General comments

This paper presents a large number of different simulations where freshwater is released in different locations in the North Atlantic – Arctic in glacial climate conditions for time scale lower than a century. It uses two different models: one OGCM at 1/6° of horizontal resolution and one AOGCM at 3° horizontal resolution in the ocean. In fact the OGCM is mainly used to derive a fingerprint of freshwater pathway that is then used within the AOGCM. A few differences concerning the sensitivity of the freshwater release is found and interpreted before to evaluate the climatic impact of the freshwater at the different locations.

The topic of the paper is of great interest given that there are now more and more studies highlighting the strong sensitivity on the way the freshwater released along the North Atlantic coast is spreading to the resolution of the ocean model used. This is crucial given that the meltwater from glaciers and ice sheets is actually largely released at the coast, while convection in the ocean generally occurs in the open ocean. Thus, depending on the lateral diffusion of the coastal freshwater signal towards the center of the gyres, the impacts of meltwater might be very different depending on the model used, and potentially not realistic at all in coarse resolution model like CMIP6 ones. Here the authors consider quite a high number of sensitivity experiments to tackle this question in the context of paleo timescale and glacial climate. The idea of fingerprinting the freshwater pathway using a high-resolution is interesting and might deserve to be published.

However, while this topic is highly relevant both for the past and the present, I find that the present study is not providing a very strong progress on the existing science. Indeed, the tools used are quite "old" (AOGCM from CMIP3 generation, while CMIP7 is now coming, and OGCM at 1/6° of resolution, which was already run more than 20 years ago, e.g. New et al. 2021 and the new standard for this kind of ocean-only model is rather around 1/20 to correctly solve the eddies, e.g. Hirschi et al. 2020 among many others). Thus, we cannot really say that the tools used are really state of the art. And this has important implications given that eddy rich models are really changing the dynamics of the AMOC, far more than eddy permitting (Hirschi et al. 2020) and are likely closer to the dynamics at play in the real ocean. This might be clearly stated!
Models with such coarse resolution are now still used since they can allow to perform long transient runs (multi-millennia) to analyse intrinsic AMOC variability during glacial time for instance, but here only simulations of 100 years or less are performed. Since this study aims at evaluating climate dynamics during paleo-periods, we can wonder if such short-time simulations are really relevant to improve our understanding of climate dynamics as compared to existing records. The implications of the results are also poorly discussed in regards of existing literature.

Last but not least, the paper suffers from other considerable issues like: not-in-depth analysis of the physical mechanisms at play, poor logical linkages in the text, lack of discussion of caveats and limitations of the study, and poor synthesis of the main results of the study. Thus, I support rejection and resubmission of a clearer and more in-depth analysis of the physics at play in their simulations.

Specific comments

- Abstract: It is quite long and not very clear. I agree that the experimental design used is quite complex and it took me time to understand it, but please try synthesize better what you've done and why (and leave details out of the abstract to focus on the main results).
- Line 68: you can consider to cite Swingedouw et al. (2022) to substantiate this claim (although this was using an eddy rich model).
- Line 75: the link with climate change is not obvious given that the focus here is on glacial time and paleostudies. Please state this more clearly.
- Line 88: unclear sentence? What was missing? Please be more explicit.
- Line 108: "cannot" is too strong. The parametrization aims at reproducing that. What you mean here is that they are not doing a good job at this, which I agree, but maybe with better parametrization this can be solved.
- Line 121-132: As far as I understand the use of MITgcm is only for finding the fingerprints that are then used in the coarse resolution OAGCM. This is not stated very clearly here, while the title of 2.1 states implies it but not that clearly. Please rephrase a bit to clarify this.
- Line 134-150: not much is said about the mean state of this MITgcm run. What is the strength of the AMOC, where are located the convection sites. Is the circulation realistic for glacial time?
- Line 165: replace "is" by "might be", since the use of only an eddy-permitting model is really questioning the realims of the results.
- Line 174: when are those 5 years selected (what time of the simulation).
- Line 181: this claim is not supported by any figures I think, so a "not shown" is necessary here.
- Line 198: if there are millennial intrinsic oscillation in COSMOS, how did you change the period of the control simulation? Is the AMOC changing on the long term?
- Line 210: since the focus here is on the freshwater release, the way this freshwater is accounted for in the model should be depicted. Is the model rigid lid or free surface? Which kind of parametrization? Is salt conserved with this parametrization?
- Line 213: still this question about intrinsic variability during glacial time?
- Line 249: within those questions, there is an implicit assumption that the response of model to freshwater only depends on their pathway, which is totally false, as shown in Stouffer et al. (2006) for the same design of freshwater release, the model responses can vary by an order of magnitude in terms of AMOC!
- Fig. 5: I do not find this analysis very enlightening: the curves are very messy and not much is said about the differences. Are they due to AMOC response? A correlation of the climate response this AMOC response (with dfferent lags) might be the least to be done. The residual might represent the effect of other processes than change in meridional heat transport due to the AMOC, including atmospheric noise, change in stratification, gye transport, etc.
- Line 389: unclear sentence.
- Line 404: this is "not shown", is it?
- Line 411; any correlation to support this claim?
- Line 417-419: unclear sentence.

- Line 435: replace “is” by “might be” since this is a hypothesis at this stage.
- Line 440: “first principles”. What do you mean? Please be more specific here.
- Line 445-447: please rephrase, I do not get the reasoning here.
- Line 450-451: Fig. S7 should show significant results only! (using a t-test of the difference for instance).
- Line 465-475: since the site of convection are not shown (among many other things, e.g. barotropic streamfunction, etc.), I think that the analysis is in the end quite poor and not going much into depth of real understanding of the differences in response.
- Line 481-483: where is it shown?

Bibliography

Hirschi, J. J. -M., Barnier, B., Böning, C., Biastoch, A., Blaker, A. T., Coward, A., et al. (2020). The Atlantic meridional overturning circulation in high-resolution models. *J. Geophys. Res. Oceans* 125, e2019JC015522. doi: 10.1029/2019JC015522

New A. L. et al (2001) On the role of the Azores Current in the ventila- tion of the North Atlantic Ocean. Prog Oceanogr 48(2–3):163–194

Swingedouw D., Houssais M.-N., Herbaut C., Blaizot A.-C., Devilliers M. and Deshayes J. (2022) AMOC Recent and Future Trends: A Crucial Role for Oceanic Resolution and Greenland Melting? *Frontiers in climate*, 4:838310. doi: 10.3389/fclim.2022.838310.