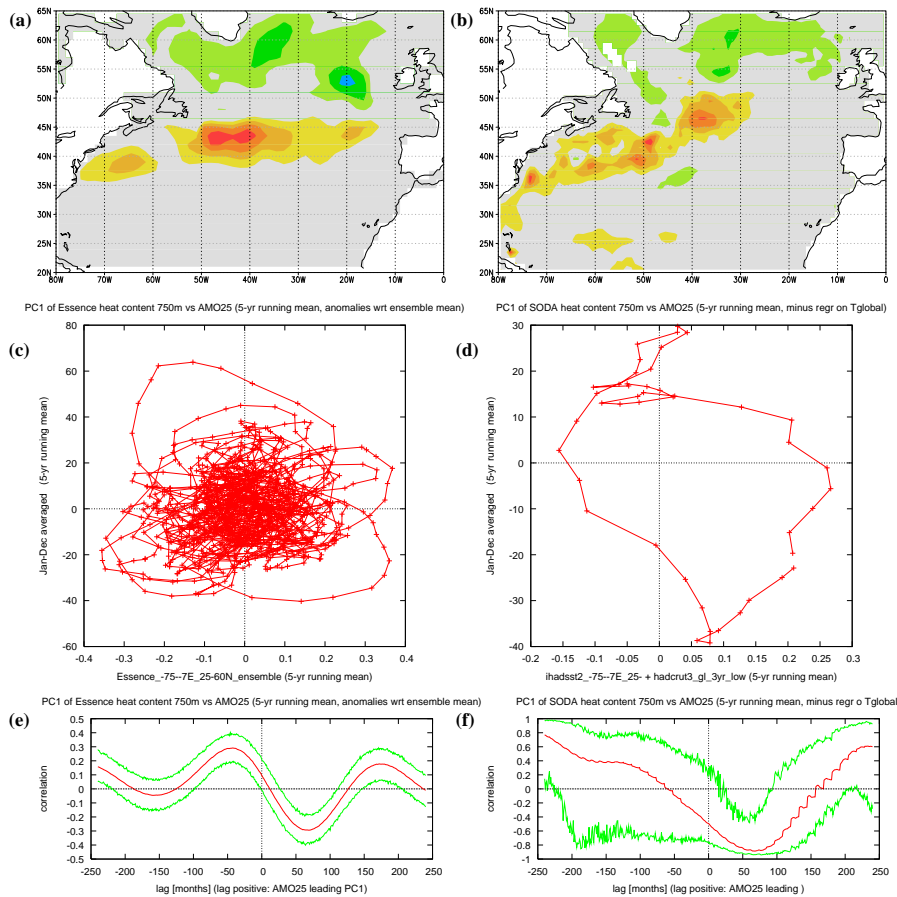


Fig. 1.

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