Reply to Reviewer#1

We are grateful to reviewer#1 for his/her time in reading and commenting on our manuscript, as well as critical suggestions. As described below, we will take all of the comments raised by the reviewer into account in the revised manuscript. Below, our responses are shown in blue, and the comments by the reviewer is shown in black. Again, thank you so much for your time in reviewing our paper!

Major comments:
Line 156: How do the modeled ice sheet conditions compare to reconstructions? At least for MIS3 reconstructions exist. Gowan et al (https://www.nature.com/articles/s41467-021-21469-w).
The ice sheet configuration used in this study comes from an ice sheet model simulation of Abe-Ouchi et al. (2013, nature), which reproduces the general pattern of ice sheet evolution over the past 400 thousand years. The simulated ice sheet volume at MIS3 (36ka) is 96 m sea level equivalent, which is larger than reconstructions suggesting 40 - 90 m sea level equivalent (Grant et al. 2012 nature, Spratt and Lisieki 2016 CP, Gowan et al. 2021 Nat comm). Hence, this suggests that our study might overestimate the ice sheet impact of MIS3. Nevertheless, Gowan et al. and other studies also show a smaller ice sheet at MIS5a compared to MIS3, so the qualitative difference of ice sheet between MIS5a and MIS3 used in this study is valid. We will add a discussion on this point in the revised Discussion.

Line 160: How do you account for land-sea mask changes for the different ice sheet boundary conditions? Are they manually adjusted? Why did you choose to leave the Bering Strait open? Do you account for Bathymetric changes in your hosing experiments? Part of this is explained in Sherriff-Tadano et al., 2021 but I believe it is necessary to include some of these important aspects in the current manuscript.
The change in the land-sea mask is incorporated manually. Figure A1 shows differences in land sea mask between MIS3 and MIS5a ice sheets. Largest changes in land-sea mask locate around the Barents sea region, where new ice sheet expands in the ice sheet model. On the other hand, changes in land-sea mask near the Laurentide ice sheet and Norwegian Sea, where main convection takes place, are small. In conducting partially coupled experiment, we adjusted the location of river runoff and atmospheric freshwater flux following the changes in land sea mask by shifting it to the closest ocean grid point. We will add this explanation in the Method section.

With respect to the Bering Strait, we did not include this change for simplicity. However, we do agree to the reviewer that the closure of Bering Strait at some point during MIS3 or MIS2, depending on sea-level reconstructions, can have an impact on the duration of stadial. We will include this discussion in the Discussion or Method in the revised version of the manuscript.

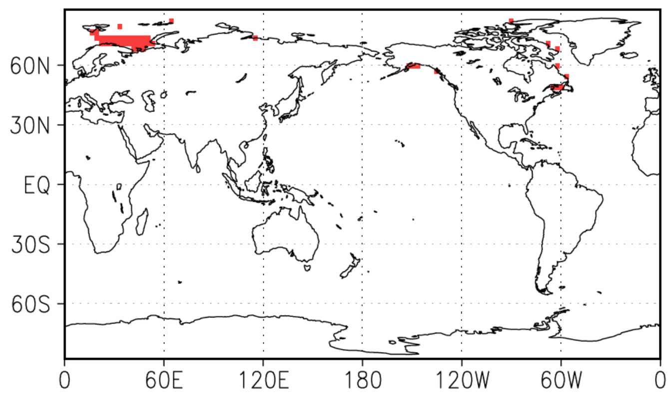

Fig.A1 Areas where the land-sea mask differ between MIS3 and MIS5a ice sheets.

Line 176: After reading the results I was wondering why you used monthly climatologies? The control hosing experiments and PC experiments show large differences in the timing of the recovery and I was wondering if a higher input frequency (e.g. 10-year monthly means or even monthly means) would avoid this issue. Specifically, since your main target of exploration is the recovery time of the AMOC. How sensitive are the results to different climatologies? And is the response to different climatologies consistent?

We agree to the reviewer's concern on the choice of input frequency, given that some previous studies suggested the importance of atmospheric noise in triggering the abrupt AMOC shift (e.g. Kleppin et al. 2015, Journal of Climate). We chose to use monthly climatology in our partially coupled experiments to demonstrate the role of atmospheric forcing in a clear and simple manner. However, we also confirmed that the general result is unaffected by the choice of the input frequency. Figures A2 and A3 show a response of AMOC recovery in partially coupled experiment forced with raw daily fields from the last 100 years of hosing experiments. Generally, the experiment shows a better agreement to the original experiment (Fig. A2, reason of this is explained in the reply to the next comment). The experiments also shows the same conclusion that the surface wind effect tries to shorten the duration, while the surface cooling effect tries to increase the duration (Fig. A3). We will clarify this point in the revised manuscript.

