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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 Flavio Lehner for a very detailed review and helpful comments which have improved the manuscript! To facilitate our response, we have changed the order of the comments (but kept the original comment numbering). Please note that the revised manuscript is considerably shortened in response to the comments of the second 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.

Discuss cold events / very weak overflow during periods of strong volcanic forcing:

10) P3265L17ff: As the correlation over the whole time period seems to be influenced considerably by the volcanic forcing, I encourage the authors to investigate the temporal stability of this correlation, for example by doing a running window correlation and discuss forced and unforced periods separately.

14) P3269L22ff: this would again be an occasion where a running window correlation could potentially help to disentangle forced and unforced behavior.

20) P3279L18ff: given these results I am again surprised that the strong ISo events are not discussed specifically with respect to volcanic forcing or external forcing in general. Also, a discussion to what extend the models are supposed to reproduce variations in AMO and ISo as reconstructed is absent. Such a discussion would give the paper much more relevance as it has – by making the paper more than a sole model study – the potential to attract the interest of the proxy community. I think this is particularly important as the authors refrain from diving deeper into the mechanisms explaining variability of ISo (P3282L25ff: "A more detailed understanding of the mechanisms explaining the variability of the Iceland-Scotland overflow strength in the three models is beyond the scope of our study"). I think the authors need to expand on either the link to specific events in the proxy data or expand on the mechanisms beyond the statistical analysis.

Technical 7) Figs. 2-4: could you include some indication of the volcanic and solar forcing timeseries used in the models?

Response:

In the revised manuscript, we have indicated years with major volcanic eruptions in the Figures showing time series of Iceland-Scotland overflow strength and/or AMO index. A running correlation between Iceland-Scotland overflow strength and AMO index is

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added to the Figure showing the two time series (new Figure 3). The cold events in the AMO index / very weak Iceland-Scotland overflow and the running correlation are shortly discussed in section 2.2 (ISO and AMO in simulations).

Apart from the correlation analysis discussed in the submitted manuscript, we have performed a composite analysis with respect to the cold events / very weak overflow following the major volcanic eruptions in years 1258 and 1815 AD. A Figure showing the composite pattern for North Atlantic SST (new Figure 5) and a short discussion about the AMO-MOC link during the cold events are included at the end of section 3.1 in the revised manuscript. The composite pattern for the various oceanic quantities (discussed in the submitted manuscript regarding the link between the Nordic Seas surface state and the Iceland-Scotland overflow strength) closely resemble the correlation pattern and are therefore not shown in the revised manuscript. A short discussion about the link between the Nordic Seas surface state and the Iceland-Scotland overflow strength during the very weak overflow is included at the end of section 3.2 in the revised manuscript.

Lead/Lag relations:

1) The paper relies heavily on the correlation analysis and identifies different leads and lags between quantities, however, these are finally not addressed in a comprehensive manner, potentially leaving the reader confused. For example, the covariation of AMO and ISo is described as in-phase, i.e., with zero-lag, with the AMO being dominated by low-latitude SSTs. However, at the same time the ISo is found to follow the Nordic Seas SST by 0, 2, and 9 years in the three models, suggesting the Nordic Seas surface state is driving ISo. Then again, the Nordic Seas SST are influenced by the heat transport across the Iceland-Scotland Ridge (ISR), which is related to changes in the Subpolar Gyre (SPG) and potentially changes in the AMO. So how can the original link between AMO and ISo be more or less instantaneous when there are considerable lags involved in all the processes listed here? A suggestion to help the reader: illustrate the process chain by a flow scheme in which you indicate the leads and lags (for the different

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models).

2) Following from 1) there it would be interesting for the modeling community to learn more about the reasons for the different lag times in the different models. Simply referring to Langehaug et al., 2012b is hardly enough (P3272).

15) P3271L19ff: I assume that the correlations here and following later apply the same lags as found for Fig. 7a, 8a, 9a? Please clarify.

19) P3276L19ff: recommend to replace 'associated with' with expressions that make clear what causes what and how leads and lags come to play (flow scheme).

Response:

Also the second reviewer is concerned about confusing the reader with the different lags. He suggests to use zero-lag correlation only. The correlation pattern between Iceland-Scotland overflow strength and various oceanic quantities representing a lead/lag of a couple of years are rather similar compared to the zero-lag correlation pattern (due to the 21-year running mean filter applied to the data prior to the analysis). E.g. the correlation pattern for IPSLCM4 in the discussion paper (Figure 8) representing a lag of 2 years are indistinguishable from the zero-lag correlation pattern (new Figure 7). We admit that the lags introduced in the discussion paper cannot be justified. We therefore use zero-lag correlation throughout the revised manuscript. Only exception is the correlation between the MOC index and the North Atlantic SST, where we use a lag of 5 years (as common in the literature) for all models. We note that the correlation between the MOC index and the SST is not part of the mechanism explaining the simulated in-phase variation of Iceland-Scotland overflow strength and AMO index.

Other comments:

3) The paper has "beyond the scope of this study" in four occasions. In all of them the authors could at least speculate on the importance of the not-researched part for the

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conclusions of their paper (in particular for P3282L25ff). I will be more specific further down.

4) Following from 3) it comes as a bit of a surprise that the possible differences arising from different forcings in the different models is beyond the scope of the study, when the forcing is key to distinguish the mechanism here from the one in control simulations. At least discuss what influence the different volcanic forcings could have on the results (P3263L19ff).

