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8, C1961–C1964, 2012

Interactive Comment

Interactive comment on "Climatic impacts of fresh water hosing under Last Glacial Maximum conditions: a multi-model study" *by* M. Kageyama et al.

Anonymous Referee #2

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Review of "Climatic impacts of fresh water hosing under Last Glacial Maximum conditions: a multi-model study." by Kageyama et al.

General Comments

The paper reviews the response to various amounts of hosing (amplitude, duration) during LGM conditions in a "ensemble of opportunity", that is, a multi-model ensemble that was not devised for model-intercomparison, but still allows to compare model results.

The set-up is promising enough. The paper somewhat falls short of expectations, in being almost merely descriptive, giving a catalog of responses, but hardly addressing the physics that might cause different response to the hosing. I will give a few examples/ suggestions where the paper could gain body, but realize that following all suggestions is probably too much asked for. I urge the authors to follow at least some of these, going a bit more into depth while revising this paper.

In addition I remark that I have read the comments of reviewer 1. I agree with almost every point made there and will not repeat the same points.

Specific comments.

My first and main comment addresses the wide range of response to hosing, indicating large differences in the sensitivity of the models. There is much more that can be said here. I'll suggest two different analyses.

1. The authors seem to have missed the paper: Weber, S.L. and S.S. Drijfhout, Stability of the Atlantic meridional Overturning Circulation in the Last Glacial maximum climate, Geophys. Res. Lett., 2007, 34, doi:10.1029/2007GL031437. This paper shows that the response to hosing crucially depends on M_ov, or F_ov, depending which school you adhere. This quantity is Stommel's advective salt feedback and essentially determines the response of the AMOC on hosing. Recent experiments with HadCM3 clearly illustrate this, and also recent analyses form CMIP3/5 ensembles indicates that differences in the response of the AMOC to changes in the hydrological cycle might be explained by different M_ov in the models. It would be good if the authors tried to analyse this indicator from the model output.

2. The AMOC scales with the meridional pressure gradient in the Atlantic. If the AMOC gets weaker, this should be reflected by a decrease in the pressure and density gradient. Part of this decrease is forced by the hosing, making the density smaller in the "Ruddyman belt". Part of this forcing is counteracted or amplified by advective feedbacks. Also the density in the South may be affected by advective feedbacks. This feedback analysis is nicely explained in a paper of Swingedouw et al., co-author of the present paper, and working in the same institute of the first author. So, it wouldn't be

too difficult to apply this method, if the first author has the ocean data from all models at his disposal.

3. The WES feedback. First, I do not understand why the WES feedback is mentioned in response 1 and not also in response 2, page 3837. On page 3842 the authors describe the different propagation of the cooling signal along the Atlantic subtropical gyre. This points to models having different WES-feedbacks. Isn't this a point where the authors could say and analyse a bit more, in terms of why the WES feedback operates more strongly in 1 model than in another?

4. What is the best scalar for the temperature response? Also Rev. 1 discusses this point, but I would like to go a bit further. the largest temperature signal occurs in the North Atlantic. The main driver for temperature changes there is the decrease in heat transport convergence, due to a decreased AMOC. This implies that to understand the temperature response there, the better index for the AMOC is the strength at 30-40N instead of 30S. An even better scalar might be the northward ocean heat transport at these latitudes. Also in Figs 6 and 7 I would use the heat transport convergence in the selected boxes as a scalar to interprete the temperature changes. Linking the responses to both heat transport and AMOC changes allows for a more physical interpretation of what's going on.

5. Minor point. Also, if the temperature change s dominated by changes in convergence/ divergence of heat transport, it is not obvious to expect a bipolar seesaw signal without much zonal detail. I would not expect SH-warming where the heat transport associated with the "global Conveyor Belt" was not divergent before the hosing was applied.

6. Minor point. I accept that your ensemble of opportunity puts is not ideal and that you cannot always make a clean comparison between the various experiments. But calculating an ensemble-mean standard deviation from model exps that used different amplitudes of the hosing is stretching things a bit too far. I suggest to redraw Figs. 3a

CPD
8, C1961–C1964, 2012

and 5a only for the 7 runs that featured 0.1 Sv hosing.

7. Minor point. You might also want to remark that different signals, as discussed on page 3844, are likely related to the difference forcing applied in part of the model ensemble.

8. Minor point. With the precipitation response being so diffuse outside the Tropical Atlantic, I suggest to shorten this section considerably.

Interactive comment on Clim. Past Discuss., 8, 3831, 2012.

CPD

8, C1961–C1964, 2012

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