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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.

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The authors wish to thank the reviewer for a very detailed review and helpful comments which have improved the manuscript! Please note that a short discussion of the cold events / very weak overflow related to periods of strong volcanic forcing is included in the revised manuscript in response to the comments of the first reviewer. Please also note that Figure numbers in the revised manuscript do not match Figure numbers in the submitted manuscript as three Figures are removed and one new Figure is added. The revised manuscript is added as supplementary.

Referee comment:

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.

Response:

In the revised manuscript, we remove Figure 5 of the submitted manuscript (correlation ISO-MOC) and its discussion in the text. The order of the remaining text in section 3.1 is changed (first discussion of correlation AMO-SST, then of correlation MOC-SST) in order to start the discussion of the 'ISO-MOC-AMO link' from the 'AMO end' rather than the 'ISO end'.

Referee comment:

Also section 3.2 is too long. It should start with explain the link between AMO and ISO. In the first part 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.

Response:

In the revised manuscript, we rewrite the explanation of the 2nd mechanism at the beginning of section 3.2: "According to the literature (e.g. Hansen and Østerhus, 2007;

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Jungclaus et al., 2008; Olsen et al., 2008; Sandø et al., 2012), the Iceland-Scotland overflow strength is affected by the pressure gradient across the ISR in the core depth of the overflow. Mechanism (ii) implies an in-phase variation of Iceland-Scotland overflow strength and AMO index due to the influence of the Nordic Seas surface state and density structure, which are positively correlated with the AMO index (left panels in Figure 6), on the pressure gradient across the ISR."

In the revised manuscript, we shorten the rather long discussion of what causes the Nordic Seas SST and SSS anomalies related to the ISO anomalies to one paragraph. The discussion about the influence of MOC and SPG on the Nordic Seas SST and SSS anomalies is removed.

In the revised manuscript, we use zero-lag correlation between ISO and the various oceanic fields for all three models.

In the revised manuscript, we include the correlation between ISO and Nordic Seas heat / salt content in the discussion of the 2nd mechanism (not in the Figures as the correlation patterns for heat / salt content resemble those for SST/SSS). The correlation between ISO and respectively atmospheric fields, convection and oceanic circulation is removed both from Figures and text.

In the revised manuscript, we limit the discussion for BCM to the eastern part of the Nordic Seas (similar to MPI-ESM). The influence of the wind stress (as one possible explanation for the less clear in-phase variation of ISO and AMO in BCM) is moved to section 4 (Discussion).

Referee comment:

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

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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.

Response:

In the revised manuscript, we remove Figure 10 of the submitted manuscript (hindcast run) and its discussion in the text.

In the revised manuscript, we shorten the discussion of the differences among models. Regarding the sea ice melting in BCM (to which the reviewer refers to in the last sentence of the comment above), the surface fresh water flux does not contribute to the SSS anomalies related to the ISO anomalies in the western part of the Nordic Seas in BCM, in contrast to the other two models. In the revised manuscript, we limit the discussion to the eastern part of the Nordic Seas and identify the influence of the wind stress on the ISO strength as the main difference between BCM and the other two models.

Referee comment:

In short: I suggest deleting Figs 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.

Response:

In the revised manuscript, we remove Figure 2 of the submitted manuscript (overflow transport across ISR and downstream near-bottom velocity) and its discussion in the

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text.

See response to comments above.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/10/C2081/2014/cpd-10-C2081-2014-supplement.pdf

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