

Review: Eddy permitting simulations of freshwater injection from major Northern Hemisphere outlets during the last deglacial, by Love et al.

The manuscript explores freshwater transport to deep-water formation regions under Younger Dryas conditions by using high resolution global ocean model simulations and with more realistic (compared to previous studies) freshwater injection amounts.

I think it is an interesting work to be published in *Climate of the Past*. My major problem with the manuscript is about the presentation of the results (text, structure and figures) which makes the readers to use their imaginations instead of the paper. The results do not really convey the main message and some important points are not discussed. The authors should re-structure the text and re-write some parts to ease the reading, and put some figures from the supplementary to the main paper. Below, some of my major and minor comments regarding the text and analysis are given in more details:

Major comments:

Introduction:

-The main story and motivation of the study is hidden behind all the text. It should be re-written with a clear and standard structure, where general description of the problem, goal of the paper and previous studies are clearly discussed.

-Figure 1, is a very nice figure but it is not really discussed and not clear why you used it. I think it deserves more explanation.

Experimental design:

-The model description should be improved (e.g., you need to clearly specify that you use a global model and how many vertical levels your model has). Next, discuss the forcing. Then, explain the initialization of experiments, the control runs, number of spin up years (exact numbers), and total simulation years. Last, explain the experiments with all the needed details. In the current version, you might have given most of these information but it is done in a messy way.

-I think your Figure S2 should be discussed in this section and be used as a main figure and not a supplementary.

Results:

-I wonder why there is no summer sea ice in the Arctic in the region above (north of) Greenland? As far as I know that is a region that is covered by sea ice in summer (for present day condition).

-How is the Gulf Stream (that is found to be highly zonal), sea ice and regions of deep-water formation in this study compared to previous studies?

-Figure S5: you show only 1 year (the last year of simulation). Please choose similar intervals and same number of simulation years for all the figures (e.g., 5-y mean of year x to y). Same for Figure 2, what is meant by single day? For your study, yearly-mean values should be fine but take the average over several simulation years.

-Would be interesting to see the timeseries plot for the MLD in Labrador and Nordic seas.

-Figure S6-AMOC:

You initialize the model from the experiment by Hill and Condron (2014), right? But why your experiment's AMOC is about 6 Sv at year -10 while it should be larger given the AMOC in Hill and Condron (2014)? If I am mistaken, please explain this part better.

Overall, the AMOC in your study seems to be smaller than some similar studies (<https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1002/2015GL064583>), right?

What is the difference in AMOC between CBS and OBS control runs? The AMOC difference between the experiments seems to be small (1 or 2 Sv), and perhaps within the range of model internal variability. I am not sure if the AMOC can give any conclusive view.

-Figure 4:

Is it surface salinity? Except in the middle panel, the salinity anomaly shows a downward trend in some of the experiments (for instance the CBS MAK). You need to be careful how you interpret these as the model is not clearly far from equilibrium.

-One implication of this study is for the Younger Dryas event which is linked to temperature changes, and I was expecting to see a plot for the sea surface temperature (SST). I realized that this is only an ocean model study but would still be interesting to see the (indirect) impact of different FW injection on SST.

-I will include a figure similar to Figure 3 but for BS closed in the main paper. Also Figure S6 is better to be in the main paper.

Minor comments:

Title: needs to be adjusted. Is it really during the last deglacial?

Line 9: You are using paleo forcing and paleo-bathymetry, please correct it.

Line 35: Sv is the common unit to use, and dSv is not really helping to make things easier.

Line 91-92: Does the model really capture the coastal boundary currents?

Line 104: "The first...": revise

Line 134: "...discussed in Experimental design section": is not discussed

Figure S6: It is strange to use negative time values for the model spin up period.