Climate of the Past Discussions

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RC: caveats not sufficiently discussed

Anonymous Referee #1

Referee comment on "Eddy permitting simulations of freshwater injection from major Northern Hemisphere outlets during the last deglacial" by Ryan Love et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-15-RC1, 2021

This paper presents a set of sensitivity experiments using an ocean-only GCM forced with atmospheric boundary conditions from CCSM3 in a LGM configuration. The ocean model has a relatively high resolution (about 5 times greater than most CMIP5 models) and can be considered as eddy permitting. The authors perform a set of 5 simulations where freshwater is released from different locations that corresponds to potential region of glacial icebergs and meltwater discharges in the last ice age and deglaciation. All the sensitivity simulations are shorter than 20 years, and the focus is put on the pathway of the freshwater in the Atlantic Ocean, and its impact on the regions of oceanic deep convection. It is found that freshwater pathways are highly dependent on the release location, as was already highlighted in a few studies.

The scientific topic tackled by this study is of interested, given the very large uncertainty concerning freshwater pathway that might be related with oceanic resolution (e.g. Gillard et al. 2016). These pathways are clearly of importance concerning the response of the deep convection and AMOC as highlighted in e.g. in Swingedouw et al. (2013). The novelty of the study as compared to existing work by e.g. Condron &Winsor (2012) is the use of glacial boundary conditions, and a more systematic analysis of the different potential outlet locations as well as the consideration of smaller rate of freshwater release, more in line with recent reconstructions. The pathways of freshwater release are of great importance to refine our understanding concerning the last deglaciation notably and the impact of freshwater release by melting ice sheet.

Nevertheless, there are a number of major caveats which might strongly limit the utility of these experiments

- As highlighted by the authors, the length of their simulation is very short, which strongly hamper the interpretations of the results from those simulations for paleoclimate timescales, which are usually two order magnitude longer, as illustrated in Fig. S1 from the paper
- The use of glacial boundary conditions apparently lead to a collapse of the AMOC in the ocean-only GCM used. The authors qualified it as a glacial state, but Fig. 6 shows a weakening AMOC index in the control simulation (which is thus not equilibrated at all) towards values of 2-4 Sv that rather correspond to an off state than a weak glacial states, according to e.g. Ganapolski and Rahmtorf (2001). AOGCMs indeed do not produce such weak state in glacial condition (e.g Kageyama et al. 2013, with all AOGCMs showing value larger than 5 Sv in their mean state). Considering an off state has major implications in term of barotropic circulation, notably in the subpolar gyre, which makes the relevance of those results doubtful for examining freshwater pathways at the beginning of e.g. the Younger Dryas as it is suggested in the paper.
- The updates with former work is quite far from substantial, and the models used is very close to the one used in e.g. Condron & Winsor (2012). After almost a decade, computing power have strongly increased, so that these simulations now cannot be really considered state-of-the-art anymore, since far higher-resolution ocean-only simulations now exist, and do show that having even stronger resolution play a crucial role for the mean state of the AMOC (cf. Hirschi et al. 2020). As such, I am surprised that the authors still consider such short simulations (cf. point 1). Improvements in our understanding of the impact of ocean resolution from models of oceanic circulation need to be more appropriately discussed (cf. Hirschi et al. 2020, Le Corre et al. 2020)
- The discussion of the implications of their results for paleoclimate understanding is very weak and deserve to be strengthen. What does those results mean in regard to existing literature that GOM and GSL affect so weakly the convection zones? What does that mean in terms of last deglaciation storylines?

While the first caveat is discussed appropriately in the paper, the 3 others are very poorly covered, if not at all. I therefore cannot recommend the paper to publication until appropriate discussions of these caveats are provided.

Please find below some specific points that provide further insights on the 4 main points listed above.

- Figure 1: The data are difficult to see during YD due to very strong red. Please consider another colour to allow proper examination of the curves
- Line 41 and elsewhere: “eg.” Should be replaced by “e.g.”
- Line 90: such ocean-only model are simulations are not that costly within present-day computing time standard (e.g. Penduff et al. 2018 who considered 50 members of multi-decadal high resolution simulations…). Improvements as compared to former work with Condron as co-author, dating than almost a decade is not clear at all, while the main message remains also quite similar with this former work.
- Line 89-90: How many vertical levels in the model?
- Line 99-100: more should be said concerning the experimental design. Since these are ocean-only simulations, how are considered the boundary conditions? Is there any SSS restoring? How evaporation fluxes are computed,

- Line 134: this very zonal Gulf Stream might also be related with the fact that the AMOC is in an off-state, since this can strongly impact Gulf Stream pathway (e.g. Caesar et al. 2018)
- Line 141: "Labrador" Sea (not sea)
- Line 145-146: This claim is not supported by anu figures, and I strongly doubt of this, given the very small value at 26°N. The AMOC is state rather resemble an off-state. Can we see the meridional streamfunction in the last 10 years of the control simulation?
- Line 152: "glacial mode" sounds very optimistic. The authors might need to discuss more what is known from data and models concerning the mean state of the AMOC during the LGM…
- Line 153: "reasonable". This might be a bit too much optimistic as well I think. Please discuss appropriately the state of your AMOC, or provide more evidences to support that it can be considered as a glacial state.
- Line 225: "yr" is not defined.
- line 267: A proper discussion of the implications in terms of the storyline of AMOC changes over the deglaciation and the link with freshwater release should be provided. As an example, we can assume that those experiments strongly support a major role for freshwater release from Fennoscandia, as suggested in e.g. Toucanne et al. 2009. Please, further elaborate on this topic in light of existing literature.
- Line 267: An additional caveat is not properly discussed which is the fact that the authors consider here ocean-only model, which prevent from considering any potential coupled ocean-atmosphere feedback, which might play a role.
- Line 268: "under Younger Dryas conditions": this statement does not really reflect the off state that is simulated in the control simulation.
- Line 276-279: it is quite unclear from where those estimates come from, which is weird to provide in the conclusion, since not shown in the result section. I assume, they are estimated from a similar approach as in line 232-241, which is considering an ocean without any circulation at all. This is quite a strong hypothesis… Thus, I'm not sure those estimates are really useful, especially in the conclusion.
- Line 283: "better ways to mitigate this problem": this sentence is quite enigmatic. Can you please clarify what is meant here?
- Line 284-285: it should be stated here that these investigations are done in an off-state for the AMOC, and during only 20 years.

Additional references

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