

Clim. Past Discuss., referee comment RC1  
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## Comment on cp-2021-47

Anonymous Referee #1

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Referee comment on "Does a difference in ice sheets between Marine Isotope Stages 3 and 5a affect the duration of stadials? Implications from hosing experiments" by Sam Sherriff-Tadano et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-47-RC1>, 2021

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Review of manuscript cp-2021-57 "**Does a difference in ice sheets between Marine Isotope Stages 3 and 5a affect the duration of stadials?**" by Sam Sherriff-Tadano, Ayako Abe-Ouchi, Akira Oka, Takahito Mitsui, and Fuyuki Saito.

Summary:

The manuscript investigates the processes behind the Dansgaard-Oeschger cycles during MIS 3 and MIS5a, using the coupled climate model MIRCO4m. To imitate D-O cycles they perform freshwater hosing experiments under MIS3 and MIS5 boundary conditions and combinations thereof. The authors then explore the duration of the AMOC slowdown and recovery process after cessation of the hosing. In sensitivity experiments they disentangle the effects of a sub-set of boundary conditions on the length of the AMOC slowdowns. The authors find that larger ice sheets associated with a surface cooling and an increased sea-ice extent tend to increase the recovery time of the AMOC. The effect of surface winds due to larger ice sheets likely plays a secondary role for the recovery from the freshwater hosing.

The study is well written, interesting and adds onto the present discussion on relevant processes behind D-O events. I believe that the study can contribute to this discussion, but the authors need to clarify a few uncertainties in this study, before I would recommend a publication.

### 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>).

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 Bathymetrie 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.

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?

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.

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/](http://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.

### **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.

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.

Line 145: More than doubled is not 'slightly increased'. Please remove the word slightly.

Line 166: Please refer one more time 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.

Line 180: Do you mean by noise the variability?

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 CO<sub>2</sub>, and thus caused shortening of the recovery time compared with MIS5aH."

Line 338: 'depend' needs an s.

Line 415: I was wondering whether MIROC4m can produce the aforementioned D-O oscillations without external forcing.