Review by Sam Sherriff-Tadano

We thank Sam Sherriff-Tadano for the very helpful comments and suggestions.
The original comments are displayed in red, our replies in black.

Summary

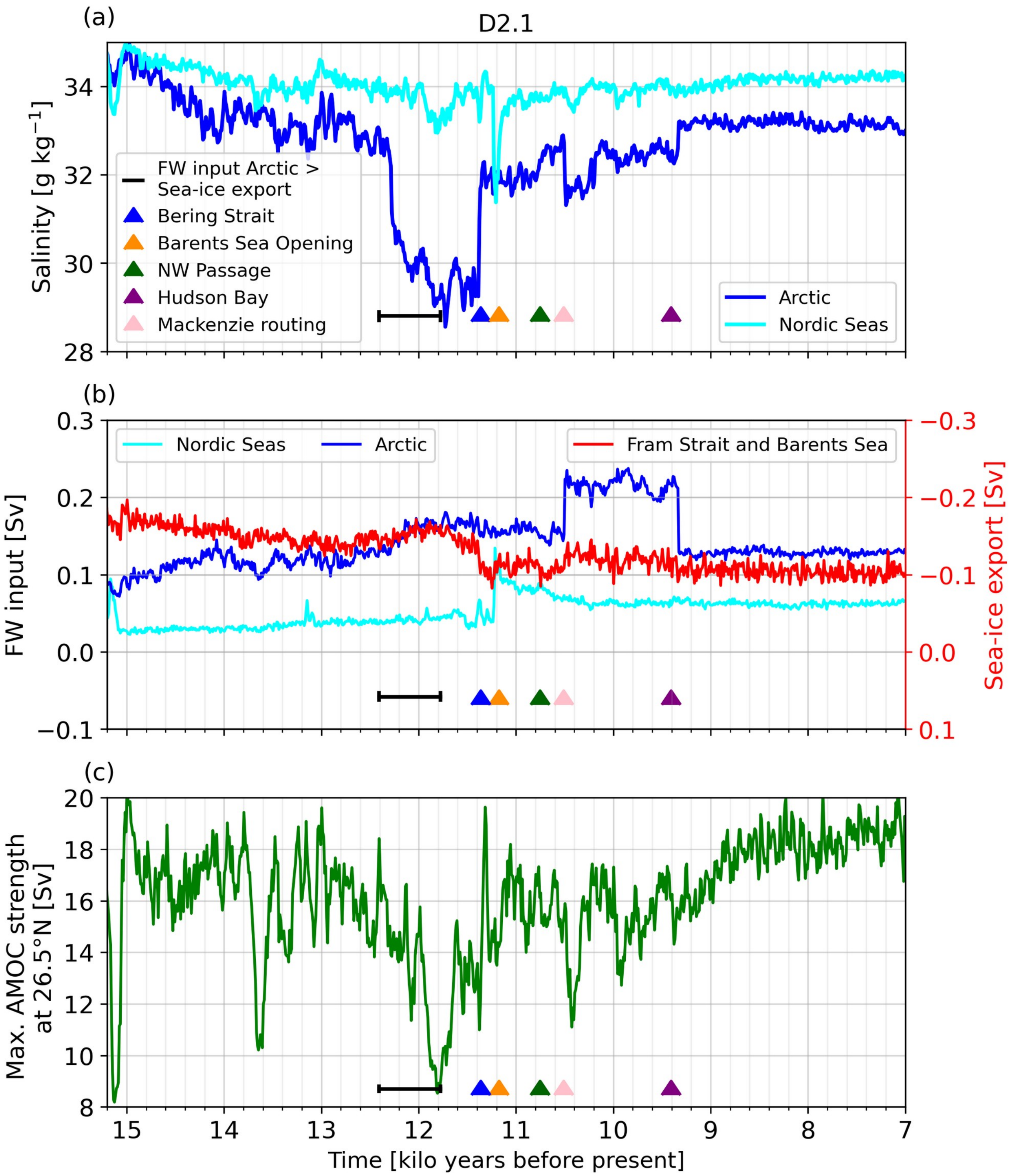
Mikolajewicz and others present the ensemble simulations of the last deglaciation with a coupled comprehensive atmosphere–ocean–ice sheet–solid earth model. All the simulations reproduce the general characteristics of climate and AMOC at the LGM and the deglaciation within the uncertainties of reconstructions. The authors further show intriguing results, such as the effects of ocean background mixing on the AMOC, the comparison of the dye tracer with the traditional definition of AMOC depth (AMOC=0 line), the regular and chaotic aspects of ice serge and their impacts on the abrupt climate change and effects of the opening of Bering Strait and the river routine on the abrupt climate changes. Particular attention is also given to the freshwater budget analysis in the Arctic when explaining non-surge-related abrupt climate changes.
The results presented in this study with their complex coupled earth system model are all exciting and are of interests of the readers of Climate of the Past. I also believe that the simulations performed in this study will greatly contribute to advance the science of the climate community. While, I recommend an intermediate revision based on the comments listed below, I do feel these results should be published. Thanks for all the effort in conducting this study!

