

Revisions to “Climatic impacts of fresh water hosing under Last Glacial Maximum conditions: a multi-model study”

Response to Anonymous Referee #1

The comments from the reviewer are repeated in black and our responses are given in blue/italic.

General Comments

The paper is generally well framed, although I found a number of type-o's and “figure captions” in the text. When these minor issues are cleaned up, I think the paper is acceptable for publication.

We thank the reviewer for this positive review. Our response to his/her specific comments is given below.

One minor general issue is the use of the word “collapse” to describe the weakening and or shut down of the AMOC. I recommend that the authors clearly define this term early in their discussion. I think it would help some readers. Our community does not use the term in a consistent fashion. I note that the term is used consistently throughout the paper.

This is indeed a crucial issue. The term “collapse” is now defined at lines ~108ff.

Specific Comments

1. Page 3834, Line 23 – Manabe and Stouffer 1988 is the wrong reference here. They did NOT do water hosing to find their 2 equilibria. I think the Manabe and Stouffer 1995 is the first hosing paper – I think. Manabe, Syukuro, and Ronald J Stouffer, 1995: Simulation of abrupt climate change induced by freshwater input to the North Atlantic Ocean. Nature, 378, 165-167.

This is right, of course. We have replaced the citation of Manabe and Stouffer in the right context (first paragraph) and added the citation to Manabe and Stouffer 1995 (Nature) and 1997 (Paleoceanography). We use all these references because we have extended the introduction to better explain the different goals of fresh water hosing experiments and the concepts of equilibrium vs. transient simulations and of stability of the AMOC/hysteresis diagrams.

2. Page 3835, top - Define “collapse” here. See General Comments above.
“Collapse” is now clearly defined (see response to the second General Comment).

3. Page 3835 – Make the distinction between transient and equilibrium response earlier. The hysteresis is an equilibrium, not transient response.

We have largely modified the introduction to better explain these concepts. We agree that it really was necessary to better explain the differences between these types of experiments. This helped us to clarify our objectives, which are to analyse and compare the transient responses to a fresh water perturbation simulated by several models, mostly in terms of climate, in connection with the AMOC.

4. Page 3836, Lines 5-15 - Manabe, Syukuro, and Ronald J Stouffer, 1997: Coupled ocean-atmosphere model response to freshwater input: Comparison to Younger Dryas event. *Paleoceanography*, 12(2), 321-336 could be referenced here too. They compare the impact of differing hosing locations.

This reference has been added, thank you for reminding us about this paper.

5. Page 3838, bottom and table 1 – I would argue that the inclusion of whether or not a model uses a free surface parameterization with true freshwater fluxes (not virtual salt fluxes) is an important table entry.

We have added this information in table 1, and in table 2 we have given the method each group has used to obtain their hosing experiment (i.e. with a true fresh water flux or a negative salinity flux)

6. Page 3840, line 1 – rapidly document – wrong meaning – I would just delete the phrase.

In this sentence, we meant that in this work, our primary objective was to document the climate response to AMOC changes in different models, rather than focussing on the AMOC response (and diversity of the responses) per se. We have rephrased this sentence and hope it is clearer now.

7. Page 3840, line 9 and line 23 – Figure X shows – Here and later. This makes the text much longer and harder to read. This information should appear in the figure caption only.

OK. This sentence has been removed and we have attempted to remove similar statements along the text.

8. Page 3841, top – Here the time scale of the perturbation experiments seems important. Include time scale over which the response is computed here. It may help

the reader to include an additional figure which shows an example of what is being discussed. I would recommend showing a model with an active AMOC and compare it to an inactive AMOC model showing the time series of the AMOC value plotted versus time.

The time scale over which the response is computed is given in Table 2. Now that the term “collapse” is clearly defined in the introduction and that it is clearly stated that we use the term collapse to describe an instantaneous state of the AMOC, and not a long-term collapse, we do not believe it is essential to show time series from the perturbed experiments, since this has been shown in many papers before.

