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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

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First of all we would like to say thank you very much for your constructive comments. In the following referee comments are shown in slanted text and the response in bold.

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.
This has been addressed in the second last paragraph of the introduction, by adding the sentence "Where earlier studies of AMO or AMOC have been restricted to single models or a subset of the available climate models, ..." at the beginning.

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.
Based on observations AMO has a time scale of ∼60 years (Kerr 2000). But the models show energy on other time scales as well. The sentence "Many models show more energy at 10-30 years time scales than observations." has been added to section 3.1, paragraph 4.

Does the 15 year window used to filter your data mask the presence of shorter period variability?
There has not been used any filter in the frequency analysis, so variability on these time scales will appear in the power spectrum. For all figures presenting time series, the use of filter does of course remove shorter time 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?
Yes, we have considered to use the longer control simulations of the climate models. But the main focus has here been on the models ability to simulate the 20th century and the relationships between AMO and AMOC in this century, and

not on the periodicities. The shorter 20th century simulations has therefore been used. Also, since AMO/AMOC might partly be externally forced, the frequencies in the control and the fully forced simulations does not have to be the same (e.g., Otterå et al. 2010)

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?

On these time scales it is shown that the atmosphere forces the SSTs. It is, however, fully possible that the ocean feeds back on this signal. This has been clarified in the manuscript by adding the text in section 3.1, paragraph 4: "Several studies have suggested that the ocean also feeds back on the atmosphere at these time scales (Kushnir 1994; Marshall et al. 2001)"

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.

This has now been highlighted in the manuscript. The text is changed to: "..., while the results are not conclusive for a strong AMOC, since some models actually show more sea ice for a stronger AMOC. In the Labrador Sea the models show diverging results, as some models show largest sea ice extent for strong AMOC, others for weak."

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.

Yes, it would be very interesting to go into these various mechanisms. However, only 4 modelling groups have contributed enough data to the CMIP3 database to make it possible to identify the convective regions (mixed layer depth), and only one of these models are among the models we have identified to have a realistic overturning strength. Since we cannot identify the convective regions, it would be difficult to draw any conclusions of potential mechanisms. The conclusion was therefore that this was not feasible. In this study we therefore only focused on the potential contribution of the AMOC strength on the sea surface temperatures.

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

This has been corrected in the new version of the manuscript.

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.

The paragraph has been rewritten for clarification, and changed according to the reviewer's comment. The text has been changed to: At present there is no consensus to what degree the AMOC variability is an ocean-only mode excited by (Frankcombe et al., 2009) or damped by (Hasselmann, 1976; Frankignoul et al., 1997) atmospheric forcing; an ocean-only mode with density fluctuations in the convection regions driven by advection of density anomalies from the south (e.g., Vellinga and Wu, 2004) or the northern high latitudes (e.g., Delworth et al., 1993); a fully coupled atmosphere-ocean or atmosphere-sea ice-ocean mode with the deep water formation rate dominated by variations in the local wind forcing (e.g., Dickson et al., 1996; Häkkinen, 1999; Eden and Willebrand, 2001; Deshayes and Frankignoul, 2008; Msadek and Frankignoul, 2009; Medhaug et

al., 2011).

Reconsideration after major revisions is recommended.

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