General comment

My main concern focuses on the analysis of freshwater budget over the Arctic region (Fig. 12) and its effect on the AMOC.

The abrupt AMOC changes in the second half of the deglaciation are explained by the balance of freshwater input and sea ice export in the Arctic, however some ambiguities remain. For example, the rapid salinity drop in the Arctic Ocean (Fig. 12a, blue) precedes the timing of the shift in the net freshwater budget defined by the author (Fig 12 black triangle), though this was not explained. Perhaps, gradual changes in oceanic freshwater transport may affect the exact timing, but please clarify this point.

This is correct. We believe that gradual changes in the transport are the cause for the abrupt salinity drop. Note also, that the black triangle are chosen, where the 100-year running mean of the freshwater input becomes for the first time larger than the sea-ice export. There is substantial variability in this variable, which is also stated in the text "While the AMOC weakening is rather gradual at first and overlaid by large centennial-scale variability, a gradual decrease in Arctic sea-ice export starting at about 12.0 ka results in a further increase in the net freshwater budget and ultimately leads to an abrupt weakening of the AMOC". To visualize that the changes are gradual, we will replace the triangles by a transient line, which indicates the dates for which the unfiltered decadal mean time series of the Arctic freshwater budget crosses zero for the first time and the last time.



Modified Fig. 12

Second, the change in AMOC in response to the opening of Bering Strait is also explained by means of sea ice export. However, the role of changes in oceanic freshwater transport between the Arctic Ocean and the North Pacific is not discussed, which could be important (e.g. Hu et al. 2012). Please add this analysis.

Thanks for this suggestion. We will add a short discussion on the role of the Bering Strait and the North Pacific. For this, we have calculated the freshwater export in form of liquid freshwater and sea ice through the Bering Strait, using a similar approach as Hu et al. (2008) by taking the global mean salinity as reference salinity to calculate the transport (note, that the global mean salinity is changing over time in our simulations). Similar to Hu et al. (2008), we find that freshwater is exported from the Arctic into the Pacific once the Bering Strait opens (see figure below). This enhanced export likely also contributed to the abrupt increase in salinity of the Arctic ocean that coincides with the Bering Strait opening. We will modify the text accordingly and add a discussion on Hu et al. (2008, 2012) in respect to the Arctic/Pacific freshwater transport.

