

Response to Reviewer #4:

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

The manuscript “Equilibrium simulations of Marine Isotope Stage 3 Climate” by Guo and colleagues well present a new MIS3 simulation in their model NorESM1-F. They employed a latest (new) ice sheet configuration without a large Fennoscandian ice sheet to conduct their MIS3-38ka simulation. By several attempts exploring the tipping point/bifurcation in their 38ka simulation, the authors claim that their 38ka simulation in NorESM1-F is too stable to reach a tipping point that was commonly used to explain the millennial-scale variability during the MIS3 in previous modeling studies.

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

The authors first described their 38ka simulation results in very detail (although it can be more compact), in accompany with a comparison with previous LGM simulations. The LGM is a good reference (LGM) to compare with, but there is lack of detailed discussion of their differences. The 38ka simulation was integrated for 2500 model years. The author argue that their simulation is almost in a quasi-equilibrium since the salinity trend in the Atlantic is less than 0.06 g/kg. How is it defined because it remains possible that the deep ocean is not in a quasi-equilibrium state if there exists a robust salinity increase in the AABW formation region. This is also my concern for their sensitivity simulation with lower CO₂ levels. It is very likely that polar regions need a much longer time scale (> 1000 model years) to cool down, producing the cold enough bottom/deep water masses and so the glacial ocean structure and circulation.

> Following the reviewer (and another reviewer)'s comment on the detailed description of the simulation results, we have significantly

reduced the length of the manuscript by moving some texts/figures/sections to the supplementary material; these include the time series of sea ice, several atmospheric diagnosis, the whole section of "Modes of variability", and part of the "stadial" experiment. The updated paper structure reads more compact than the previous version.

Regarding the reviewer's comment on comparison with previous LGM simulations, we actually tried to avoid a detailed discussion, as the two periods are distinct with each other in certain important aspects, e.g. MIS3 features reduced ice volume and a relatively warm climate compared to the LGM. In the updated manuscript, we've added more discussions with the existing MIS3 studies which were not adequately addressed in the previous version.

As for the length of model simulation, we are aware that the deep ocean, in particular the deep Pacific, cannot be fully ventilated. However, the ideal age in the deep Pacific by the end of model integration (2500 years) is less than 1600 years, indicating a relatively well ventilated ocean. There is not any 'hard' threshold to define the model state of quasi-equilibrium in the paleoclimate simulations. The only proposed threshold for the glacial simulations was proposed by Zhang et al. (2013), e.g. a salinity trend < 0.006 g/kg per century in the deep Atlantic can be considered as quasi-equilibrium. As for the lower CO₂ experiments, one integrated for 800 years and the other 1200 years, we agree that both runs can be extended. The experiments, with their current length of integration, do suggest that our simulated MIS3 climate stays far away from the bifurcation/tipping point, and is in contrast with previous studies that show 'sweet spot' within a certain range of external forcing, therefore addressing model dependence in studying model bi-stability.

The authors also assess the simulated climate mode variability in their simulations. It makes the manuscript more comprehensive. However, I do not find a clear connection to the following investigation of the AMOC bistability? why does the "stadial" condition under 40ka boundary not include freshwater forcing in the North Atlantic? I fully understand the

authors' purpose, but since H4 did feature a robust freshwater input, it would be more comparable and reasonable to force a stadial climate with the North Atlantic freshwater forcing under 40ka-38ka boundary conditions. If the authors would like to investigate the changes in climate variability in stadial conditions, I would suggest conducting the hosing under 38ka boundary condition. This will largely reduce the difficulty in the discussion of differences between interstadial and stadial runs. The present "stadial" climate can be included in the section regarding exploration of AMOC bistability.

> Following the reviewer' comment, we have moved the section on climate variability and the majority of the "stadial" experiment to the supplementary material, and the rest of the discussion on the "stadial" experiment to the AMOC bi-stability section.

We set up the "stadial" experiment to test if a non-Henrich stadial can be simulated with NorESM, as motivated by some previous studies (e.g. Zhang et al. 2017; Klockmann et al. 2018) that show a transition of AMOC mode and therefore a stadial-like climate upon relatively small change of CO₂ level. As the reviewer pointed out, freshwater flux is indeed present at H4, though with large uncertainties in magnitude and location, and should be applied for a 'realistic' stadial climate simulation. This is our ongoing work that we intend to write up in a follow up study; we use the hosing experiments with 38 ka BP boundary conditions to study the transition process from H4 to GI8.

In the discussion part, the author design several sensitivity experiments to explore the potential nonlinear behavior of their 38ka climate. The experiments are reasonable and clear, which can provide end-members of climate responses to glacial-interglacial variations regarding ice volume and pCO₂. It is a promising try here although probably these runs (especially lower CO₂ runs) are not in quasi-equilibrium, therefore the runs are not as conclusive as the authors argued. In addition, one conceptual mistake in the manuscript is that spontaneous oscillation does not share the same definition with the AMOC bistability, but rather a hopf bifurcation feature. The authors shall go through the manuscript carefully to distinguish their difference.

Overall I find the manuscript is interesting and well within the scope of Clim. Past. It can be accepted for publication after some modifications in the structure as well as refinements of model results description and discussion.

> We thank the reviewer for the positive comments on our manuscript. As we responded above, we agree that extending the sensitivity experiments, together with a wider forcing range, could be more beneficial to the simulation results and conclusions.

We went through the manuscript to double check the conceptual issue pointed out by the reviewer (e.g. see the second and third paragraphs of Section 4.3).

Finally, following all the reviewers' comment, we've made efforts to restructure the manuscript and present the results in a more succinct way.