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Climate of the Past Discussions

Interactive comment on "Equilibrium simulations of Marine Isotope Stage 3 climate" *by* Chuncheng Guo et al.

Anonymous Referee #4

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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.

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

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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.

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.

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.

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