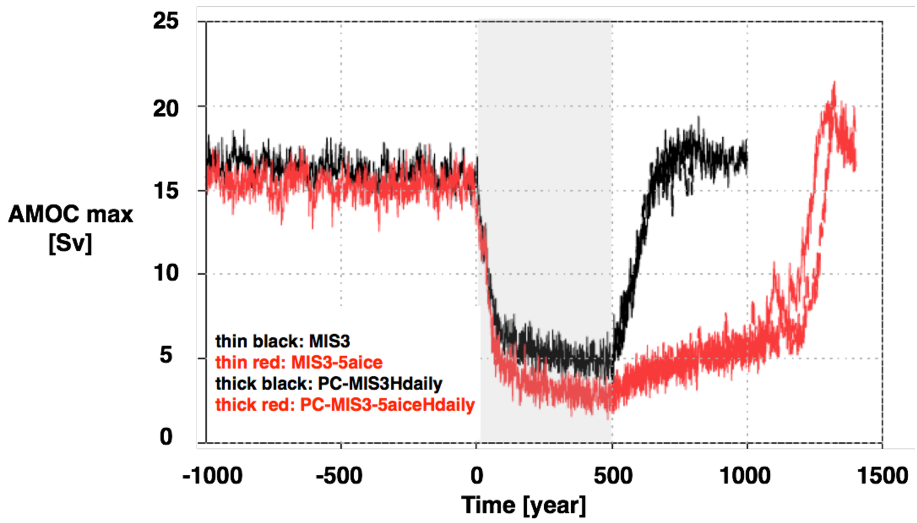

Fig. A2 Time series of AMOC. Freshwater hosing of 0.1 Sv is applied during year 0 to year 500. Black and red colors correspond to MIS3 and MIS3-5aice, respectively. The original experiments are shown in thin lines, while results of partially coupled experiments forced with raw daily fields obtained from the last 100 years of the hosing experiments are shown in thick lines. This figure shows that the partially coupled experiments reproduce the original experiment better when forced with raw daily values.

