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Climate of the Past Discussions

Interactive comment on "Variations of the Atlantic meridional overturning circulation in control and transient simulations of the last millennium" *by* D. Hofer et al.

Anonymous Referee #2

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Review of "variations of the Atlantic meridional overturning circulation in control and transient simulations of the last millennium" by D. Hofer, C. C. Raible and T. F. Stocker

This paper investigates the AMOC variability in control simulations and transient simulations of the last millennium. It makes use of an original method to detect variations of the AMOC and compare transition types and time scales under both conditions. Major result is the evidence of a specific transition which probability of occurrence is enhanced under historical forcing and which has important climatic consequences for the Scandinavian climate. The paper is generally well written and results are well presented. However, the modes of AMOC variability and the resulting climatic anomalies

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are not sufficiently related to existing literature both related to decadal and multidecadal variability of the AMOC and to results from proxies of the last millennium. This lack greatly reduces the reach of the paper and thus its general interest for the community. This is the major reason for me to advise major revisions for this manuscript before possible publications in Climate of the Past. I detail this points and others below.

1. Several studies have already focused on AMOC modes of variability. Here, the authors propose a novel approach based on transitions rather than modes of variability. In order to validate this method, and also in terms of scientific approach, the composites and transition types detected in 3.2 (Fig. 6) would need to compared to this literature. This literature may include e.g. Eden and Willebrand 2001, Mignot and Frankignoul 2005 Clim. Dyn., Danabasoglu 2008 J. Clim., (same model!), Frankcombe et al. 2010 (again!)... More specifically, the high frequency composite looks like the interannual adjustement to atmospheric variability, namely the NAO. Types 2 and 3 resemble more to the decadal mode of variability of the AMOC generally found to be associated to deep convection (the 70yrs time scale of Frankcombe). This is consistent with the MLD analysis of section 3.4.1, but should be more clearly discussed somewhere in the paper. What is the link of type 2 and type 3 transitions with such mode of variability?

2.Most of the paper then focuses on type 3 transition. By eye in Fig. 6, it is not very far from the other two. What are the correlations among the patterns? Could the type 3 transition be found by any other statistical method? Why or why not?

In other words regarding the points above, the detection and distinction of transition types is original but not fully convincing in its present form. It is a bit presumptuous in my view to distinguish among different modes of variability solely based on a statistical detection of transition rates and without discussing physical mechanisms.

4.I find it difficult to really realize which are the transition of type 3 on the different time series. In this respect, Fig. 8 is very instructive and the AMOC behavior in this figure should perhaps be discussed much earlier, at the beginning of section 3.4. Concerning

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Fig. 9, the text states that “the temperature response due to the AMOC transitions can be recognized” (p. 1285 l. 11). This is not clear to me at all. Can you find a way to indicate the specific transitions you are talking about on this figure? And perhaps reproduce again the AMOC? At least, please avoid that the legend masks some parts of the time series!

5. The conclusions and abstract are sometimes going a bit too far: (i) The statistics given at the end of section 3.2 highlight their rarity, even the transient simulation (5 occurrence over 3000 years). This point should be better pointed out in the abstract and conclusions. (ii) p. 1287 l. 4: the “realistic climate evolution” was never checked in this study, nor did you explicitly refer to a study showing this. (iii) p. 1287 l. 7-8: the effect of aerosol forcing (or non-forcing) on the AMOC variability was not checked... please rephrase these points.

Specific points

p.1269 l. 11: “Furthermore”: no clear link with the previous sentence. Please rephrase.

p.1269 l.11: “existing”: omit

p.1269 l.15-20: I would advise to refer to this recent paper :North Atlantic Multidecadal Climate Variability: An Investigation of Dominant Time Scales and Processes, by Frankcombe et al. Journal of Climate, Vol 23, pp. 3626-3638 doi: 10.1175/2010JCLI3471.1

p.1275 l. 4: I would advise to refer to this recent paper: Servonnat et al. 2010 Influence of solar variability, CO2 and orbital forcing between 1000 and 1850 AD in the IPSLCM4 model, Clim. Past, 6, 445-460, 2010 (I recognize that both these papers were hardly published at the time of submission)

p. 1275-1276: “anomalies from the long term mean”: what do you mean exactly? If it is just anomalies from the average over the full simulation length, the drift that was problematic a few lines before should remain here...? Please explain. The argument is

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probably simply that the study focuses on multidecadal (and not longer) variability.

The description of Fig. 4 is not very convincing if you do not specify the “prolonged periods of strong or weak overturning” you are thinking of in the transient simulations. In Tra1, Tra2, Tra3, Trb1, I rather see a long term drift.

p.1277 l1-6: the protocol is not perfectly clear to me: what does it mean to look for a level lasting at least 20yrs when a running mean window of 51 to 201 yrs has been applied?

p.1280 l. 18-20: Here again, the protocol is not fully clear to me: during “the strong AMOC periods”, the AMOC mean value can still be different, isn’t it?

p.1280 l.22-23: “clockwise / anticlockwise” is not physical vocabulary and depend on your own convention. Please use “anticyclonic” and “cyclonic” instead.

p. 1282 l.9 “the deep” replace by “depth” or “the deep ocean”.

p.1290 l. 4 reference to Brocker 2000 and Denton and Brocker 2008 does not need to be repeated here.

The comparison to available paleoproxies is limited to the last few lines of the manuscript. Of course, I recognize that it is difficult to find estimations of the AMOC or oceanic data variations over this period with decadal resolution, is it possible to try to detect such transitions in atmospheric reconstructions, or available SST reconstructions such as Sicre et al. 2008?

Interactive comment on Clim. Past Discuss., 6, 1267, 2010.