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Climate of the Past Discussions

Interactive comment on "Using simulations of the last millennium to understand climate variability seen in paleo-observations: similar variation of Iceland-Scotland overflow strength and Atlantic Multidecadal Oscillation" *by* K. Lohmann et al.

Anonymous Referee #2

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This manuscript tests 2 mechanistic hypotheses that could explain a link between Iceland Scotland Overflow (ISO) and the Atlantic Multidecadal Oscillation (AMO) in historical runs performed with 3 different climate models participating in the EU Programme THOR. The link was suggested by paleo-reconstructions.

The set-up of this project is sympathetic and deserves encouragement. The final results are somewhat disappointing, i.e., not very conclusive, maybe because of model biases, but we should not blame the authors for that. Nevertheless, I have some problems with the write-up, analysis and discussion and recommend minor/major revision

(halfway between minor and major).

My main problem with the present ms is that it is too lengthy, full of details and discursions that not always seem relevant and difficult to follow for the reader. It lacks clarity and a main line of reasoning is often missing or obscure to detect. I recommend a major revision of the write-up, without the need to include significant new analyses.

2 Mechanisms are discussed. Mechanism3.1 may work in the real world, but it is already preluded on page 3265 lines 9-13 that it probably won't work in the models, because of biases in flow path. Section 3.1 can be shortened considerably. After seeing the 3 right panels of fig. 6 we know enough. We don't need Fig. 5 and its discussion. If the MOC-AMO link is not there, the ISO-MOC-AMO link does not exist, regardless whether the link ISO-MOC exists or not.

Also section 3.2 is too long. It should start with explain the link between AMO and ISO. In the first par of this section the explanation is unclear and the sentence seems grammatically incorrect. It is better explained at the end, page 3277, lines 16-24. If the pressure gradient across the ridge is instrumental the discussion should focus on this aspect, discussion about MOC and SPG are distracting. Also, the authors should focus on lag-0; there is no physical process involved that could motivate a lagged-correlation, even if in practice correlations maybe somewhat higher at certain lags (by chance?). A link with AMO exists if pressure variations are dominated by thermal density anomalies with an equivalent barotropic character, that is heat content should well correlate with SST. In addition the impact of salinity anomalies should be weak or these should also well correlate with SST. This is all we need to know.

Section 4 is also too long. A hindcast run with MPI-ESM is discussed as "ground-truth" replacing the too short observational database. I don't buy this and suggest removing this part. The authors also discuss why correlations between ISO and AMO are lower in BCM than in the other 2 models. Again, the discussion is too long; too many elements are entrained in the discussion and no firm conclusion is reached. It seems tome that the clue for the anomalous behaviour of BCM is presented on the par starting at page 3274 line 19; ending on page 3275, line 4.

In short:

I suggest deleting 2, 5, and 10. Figs 7-9 should be condensed; at present, readers (and authors) drown in correlation coefficients and lags. Focus on the main results here. A final schematic might be helpful. For mechanism 3.2 discussing the relation with MOC and SPG are distracting and should be avoided. Especially the westward retreat of the SPG was unclear from the figs, and how it contributed to the interpretation was even more unclear. In sec 4, a short link with MOC/SPG can be made when explaining the difference between BCM and the other 2 models in addition to differences in sea-ice melting.

Interactive comment on Clim. Past Discuss., 10, 3255, 2014.