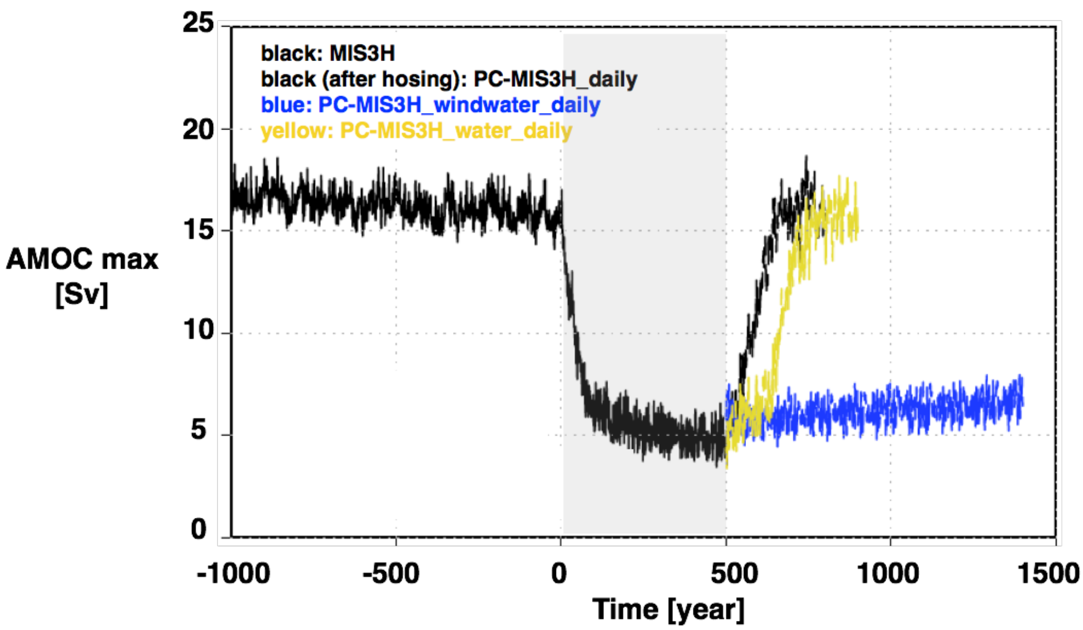

Fig. A3 Time series of AMOC in MIS3H (black) and new partially coupled experiments after the hosing is ceased (starting from year 500). The new partially coupled experiments are forced with raw daily values from the last 100 years of the hosing experiment. Black: PC-MIS3H_daily. Blue: raw daily surface winds and atmospheric freshwater flux of the last 100 years in MIS3-5aiceH is applied to MIS3H (PC-MIS3H_windwater_daily). Yellow: Raw daily atmospheric freshwater flux of the last 100 years in MIS3-5aiceH is applied to MIS3H (PC-MIS3-5aiceH_water_daily).

Line 301: 'slightly shorter' appears to be more than 500 years in Fig. 11 for PC-MIS3-5ahice and its reference experiment. These numbers make me wonder how sensitive the experiments are to the climatology that is used. See comment to Line 176. For me the PC experiment is hardly comparable to the original experiment, also the stepwise recovery in the original experiment does not occur in the PC experiment. Also in the PC-MIS3H experiment, there is no stepwise recovery. This needs to be discussed.

As pointed out by the reviewer, the duration of staidial in PC-MIS3-5aiceH is shorter compared to MIS3-5aiceH by 200 years when comparing the onset of AMOC recovery, or shorter by 300 years when comparing the timing of fully recovered AMOC state (Fig. 11 in the original manuscript). The shorter recovery period in the partially coupled experiment is associated with the thinner sea ice over the deepwater formation (Fig. 11b and c in the original manuscript). When we use the monthly climatology, less sea ice is transported to the deepwater formation region. As a result, it gets easier for the deepwater to form and causes the early recover of the AMOC. This problem is resolved when we force the partially coupled experiment with raw daily fields as shown in Fig. A2 and in Fig. A4, which compares the sea ice thickness over Irminger Sea. We will add a discussion on this topic in the revised Discussion or Supplementary. Nevertheless, since partially coupled experiments forced with monthly climatology reproduces the general feature that MIS3-5aiceH has longer recovery time compared to MIS3H, we would like to keep using the original experiments in the revised manuscript.

With respect to the abrupt recovery, the lack of stepwise recovery in the partially coupled experiment could be associated with weaker decadal variability and thinner sea ice over the deep water formation region. For example, in Fig. 9 of original manuscript, we have shown that the temporal cessation of deepwater formation at Irminger and Norwegian Sea associated with decadal variably could result in a temporal weakening of the AMOC. This temporal weakening of the AMOC then causes a slower recovery of the AMOC during the abrupt resumption in MIS3-5aiceH. However, we assume that this effect is weaker in partially coupled experiment since the coupling between the atmosphere and ocean is removed. While this topic is very interesting, we feel that it is beyond the scope of the study, since the main focus of the study is the duration of the recovery time of the AMOC, rather than the speed of the abrupt recovery of the AMOC. We are currently working on the interaction of decadal and millennial time-scale climate variability using partially coupled experiments and further results will be presented as a different study.

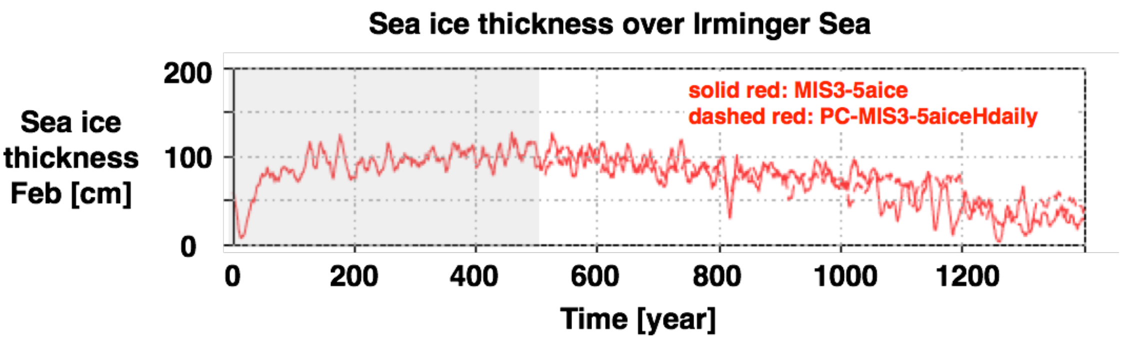

Fig. A4 Temporal evolution of sea ice thickness over the Irminger Sea (35W-25W, 55N-63N). Solid line corresponds to the original MIS3-5aiceH experiment, while the dashed line corresponds to a new partially coupled experiment forced with raw daily fields obtained from the last 100 years of hosing in MIS3-5aiceH. The result shows an improved reproducibility of sea ice thickness in partially coupled experiments when forced with raw daily values.

