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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 paper by Lohmann et al. takes up the suggestion by the paleo-reconstruction community that the Iceland-Scotland overflow (ISo) covaries with the Atlantic Multidecadal Oscillation (AMO) and examines this relation in last millennium simulations with three coupled climate models. The authors show that this relation exists in two of the models, but that the detailed mechanisms are difficult to identify and seem to differ between the models. They dismiss the Atlantic Meridional Overturning Circulation (MOC) to play a dominant role in this relation and rather identify low-latitude sea surface temperatures

(SST) to drive the AMO and its imprint on the SST in the Nordic Seas. Nordic Seas SST, in turn, serve as explanation for variations in the ISo. Interestingly, the general chain of processes in these externally forced last millennium simulations is distinct from the chain in unforced control simulations with the same models.

General comments

The paper is very well written, well structured, has good figure quality, and generally tells a concise story. Climate of the Past is a well-suited format for the publication of this research and I recommend the paper for publication after some minor revisions are applied. The paper does not present novel concepts, ideas, tools, or data, but tests existing theories in a model framework, which is an important contribution to the potential verification/falsification of these theories. Although a bit ambiguous by nature (different models with different mechanisms, sparse data to verify), the conclusions are important as they clearly seem to favor one of the two theories assessed. The authors do a good job in pointing out where the results are sufficient to support conclusions and where not. Addressing the following list of comments and questions should hopefully improve the manuscript.

Specific comments (in no particular order)

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

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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 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).

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 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).

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.

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.

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

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.

11) P3266L15ff: coufd 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.

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.

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

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

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

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

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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?

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?

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).

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.

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

Technical corrections

1) P3259L25ff and P3268L4f and P3279L14f: recommend to use normal brackets instead of square brackets.

2) P3262L8: ‘importantly’

3) P3262L22: ‘. . .discuss differences among the coupled climate models. . .’

4) P3263L2: ‘small amplitude’ instead of ‘weak scaling’

5) P3264L16: ‘and’ instead of ‘as well as’

6) P3267L3f: ‘with events of weak overflow’

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

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