Dear Christo,

Thank you for the positive feedback. We revised the manuscript based on the reviewers' comments and provided point-by-point replies to each reviewer comment and amended the manuscript accordingly. Thanks to the reviewers' and your input, we believe this review process has greatly improved the manuscript. We hope the manuscript is now clear and comprehensive.

Best wishes,
The author team

Reviewer #1

First of all, I apologise for the delayed report!

I thank the authors for the revision of the manuscript and the careful addressing of my comments. I find that most of my comments have been addressed satisfactorily. I have a few remaining points, mostly editorial and some necessary clarifications.

We thank Dr. Marlene Klockmann for her positive assessment and constructive comments.

l.64-68: I find this sentence a bit confusing. The first half (easier testing) is due to computational resources, right? The second half refers to the complexity of the model system which is being tested. And we have already found indications for threshold over a large range of complexities (as you correctly mention in the sentence before). It needs to be more clear what the sentence wants to say. Perhaps it is just the connection of the two halves with the "but" that makes me stumble.

We divided the sentence into two and moved it to the end of the introduction, where it fits more clearly.

l.87-88 please add Klockmann et al 2020 (https://doi.org/10.1029/2020GL090361) for completeness here

Done.

l.113-115: can your simulations be compared more directly to the proxies? And why exactly? In l.600-602 you say you do not expect a direct agreement with proxies.

We expanded on these two points in the text (lines 110-113 and 613-616 in new manuscript version):

"While providing crucial process understanding, the limited simulation length makes direct comparisons of these simulations to proxy timeseries challenging, which is required to assess the role of these processes in glacial-interglacial AMOC changes."

“Since we chose to focus only on radiation driven AMOC changes in our experiments, while in reality AMOC was also influenced by freshwater flux changes, particularly during Heinrich events, we would not expect a close model-data match with reconstructed millennial-scale AMOC changes in the paleo-records.”

l.239: "lower three panels" instead of "lower two panels"

Done.

Fig.3/ l.295-299: I think the caption should refer to the modes in simulation A3 or simulation set A and not to the first 30kyrs of simulation B.slow? Also, please make the caption consistent with "Top:" and "Bottom:" as you did in the caption for Figure 2 (easier to read). Also, the modes in Fig.2 run from right to left, while in Fig.3 they run from left to right. Consider having them in the same order in both figures

Done.

l.538-541: Which simulation do you refer to, here? Also B.slow?

This is a summary of the observed behaviour in all simulations, A and B. We mention this explicitly now.

l.543-558: I find this whole paragraph difficult to read and follow. Is this meant in contrast to Oka et al 2021? Also some sentences don't work. Please rewrite for clarification!

We now specify the models we discuss in each sentence. We also moved the last sentences of this paragraph into the next paragraph to improve clarity.

l.600-602: Still, I find the comparison in Fig. 7a and 7b very impressive! Also, this sentence is somehow in contrast to l.113-115, where you seem to imply that your simulations can be compared more directly to proxies than other simulations (misunderstanding?)

Yes, this was unclear. Our computationally-efficient model can produce long time series that are required for model-data comparisons for glacial-interglacial time scales but our forcing (specifically no freshwater hosing) prevents a model-data comparison of millennial-scale AMOC variability. Instead, our comparison focuses on long-term AMOC shifts during glacial cycles. We clarified this now in the introduction and discussion (lines 110-113 and 613-617 in the new manuscript version):

“While providing crucial process understanding, the limited simulation length makes direct comparisons of these simulations to proxy timeseries challenging, which is required to assess the role of these processes in glacial-interglacial AMOC changes.”

“Since we chose to focus only on radiation driven AMOC changes in our experiments, while in reality AMOC was also influenced by freshwater flux changes, particularly during Heinrich events, we would not expect a close model-data match with reconstructed millennial-scale AMOC changes in the paleo-records. Still, we can compare the long-term evolution of AMOC strength in our simulations and the reconstructions.”

Fig.9: Do the grey bars also correspond to MIS3 and MIS6? If yes, this could be added.

Yes, we added this info to the caption now.

Reviewer #2

This is the second round of my review on the manuscript “Multiple thermal AMOC thresholds in the intermediate complexity model Bern3D” by Adloff et al. I’m glad to see the revised version that answers most of my previous comments well, and has been largely improved in the demonstration. I believe this work provide new understanding of the AMOC thermal threshold during glacial cycles. Nevertheless, I still have some comments that shall be considered before my full support for its publication.

We thank Dr Xu Zhang for the thorough review and the additional suggestions.

Implications of abrupt AMOC change at ~27 kyr in B.Slow. In line 410-413, the authors argued that a weak bipolar seesaw (i.e. sea-ice retreat/warming in the Southern Ocean) could be identified during the biggest AMOC weakening, which is hard for me to confirm this from Fig. 5. I would rather suggest that the simulated cooling/sea ice expansion in North Atlantic and Southern Ocean at ~27 kyr are similar to glacial inceptions for instance MIS5-4 transition, when bipolar regions are characterized by significant cooling together with AMOC shoaling. This further indicates that AMOC-induced bipolar thermal seesaw might just be in a second order during glacial inceptions while decreasing radiative forcing (e.g. insolation, CO_2, etc) is the dominant one, different from glacial conditions with mild insolation changes (e.g. MIS3).

We agree that the bipolar seesaw effect is too weak to counteract the negative radiative forcing. We rephrased the section to avoid any confusion (lines 414-418 in the new manuscript version):

“The biggest AMOC weakening at ~27 kyr was also accompanied by a very weak bipolar seesaw effect (Stocker and Johnsen, 2003), which caused a temporary decline in sea ice coverage in the Atlantic sector of the Southern Ocean (Fig. 5). This sea ice decline, however, was too small to reduce the radiation-driven sea ice increase in the longer term.”