Line 385-388: What impact do uncertainties have? Previous studies have shown that uncertainties in the ice sheet reconstructions play a significant role for the glacial AMOC (e.g. Ullmann et al., 2014; www.clim-past.net/10/487/2014/). May some of these differences in the studies related to differences in the ice sheet boundary conditions? How sensitive are the results to these uncertainties? Comes also back the comment on Line 156.
Uncertainties in the volume and especially the shape of the glacial ice sheet can have a large impact on the result. For example, as we discussed in our previous paper (Sherriff-Tadano et al. 2021 CP), if the ice sheet has a thiner and wider configuration rather than thicker but smaller spatial extent, the effect of surface cooling likely gets stronger, which will favor longer staidal. Since, there's still a large debate on the volume and the shape of ice prior to LGM, we don't think we can draw a strong conclusion whether the ice sheet differences in MIS3 and MIS5a will try to reduce the duration of stadials. That is the reason why we choose to write the title and the last sentence of the abstract in a modest way. Our study, therefore, encourages further study on similar topic using other ice sheet reconstructions to better interpret the evolution of millennial-time scale climate and AMOC variability over the glacial period. We will clarify this point in the revised Discussion.

With respect to the comparison of this study and Sheriff-Tadano et al. (2021, CP), we used the same ice sheet configuration. Hence, the different sensitivity of stadial and interstadial AMOC to boundary conditions is not caused by the differences in the boundary conditions used in these studies. The result seems to be similar to ice core data, so we believe that the discussion on the different sensitivity of AMOC during stadial and interstadial is valid. But of course, as explained in the previous paragraph, if one uses different ice sheet configuration, which modifies the balance between the wind effect and surface cooling effect, different results might be obtained. One of the advantage of this study is that we clearly pointed out that the balance between the surface wind and surface cooling effects is important in determining the overall effect of ice sheets on the AMOC.

Minor comments:
All the text is written in past tense, I would suggest to write it in present. It might make it easier to distinguish between past studies and results from the present study. This would be very beneficial not only for the abstract but also the result section.
We agree to the reviewer that the present tense is better. To be honest, we wrote the first manuscript in present tense, but it was changed into past tense when we send it to an english correction service. If the past tense does not harm the clearness of the content severely, we would like to stick to the past tense at the moment, but if you disagree, we will change it to the present tense in the next round.

Line 21: I would suggest to rephrase to "under MIS5a and MIS3 boundary conditions and MIS3 boundary conditions with MIS5a ice sheets." or something similar. Otherwise it is confusing and not clear.
We will fix this.

Line 145: More than doubled is not 'slightly increased'. Please remove the word slightly.
Yes, we will remove "slightly".

Line 166: Please refer one more time to Table 1.
Yes, we will refer to table 1.

Line 176: I would recommend to remove 'that drove the ocean'. Also it should be 'a monthly climatology' or 'monthly climatologies'. Same at Line 179.
Thanks, we will fix this.

Line 180: Do you mean by noise the variability?
Yes, such as NAO and others discussed in Kleppin et al. (2015, Journal of Climate)

Line 241-242: It's not clear to me how you disentangle the effects or what you mean by: "In MIS3H, the effect of the glacial ice sheet was stronger than that of CO2, and thus caused shortening of the recovery time compared with MIS5aH."
Thank you for the comment. Effects of glacial ice sheets and CO2 (plus insolation) on the recovery time of AMOC can be decomposed by looking the difference between MIS3H and MIS3-5aiceH, and between MIS3-5aiceH and MIS5aH, respectively. These results show that the lower CO2 causes longer recovery time whereas the larger ice sheet causes a shorter recovery time. When comparing MIS3H and MIS5aH, the duration of stadial is shorter in MIS3H despite lower CO2 in this experiment. This occurs because the effect of ice sheet is stronger than that of CO2. We will clarify this point in the method section of the revised manuscript.

Line 338: 'depend' needs an s.
Thanks! We will fix this.

Line 415: I was wondering whether MIROC4m can produce the afortmentioned D-O oscillations without external forcing.
Yes, we have quite a few intrinsic AMOC variably in MIROC4m. These results will be presented elsewhere.