9. Page 3831 – therefore does not – Delete therefore.

Done.

10. Page 3842, figure 3 – Nice way to show the results.

Thank you.

11. Page 3843, lines 7 – 8 – This shows : : : simple advection – What does this mean? I would delete this phrase or add a lot more to make meaning clear.

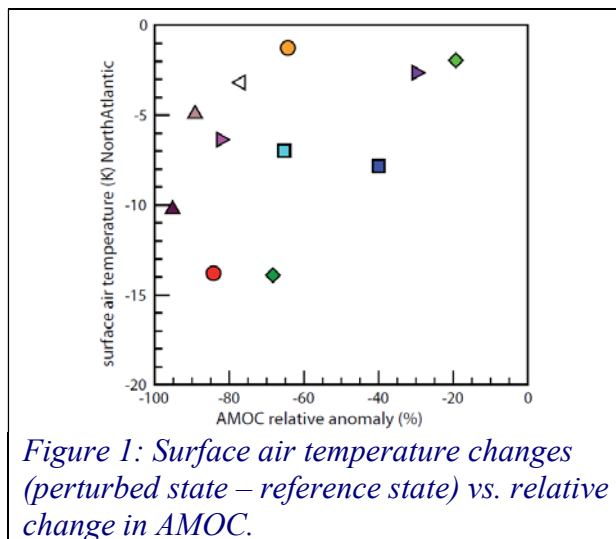
We have rephrased this sentence.

12. Page 3845, line 8 – surprisingly – Type-o.
Corrected, thank you.

13. Page 3846, line 3 – relationshipd – type-o.
Corrected, thank you.

14. Page 3846, line 14 and figure 6 – I recommend showing the fraction of the total AMOC response versus surface temperature change. In my analysis of models, the total heat transport by the ocean is much more similar between models than the AMOC strength. This implies that the relationship between AMOC and heat transport varies a lot from model to model and therefore the surface temperature change when the MOC changes. Showing the fraction of the AMOC change MAY provide a better relationship.

Figure 1 below shows the surface air temperature changes over the North Atlantic as a function of the relative change in AMOC. The relationship between these variables does not prove better than the one between surface air temperature changes and absolute AMOC changes (Figure 6, top, in the original manuscript). Nevertheless, we feel that it would be interesting to show both figures and we have included this new panel in Figure 6.