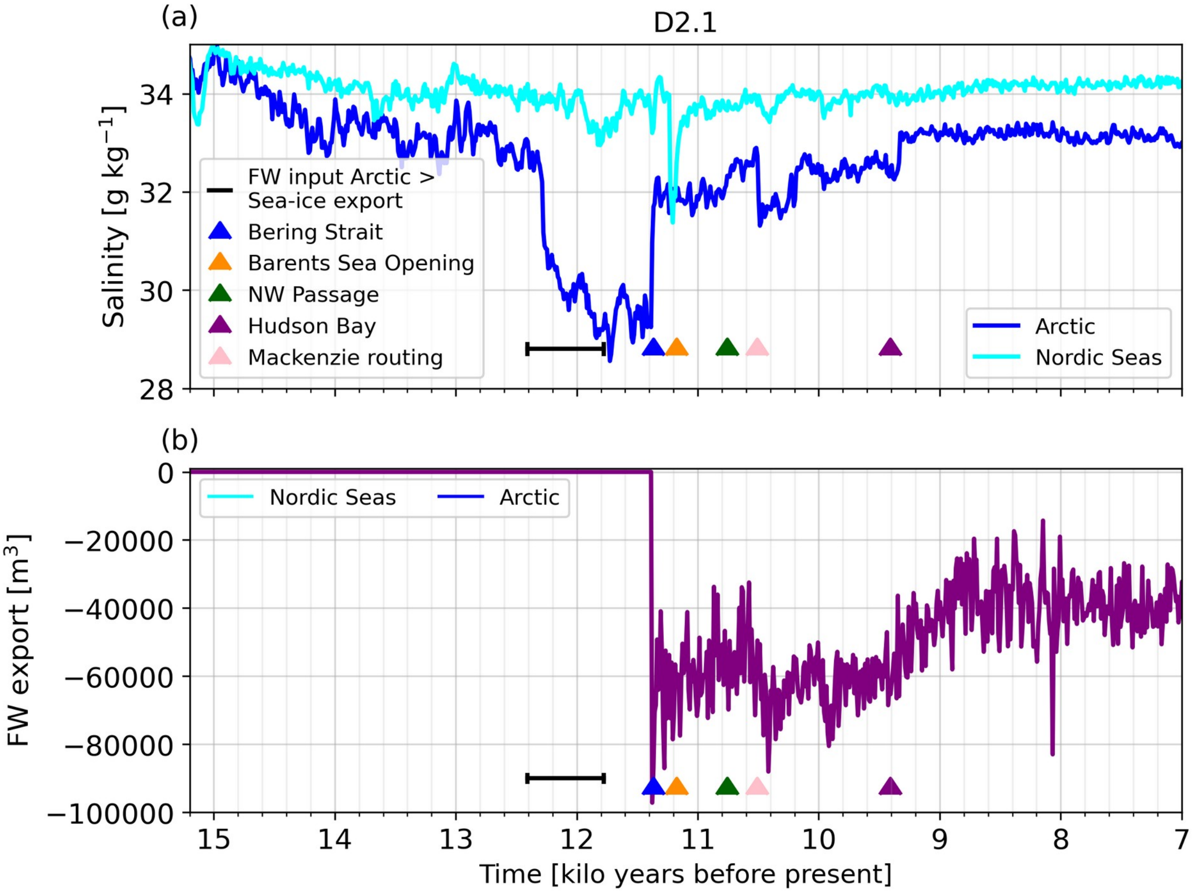


Fig. Mean surface salinity of the Arctic and the Nordic Seas (a) and calculated freshwater export (liquid + solid) from the Arctic to the North Pacific (b).

Lastly, I would like to suggest the authors to add a discussion on the stability of the AMOC in this model. In the simulations of deglaciation, the AMOC basically remains stable in its vigorous (or so-called interstadial) mode; e.g. even after a drastic weakening due to substantial freshwater input, the AMOC rapidly recovers to the strong mode once the freshwater flux ceases. This may be influenced by the presence of the North American ice sheet, but if the AMOC remains stable in the strong mode throughout the deglaciation, it may be more challenging to reproduce the Heinrich Events (HE) and BA-like events. Given that the introduction mentions the relationship between freshwater input and BA, it would be valuable to expand on this point in the discussion or conclusion.

We agree with the reviewer that the mono-stability of the AMOC in our model is a point worth discussing. We will add a remark on the mono-stable AMOC around L396, where we will also mention the mono-stability of other versions of MPI-ESM (Jackson et al. 2023). In addition we will discuss the mono-stability in the conclusion.

HE events are Eigenoscillation of the Laurentide ice sheet leading to occasional ice surges. This is largely independent of the AMOC stability. However, the amplitude of the climate response on these surges clearly can depend on the AMOC stability. We will discuss this in section 4.2.

Minor comments

L43-44: Not an easy sentence to understand. Could you explain a bit more, please?

We will revise the sentence:

“However, the sources of uncertainty in this methodology are numerous and diverse. They include an incomplete understanding of the recording and storing of climate signals through proxy data (Dolman and Laepple, 2018; Liu, 2023), as well as a methodological weakness in the reconstruction of the water mass mixing based on proxy data. “

L60: Please cite Snoll et al. (2024, https://doi.org/10.5194/cp-20-789-2024)

We will do so.

L90: Would be useful to describe the climate sensitivity of the AOGCM somewhere in this subsection.

We did not perform the simulations (pictrl and abrupt4co2) which are required to calculate the climate sensitivity. Beside this, for such short time-scale simulations and analysis annual coupling frequency between mPISM and MPI-ESM would be more appropriate. We consider these numbers to be important for simulations of future climate change, but not so much for the transient simulations shown here.

L119: Could you elaborate a bit more on how the SMB are given to the ice sheet model? Is it a 10 year average?

We will add some more details in our model description.

L131: Just wanted to make sure, but in D1.2 and D1.3, changes in ocean mixing is applied after the spin-up, right? If so, please clarify it wherever appropriate.

It was already clearly mentioned in L 143/144 and shown in table 1. We will additionally mention it in the figure caption of fig. A1.

L149: Could you briefly explain the reason of changing the sea ice parameter, please?

Our original choice of the sea ice strength parameter Pstar was based on Hibler 1979. More recent papers e.g. in Hunke and Dukowicz (1997) or Mehlmann and Korn (2021) use a similar high value. The reduced drag coefficient (cw) between ocean and sea ice is motivated by the fact that the catabatic winds in this model are somewhat underestimated due to the coarse resolution and thus the sea ice transports. We will mention this in the revised paper.

