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Ocean Science
Discussions

# *Interactive comment on* "North Atlantic 20th century multidecadal variability in coupled climate models: sea surface temperature and ocean overturning circulation" *by* I. Medhaug and T. Furevik

**Anonymous Referee #1**

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The manuscript presents an analysis of the output of a suite of IPCC-class climate models, focusing on multidecadal variability in sea surface temperature and meridional overturning strength in the North Atlantic. A link is found between SSTs in the North Atlantic and the strength of the MOC.

There seems, at first, to be significant overlap between the topic of this manuscript and that of Knight (2009). It would be helpful to add a sentence or two to the introduction describing how this manuscript enlarges on earlier works. Otherwise it is not clear what makes another study of North Atlantic multidecadal variability in a suite of IPCC-class



models worth reading.

There has been some discussion on the exact period of the AMO, whether it is in the 40-80 year range or whether shorter periods (20-40 years) are also seen. It would be interesting to include some discussion on whether any of the models show variability on different time scales. Does the 15 year window used to filter your data mask the presence of shorter period variability? Another comment in this vein is that the runs used are not really long enough to establish definite periods. Have the authors considered using control runs or more simulations from each model, where available?

The comment is made that the interannual-decadal power maximum in observations is likely due to imprint of the NAO on the SST since the NAO also shows power at the same timescales. In the absence of a reason for the NAO to have an intrinsic timescale, could the reverse not also be true?

I found the section on the surface response to AMOC variability to be the most interesting part of the manuscript. However, in the discussion section there is mention of some models showing more sea ice for stronger AMOC. It would be less confusing if this was mentioned more clearly in section 3.3 before being discussed in section 4.

What would really make the paper worth reading would be a more thorough comparison of these model results with the various hypotheses that have been put forward to explain the AMO. While many of these do link MOC strength to the AMO, there are also various other physical processes which may be included (some of which are mentioned in the manuscript) and which could be examined, such as density fluctuations in convection regions, advection of anomalous dense water from the south, variations in wind forcing, export of sea-ice/fresh water from the Artic, etc. These hypotheses have, in general, been studied only in individual climate models so it would be interesting to see if the same effects are found in other models as well.

Page 356, line 10: The ä in Häkkinen is missing.

Page 356, lines 12-14: Hasselmann (1976) theorized that the AMO is a damped response to atmospheric forcing, Frankcombe et al. (2009) said that the AMO is a damped ocean-only mode, excited by atmospheric forcing.

Reconsideration after major revisions is recommended.

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Interactive comment on Ocean Sci. Discuss., 8, 353, 2011.