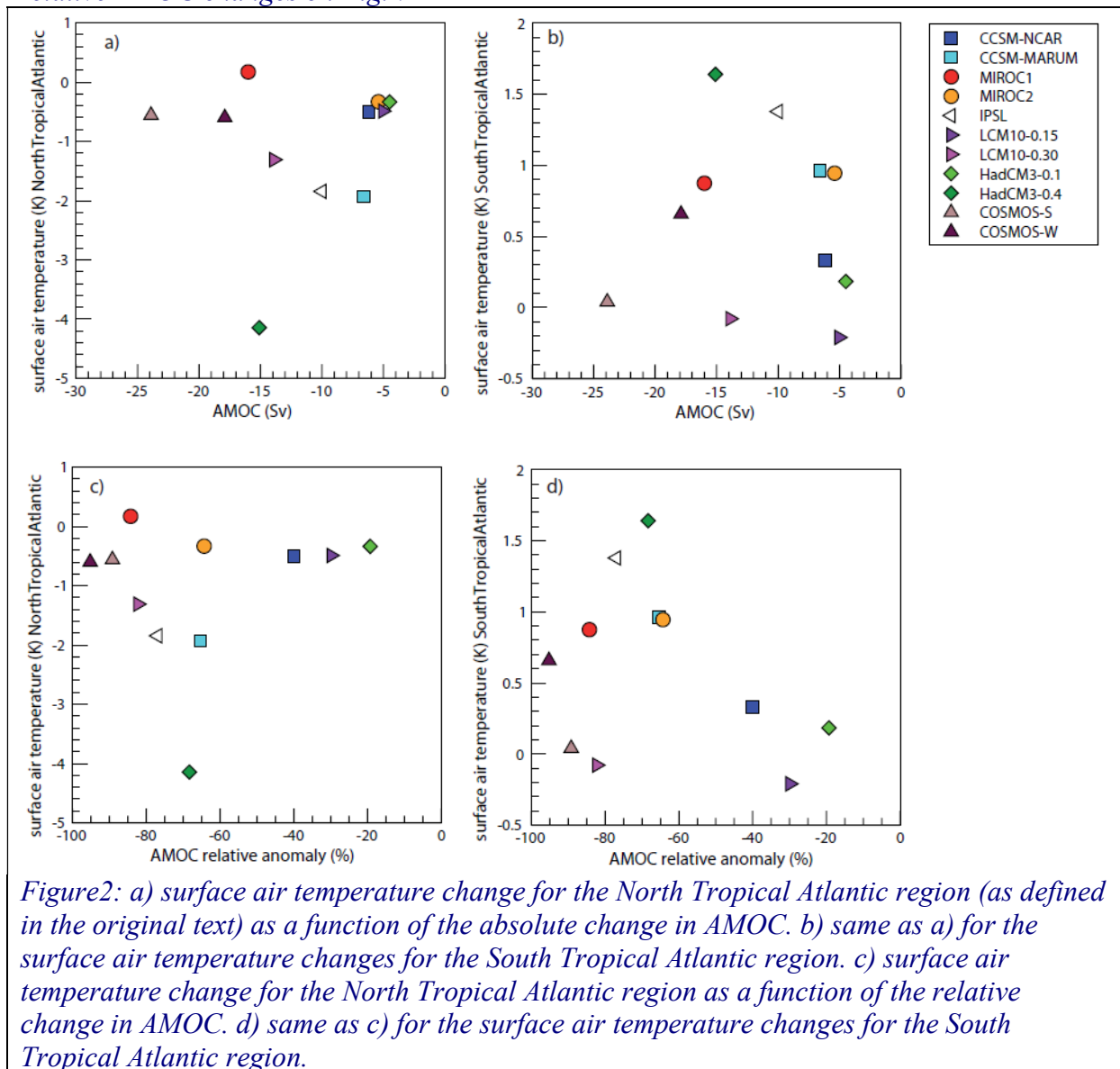
15. Page 3846, line 21 – except – “Exclude” is the correct word. I think.
Corrected, thanks.

16. Page 3847 and figure 7 – See comment 13.

Here the reviewer probably wants to refer to comment 14.

Figure 2 below shows the surface air temperature changes over the North Tropical Atlantic and the South Tropical Atlantic regions, as a function of the absolute and relative changes in AMOC. The relationship does not improve if we plot the temperature changes vs. the relative changes in AMOC. However, it becomes clearer that strong NorthTropicalAtlantic cooling can only be obtained for a significant decrease in AMOC (more than 50%), while this is not a sufficient conditions some experiments with a strong AMOC decrease do not show a large change in NorthTropicalAtlantic temperatures. The same is valid for SouthTropicalAtlantic

warming. We have therefore opted for showing the tropical Atlantic temperature changes vs relative AMOC changes on Fig. 7



17. Page 3852, line 1 – quite encouraging – These seem to be the wrong words. The fact that the mean climate state does not seem to influence the results (comparing the LGM hosing results here to the present day hosing of Stouffer et al) is interesting. The fact that the results are similar allows one to use the physical reasoning and analysis of Stouffer et al to explain these results.

We have removed this sentence which was not very clear anyway, sorry.

18. Page 3859, table 2 – It would help the reader if the model years of the perturbation run were shown for the CCSM-MARUM model. Saying the last 100 years, does not allow the reader to adjust the transient runs

Fine, this entry has been modified.