Response:

Regarding differences in external forcing:

The difference in volcanic and solar forcing among models is small. The 'beyond-scope' sentence mainly relates to the differences in anthropogenic forcings. Since the analysis of mechanisms underlying the in-phase variation of Iceland-Scotland overflow strength and AMO index is limited to the pre-industrial period, we remove the 'beyond-scope' sentence in the revised manuscript. We note that in the revised manuscript, a discussion of periods with strong volcanic forcing is included (see response to comment 20).

Regarding mechanisms of Iceland-Scotland overflow variability:

The revised analysis (e.g. including Nordic Seas heat/salt content as suggested by the second reviewer) better describes the underlying mechanism. In the revised manuscript, we remove the 'beyond-scope' sentence and merge the corresponding text in section 4 (Discussion) into section 3 (Mechanisms).

Regarding importance of thermal or saline density anomalies:

The 'beyond-scope' sentence relates to subpolar density anomalies described in previous studies. To shorten section 4 (Discussion), leaving only the most relevant parts, we remove the corresponding text in the revised manuscript. The discussion of the Nordic Seas density anomalies in the model simulations used here, including possible

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explanations for the differences among models, remains.

11) P3266L15ff: could you at least plot the different AMO reconstructions in Fig. 1? This would help the reader in the sense that he/she can get a proper picture of the diversity. Also, you should at least briefly discuss the differences between the reconstructions. Why did you pick Gray et al.? Does it fit best to Mjell et al.? What are possible reasons for a match or mismatch? If you can prove that your choice is with good reason, this would make the paper much stronger.

Response:

Within the EU project on which both Mjell et al. and our manuscript are based, we have used Gray et al. (2004) as one of the most common AMO reconstructions. In the revised manuscript, we include the AMO reconstructions from Mann et al. (2009) and Svendsen et al. (2014) in the Figure showing the Iceland-Scotland overflow reconstruction from Mjell et al. and the AMO reconstruction from Gray et al. (2004) and very shortly discuss the different AMO reconstructions in the text.

9) P3265L9ff: could you investigate/speculate/give literature on what the possible effects of this model bias are?

Response:

In the revised manuscript, we move the following text, which in the submitted manuscript is stated in section 3 (Mechanisms), to section 2 (Model description): “Due to this model bias, the influence of the Iceland-Scotland overflow strength on the MOC variability might be underestimated in the models.”

12) Regarding IPSL detrending: is a linear trend the best fit? I generally have the impression that for ocean variables a quadratic trend is often better suited. More importantly, one of the reference given for the detrending (Mignot et al. 2011) in fact uses a quadratic trend. Please adjust or clarify.

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In the revised manuscript, we use a quadratic trend in accordance with the previous IPSLCM4 studies. We note that our conclusions are insensitive to the choice of the detrending method.

16) P3272L1f and P3274L28ff: could it be a weaker coastal current in response to changes in Nordic Seas gyre strength (Lehner et al. 2013, J. Clim.)? Or an upstream relation with the MOC (Holliday et al. 2008, GRL)? Could a composite analysis of the velocity field during strong ISo events help to get a clearer view?

Response:

A composite analysis with respect to very weak Iceland-Scotland overflow has been performed in response to comment 20, but does not give a hint of a relation with the MOC. We note that the Norwegian Coastal Current is not properly resolved in the MPI-ESM grid configuration used in this study.

21) P3285L4f: here the authors could summarize again to what extent these differences affect the robustness of their results.

Response:

In the revised manuscript, we change the conclusion regarding the differences among models to “However, the importance of the barotropic or baroclinic pressure gradient differs among models. In the model showing a less clear in-phase variation of Iceland-Scotland overflow strength and AMO index, also the wind stress in the Nordic Seas influences the overflow strength.”

5) As the analysis focuses on pre-industrial, I think the discussion of the ‘historical’ simulation can be removed (P3263L27ff).

6) For the introductory paragraph on external forcing influence (P3259L1-19), the authors might be interested in Lehner et al. (2013, J. Clim.), where volcanic and solar forcing are looked at separately in context of last millennium changes in the Nordic Seas and North Atlantic.

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7) I do not think the details and references on the ocean biogeochemistry module of MPI-ESM are needed, as it does not influence the physics. If it does, the authors should clarify this.

8) The used temporal filter is described as “21-year running mean lowpass-filter”. Is it just a running mean? Then I would just write “running mean” without “lowpass”. Or is it further treated in the frequency domain? Then please give the necessary details to be able to reproduce the filter.

Technical 1) P3259L25ff and P3268L4f and P3279L14f: recommend to use normal brackets instead of square brackets. Technical 2) P3262L8: ‘importantly’ Technical 3) P3262L22: ‘. . .discuss differences among the coupled climate models. . .’ Technical 4) P3263L2: ‘small amplitude’ instead of ‘weak scaling’ Technical 5) P3264L16: ‘and’ instead of ‘as well as’ Technical 6) P3267L3f: ‘with events of weak overflow’

Response:

The above suggestions are implemented in the revised manuscript.

13) P3268L27f: this seems congruent with simulations with CCSM3 (e.g., Lehner et al. 2013).

17) P3273L1ff: could you illustrate the westward retreat of the SPG? And why do you write “The retreat of the SPG could allow”? Could this be tested in order to be able to remove the “could”?

18) P3274L17f: could you give a reference for this?

Response:

The above suggestions do not apply to the revised manuscript, as the corresponding text is removed in response to the comments of the second reviewer.

Please also note the supplement to this comment:

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http://www.clim-past-discuss.net/10/C2072/2014/cpd-10-C2072-2014-supplement.pdf

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