Line 80-81: Please categorize references here to specify to 1) stability/sensitivity of the AMOC and 2) AMOC self-oscillation. Please also cite Zhang et al. (10.1038/nature13592) for the former. This literature elaborates roles of wind circulation associated with ice sheet height changes in modulating the AMOC sensitivity/stability during glacial periods, and can also be referred to in the discussions for instance in line 554-558, 575-580, etc.

Done.

Line 87: Zhang et al., 2014 (10.1002/2014GL060321) is not a proper reference here since they did not resolve AMOC oscillations in their model. Instead, Zhang et al (10.1038/s41561-021-00846-6) should be cited in which simulated AMOC self-oscillations are directly associated with successive DOs during MIS3. Please also update the citation in other relevant parts.

Done.

Line 90: please also cite Ganopolski & Rahmstorf (2001, Nature), which from my point of view is of direct support to this statement, but not for statement in Line 574-576.

We added the reference to the introduction as suggested but also keep it in the discussion because they discuss differences of AMOC stability under LGM and PI conditions.

Roles of earlier enhancement of AABW (associated with radiative forcing) in establishment of LGM ocean circulation. Fig 5 provides a good example to emphasize this point, which shall also be re-emphasized and discussed in Line 622-646, etc. There are dozens of literatures discussing such point but by snapshot experiments, for instance, Zhang et al. 2013 (10.5194/cp-9-1-2013), Galbraith & de Lavergne 2018 (10.1007/s00382-018-4157-8), which could be referred to in such discussion together with the transient modeling outputs in this study.

We now mention this in the discussion of Fig. 5 and in the discussion of our model limitations (lines 419-422 and lines 594-597 in the new manuscript version) as suggested:

"The volume of AABW in the deep Atlantic influences AMOC stability (Zhang et al., 2013, Galbraith and Lavergne, 2019). Thus, the spread of AABW into the deep North Atlantic after the first AMOC shift at ~24 kyr might have preconditioned the AMOC for the following shift at ~27 kyr in B.slow."

"Northern Hemisphere ice sheets also affect the composition and volume of AABW through teleconnections (Galbraith and Lavergne, 2019), and the buoyancy difference between AABW and NADW, as well as their fraction in Atlantic deep water, have been found to precondition AMOC stability (Zhang et al., 2013)."

Reviewer #3

The authors have made a huge effort in the revision and also managed to address all the comments I've wrote in the first round. I only have some minor comments left and happy to recommend the paper to be accepted for publication at the Climate of the Past.

Cheers,
Sam

We thank Dr. Sam Sheriff-Tadano for the detailed and constructive comments on the manuscript.

1. Relation of simulations A and B
Is simulation B.slow similar to A8? Sometimes I got confused when comparing results of B.slow and A2 or A3 since B.slow doesn't show a clear AMOC mode shift as in A2 and A3 in Fig. 1. Considering the speed of changes in the radiative forcing, I feel that B.slow and A8

are the closest. In that case, the fact the B.slow basically showing 2 or 3 modes makes sense. If the authors agree on this, it might be worth pointing it out in the Method section. One sentence would be sufficient. (Sorry if it was already explained somewhere..)

This is a good point. We added a sentence to the Methods section as suggested:

"For comparison, the magnitude of this forcing is on the upper end of the range explored in simulation set A (A6-A8)."

2. Role of Southern Ocean on the thermal threshold (L473-476 & L534-541)
The authors came to the conclusion that the North Atlantic processes are essential for the changes in the AMOC based on their analysis on salt transport, sea ice and deepwater formation regions. I agree to this in some sense, but also feel it's bit early to rule out the role of Southern Ocean (L473-476 & L534-541). To make this statement, I think additional experiments are required as in Oka et al. (2021). A good example is Oka et al. (2012) and (2021). In the 2012 study, they assumed that the thermal threshold was mostly related to the drastic shift of sea ice and the NADW formation region based on analysis. However, they found that this wasn't the case in their sensitivity experiments in the 2021 paper. While Fig. 5f doesn't show big changes in meridional salt flux across 32S, Fig. SI13 does show some salinity anomaly entering the Atlantic basin from Southern Ocean. Please amend these sentences (or some of the wordings) in a modest way so that the effect from the Southern Ocean cannot be ruled out at the moment.

We agree. We weakened our statement and added two sentences (lines 548-551 in the new manuscript version) to clarify:

"Thus, in our model, Southern Hemisphere cooling does not need to exceed the cooling of the Northern Hemisphere to affect AMOC but further sensitivity tests would be required to establish the relevance of cooling in each hemisphere separately (as shown in Oka et al., 2021)."

L299: Sorry, if it was explained in the response letter, but wasn't this figure created using results of the simulation A3?

Yes, this was wrong. We corrected the caption.

L383-384: Please explain the cause specifically. (e.g. due to fresher SSS and colder deep ocean temperature)

Done.

L386-389: Probably better to separate the sentence into two in my opinion. First one describing the shift of NADW formation region using Fig. S10 and S11 (is it correct?). Second one explaining the cause of it.

We shortened the sentence and hope it is now easier to comprehend (lines 392-394 in the new manuscript version):

"After about 6 kyr, NADW formation moved south as surface freshening stabilised vertical density profiles in the subpolar east North Atlantic and density profiles further south steepened due to surface cooling combined with subsurface warming (Fig. SI.7-9)."

L390 & L393; Are these sentences explaining similar thing? If so, please remove one of them to shorten the manuscript. Also shortening the manuscript is encouraged elsewhere.

Done.

L399: Probably better to say; a net increase in precipitation minus evaporation (P-E) led to …

Done.