L150: Could you briefly describe the reason why the modifications in background mixing is applied only at the upper ocean, please?

The changes in background mixing had a strong effect on the age distribution in the deep Pacific. In order to avoid these undesired effects in the age distribution but to benefit as much as possible from the effect of the reduced background mixing, we restrict the changes to the upper 1000m. We will add a sentence in the text.

L222: Nice results! This might be a difficult question, but why does this model manage to reproduce the LGM AMOC, but not the model version(MPI-ESM) submitted to Kageyama et al. (2021)? Resolution, meltwater from ice sheet or shape of the ice sheet? Please add a sentence on this point.

The behaviour of the standard MPI-ESM-LR is relatively close to model P1 in Kapsch et al. 2022. Our model D1.1 is rather similar to model P3, except for the iceberg component. We are not discussing the MPI-ESM data submitted to the Kageyama et al. (2021) in this paper. We would have to introduce the model simulations presented in Kapsch et al. (2022), which use prescribed ice sheets, for a detailed discussion. This is outside the scope of the paper, as the LGM AMOC is only a small part of this paper. One of the key aspects for the difference in the AMOC response is probably the reduction of the warm bias in AABW.

L269: While it is displayed in Fig. 8, I think it would be extremely useful to summarize the volume of individual ice sheets at the LGM and PI in a independent Table. This would be a great reference for the readers.

Thanks for the suggestion. We will add a table listing these quantities to the appendix

L271: This is a common problem in other climate-ice sheet model simulations so it might be a good idea to refer to those studies as well (e.g. Ziemen et al. 2014, Sherriff-Tadano et al. 2024, https://doi.org/10.5194/cp-20-1489-2024)

We will add the reference to the papers mentioned.

L360: It’s interesting to see the decoupling of Antarctic ice sheet and NH ice sheets, despite the climate model reproducing the temperature evolution of SH high latitudes (FIg. 6c). What happens to ice calving of Antarctica ice sheet in response to sea level rise due to NH ice sheet melting (e.g. Gomez et al. 2020, https://doi.org/10.1038/s41586-020-2916-2)?

The coupling between the northern hemispheric ice sheets and the Antarctic ice sheet remains unchanged during the simulation. Evidence for this is that the Antarctic ice sheet does respond to sea-level changes during Heinrich Events. However these changes are short-lived and unlike in the study of Gomez et al., they do not seem to cause the Antarctic ice sheet to cross any stability threshold. Having said this, the ice volume trajectories between northern hemispheric ice sheets and the Antarctic ice sheet diverge in most simulations deeper into the deglaciation. We think that this is primarily caused by different processes driving ice-sheet retreat in the respective hemispheres. In the north, ice sheets are predominately land-based and their decay is driven by atmospheric processes. In the south, large parts of the ice sheet are marine-based and here basal melting regulates most of the simulated retreat.

L382: The effect of surface warming on the AMOC can be state dependent. For example, Zhu et al. (2015, https://doi.org/10.1007/s00382-014-2165-x) showed that surface warming from a glacial state can cause an intensification of the AMOC by reducing the amount of sea ice.

We will formulate the text a bit more cautiously to avoid this problem. We will restrict the effect of the AMOC weakening to the meltwater injection. We will only mention that the slow surface warming enhances vertical stability.

L434-443 & Fig. 12: The abrupt reduction of Arctic salinity (Fig. 12a) precedes the shift in net-FW input over the Arctic (black triangle) and the abrupt AMOC weakening (Fig. 12c), which may imply a role from other processes in reducing the surface salinity over the Arctic and in weakening the AMOC. Could you add an explanation on this point please?

As pointed out in an earlier comment, the shift in the net-FW balance of the Arctic is gradual and cannot be related to one single point in time. To emphasize this point and to visualize that the changes are gradual, we will replace the triangles by a transient line, which indicates the dates for which the unfiltered time series of the Arctic freshwater budget crosses zero for the first time and the last time. This marker indicates, that the shift in the net-FW budget of the Arctic occurs before the abrupt change in salinity.

L441-443: I'm not entirely convinced by this sentence because the role of sea ice export on the AMOC can be quite complicated. For instance, reduced sea ice export from the Arctic can lead to a freshening of the Arctic Ocean, which may increase the oceanic freshwater transport to the Atlantic and weaken the AMOC. Conversely, reduced sea ice export can also decrease sea ice melting in the North Atlantic, potentially increasing salinity in the DWF region and intensifying the AMOC. Therefore, a more comprehensive analysis of the freshwater budget is necessary to clarify this point.

