Response to Reviewer #3:

We respond to the referee's comments in blue font below.

In the manuscript cp-2018-165 entitled “Equilibrium simulations of Marine Isotope Stage 3 climate” by Guo et al., the authors compared the simulated climate mean state of Marine Isotope Stage 3 (MIS3) and preindustrial (PI) era using the Norwegian Earth System Model (NorESM). They found a cooler climate in MIS3 relative to PI conditions with a thicker and more expanded sea ice. The AMOC strengthen by 13% with reduced AABW reaching the North Atlantic. Moreover the AABW production actually increases due to the increased sea ice cover in the southern oceans in association to the cooler MIS3 climate. They also show a reduced ENSO and NAM variability. Finally, by doing a few sensitivity simulations by reducing CO2 concentration or ice sheet height in the North America, they suggest that abrupt transitions of climate from interstadial to sta- dial state is not likely, and raised the question whether abrupt climate transition would be possible without changes of external forcings. I found this manuscript is well written and easy to follow. The results are interesting to the readership of the Climate Past community. Thus I would like to recommend this manuscript to be accepted after some revision:

We thank the reviewer for his/her overall positive comments on our manuscript. We respond to the reviewer's comments below point by point.

Comments:

1. The simulations are primarily focus on the PI and MIS3 climate background, thus it is not surprising that the climate states are stable for both conditions. One question the authors did not specifically clearly state is the initial condition of these runs. It seems that both PI and MIS3 runs state from the same ocean initial state, except an increase of the mean salinity for MIS3 run. Is this true? If so, how will this affect the model sensitivity when icesheet height or CO2 concentration changes?

> What the reviewer commented is true. We state in the manuscript that, for the MIS3 baseline experiment: "As for the PI experiment, the ocean model is initialised with modern temperature and salinity (steele et al., 2001) with the above mentioned salinity increment applied."; for the sensitivity experiments: “All the sensitivity experiments are branched off and initialised from the MIS3 interstadial simulation, and all other parameters are kept fixed.”

The addition of extra salt to the global ocean in the MIS3 simulations must have some effects - although we expect them to be small - on the modelled ocean and climate background state, and therefore the model sensitivity to, e.g. ice sheet height and CO2 levels. As far as we are aware of, there has not been any study looking into this problem, which merits more investigations. This is highly relevant to any glacial simulations, especially to LGM experiments where PMIP protocols define a global salinity addition of +1 psu.

2. The authors tested the model response of CO2 reduction be 15 ppmv. The question here is whether the MIS3 stadial climate is caused by CO2 reduction or by changes of the AMOC? It seems that the authors assumed that the CO2 reduction is the cause, is it true?

> We did not assume that the CO2 reduction is the cause of the stadial climate state. We support the wide-accepted view that changes in AMOC (e.g. strong or weak/off mode) are behind the interstadial/stadial climate states. As described in the Introduction, changes in AMOC can be invoked by changes in CO2 concentrations, as well as other factors such as changes in the size of Laurentide Ice Sheet.

The reason why we do such a sensitivity experiment (15 ppmv lower CO2), as also discussed later in the text, is partly motivated by some previous studies (e.g. Zhang et al. 2017; Klockmann et al. 2018) that do indeed show a transition of AMOC mode upon relatively small change of CO2 level. Another motivation is that if the model is already close to the threshold of mode change, which we did not know beforehand, a small change of external forcing is able to kick the system into a different state (see also our response to the comment below).

3. Although the experiments done by the authors don't show significant AMOC changes, it seems this is not enough to question the possible multi-equilibria of AMOC, especially the experimental design may not serve the purpose of the authors. A better test is to check whether AMOC has multi-equilibria in the NorESM under glacial condition. If yes, then the authors can test whether an abrupt transition of the AMOC is possible with the absence of the external forcing change. It may be important to test the small changes of the external forcings and whether this small changes can bring the climate state to a critical point in which even smaller changes in freshwater forcing is capable to collapse the AMOC.

> We agree with the reviewer that given the experiments performed, we cannot question the multi-equilibria of the AMOC; rather, the experiments suggest that our simulated MIS3 climate stays far away from the bifurcation/tipping point, and is in contrast to some previous studies that show 'sweet spot' within a certain range of external forcing, therefore addressing model dependence in studying model bi-stability.

We also agree that a more thorough and systematic design of model experiments are needed in exploring the multi-equilibria and hysteresis behaviour of the model, forced with a wider range of external forcing. Once we are close to the 'threshold', a small change of forcing is expected to be able to tip the system from one state to another - or even with self-sustained climate transitions. We realise that such studies are certainly meaningful, but are beyond the scope of the current study, and are worth further investigations in the future.