We agree that the dependence of AMOC changes on sea ice export can be quite complex and depends probably strongly on the patterns of deepwater formation. In this particular case, however, the decreased sea ice export leads to the development of a sign shift of the Arctic surface freshwater budget (P-E + river runoff + iceberg melt – sea ice export) and allows the development of a halocline and suppresses deepwater formation in the Arctic. The resulting reorganisation of the overturning circulation results in a weaker AMOC. We will try to make this clearer in the text.

L444-449: I think this paragraph is missing the perspective of freshwater export from Arctic to North Pacific (e.g. Hu et al. 2008 https://doi.org/10.1175/2007JCLI1985.1). Please rewrite the paragraph including this point.

This is correct. We will add a short discussion on the role of the Bering Strait for the freshwater export into the North Pacific (see earlier comment). Similar to Hu et al. (2008), we find that freshwater is exported from the Arctic into the Pacific once the Bering Strait opens. This enhanced export likely also contributed to the abrupt increase in salinity of the Arctic ocean that coincides with the Bering Strait opening. We will modify the following sentences to "The opening of Bering Strait leads to a considerable reduction of Arctic sea-ice volume (about 20%; not shown) and sea-ice export to the Nordic Seas (about 20%) and allows for the export of freshwater through the Bering Strait into the Pacific (not shown)." and will add a discussion on the Hu et al. (2008) in respect to the freshwater transport.

L538: I think it would be helpful to clarify that Hu et al. (2015) drew their conclusion based on simulations without any ice sheet melting flux involved, while this study included the effect of ice sheet melting in a realists framework. The finding of Hu et al. (2015) was useful to understand the effect of Bering Strait on the net fw input over the Arctic, however as the authors state, the inclusion of ice sheet melting is important in controlling the timing of the shift of the net Fw input over the Arctic during the deglaciation.

Thanks for pointing this out. We agree with this comment and will add "These differences are likely related to Hu et al. (2015) not considering the effect of melting ice sheets, hence, not accounting for the inflow of freshwater from the ice sheets into the ocean."

Fig.3: Sorry if I'm wrong, but I feel the temperature anomaly and sea ice edges over the North Atlantic look inconsistent in (C) and (D). For example, a larger sea ice expansion is observed over the North Atlantic in (C) than (D), whereas the magnitude of surface cooling is larger in (D) than (C) in the same region. By any chance, the temperature anomalies are mistakenly swop between (C) and (D)?

This is a misunderstanding in how panels C and D should be interpreted. Both the temperature anomaly fields and sea ice extensions in C and D show the ensemble max and min at each grid point. So they should not be interpreted as dynamical fields but rather as indicators of the range of variations occurring in our ensemble. Even for temperature, the simulation with the warmest temperature at one place is not necessarily the one with warmest temperatures all another places.

We will try to make this more clear in the caption of Fig. 3.

Fig.A1: In terms of Dye Tracer and AMOC in PI, experiment D1.2 seems the best, but that is the one which does not simulate shallower and weaker AMOC at the LGM. Do you have any comment on that?

Fig. A1 compares the vertical profiles of the AMOC and the dye fraction for different vertical background mixing rates. We are not aware of any robust metric to rate the quality of the 3 experiments. Quantitatively, estimates from proxy data rather indicate a reduction of the dye below 2000m for the LGM compared to PI, which is not simulated in D1.2.

Fig. 7: Please expand the area of the NH so that the readers can compare the full extent of the simulated North American ice sheet with reconstructions.

We will do so.

Fig. 12b: Does the blue line include the input from ice sheet melting and river run-off? It seems so from the main text, but it is not explained in the caption.

Yes, we will make this more clear in the caption.

References not given in the original paper:

Hunke, E. C., and Dukowicz, J. K. (1997). An Elastic–Viscous–Plastic Model for Sea Ice Dynamics. *J. Phys. Oceanogr.*, **27**, 1849–1867. doi: 10.1175/1520-0485(1997)027<1849:AEVPMF>2.0.CO;2 .

Jackson, L., Alastrué de Asenjo, E., Bellomo, K., Danabasoglu, G., Haak, H., Hu, A., Jungclaus, J., Lee, W., Meccia, V., Saenko, O., Shao, A. and Swingedouw, D. (2023). Understanding AMOC stability: the North Atlantic Hosing Model Intercomparison Project. Geoscientific Model Development, 16, 1975-1995. doi:10.5194/gmd-16-1975-2023

Mehlmann, C., and Korn, P. (2021). Sea-ice dynamics on triangular grids. Journal of Computational Physics, 428, 110086. doi:10.1016/j.jcp.2020.110086