

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

Anonymous Referee #1

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Guo and colleagues present an equilibrium simulation of the Marine Isotope Stage 3 (MIS3) with a fully coupled climate model. The simulated climate is very stable and representative of an interstadial climate state with a strong AMOC and relatively high temperatures over Greenland. A stadial climate with a weaker AMOC and lower Greenland temperatures cannot be simulated, not even with typical stadial CO₂ concentrations. Sensitivity studies with even lower CO₂ concentrations and flat ice sheets support the hypothesis that the NorESM model is very far away from a potential threshold where the climate changes from interstadial to stadial conditions.

The topic of the paper – MIS3 climate state and variability – fits well into the scope of Climate of the Past and is very relevant for the community. There are few fully coupled MIS3 simulations to date and the presented simulations is therefore very valuable as

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it adds more data points to the parameter space of glacial forcings and thus helps to understand (1) MIS3 climate variability and (2) the model dependence of glacial climate states.

I recommend the article for publication after some suggested revisions: I believe, the study could be put more into context with existing MIS3 simulations and reconstructions (see general comments below), and a few issues require clarification before publication (see specific comments below).

_____ General Comments: _____

The presented MIS3 simulation could be more embedded into the existing literature, both in terms of simulations and existing proxies. Throughout the text, especially in Sect 3.2, the analysis is very descriptive and there are very few comparisons with the existing MIS3 simulations in terms of surface temperature response, sea-ice patterns or AMOC state. The authors mention Barron&Pollard (2002), Van Meerbeeck et al (2009) and Brandefeld et al (2011) in the introduction. It is true that there are not so many coupled simulations with MIS3 boundary conditions, but there are some more MIS3 control simulations available that have been published as reference simulations for hosing experiments, e.g. Xiao Zhang et al (GRL, 2014) or Kawamura et al (Science Advances, 2017, here the information is somewhat hidden in the Supplementary Information).

Often the authors compare their simulations to existing simulations and proxies from the LGM. This is an obvious choice, since there are more simulations and reconstructions available for the LGM than for MIS3. But then these comparisons can be a bit confusing/misleading, since we would not expect the climate state of MIS3 and LGM to be the same. I therefore suggest that the authors go carefully through their manuscript again and check in each case what they want to obtain from the LGM comparison. Can some insight be gained from the LGM /MIS3 differences? If possible it would also be good to have a few more comparisons with existing MIS3 reconstructions, there is

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e.g. a recent study by Sessford et al (Paleoceanography, 2018) on water masses and sea-ice in the Denmark strait.

I believe, a more thorough comparison with the existing simulations and MIS3 proxies can help to highlight where the presented MIS3 simulation provides new insight and thus make the study more interesting and relevant.

————— Specific Comments: —————

p.1, ll.12-15: '[. . .] questioning the potential for unforced abrupt transitions [...]' In the text you phrase that conclusion quite carefully and refer to the model dependence of MIS3 climate (in)stability. In the abstract, the formulation is perhaps a bit too general, the model dependency should appear here, too.

p.3 - Model description: Would it not be easier to directly describe NorESM1-F rather than describing first how NorESM1-M differs from CCSM4 and then to describe how NorESM1-F differs from NorESM1-M?

p.4, ll.28 – p.5, ll.12: It is not quite clear to me, how the exact MIS3 ice sheets are obtained. Are they assembled from different sources? Why not take them all from the same reconstructions? And why is the Barents Sea so problematic? According to its mean depth it should be open also with 70m lower sea-level, no? Are there conflicting reconstructions?

p.6, ll.20: based on what do you decide that the trend is small? Is there also a threshold value such as for deep ocean salinity in the next sentence?

p.6, ll.30: is the sea-ice drift acceptably small?

p.7, ll.8-12: It is interesting though, that the final simulated MIS3 AMOC is still stronger and deeper than at PI, and the AABW cell is also weaker than at PI, even though AABW is saltier and more ventilated. I'll come back to this issue in a later comment.

p.7, ll.19-22: Are there no MIS3 studies available for the comparisons? (see also

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general comments)

p.8, ll.22: The warming in subpolar gyre seems to be more of a dipole. Ist the NAC shift a north-south shift? Can it be seen in the barotropic stream function in Fig. 11?

p.9, ll.22: Why is there more runoff into South China Sea, when precipitation is decreased according to Fig. 8?

p.9/10, AMOC and hydrography section: I find this section somewhat confusing for many reasons.

(1) I think, the LGM comparisons are not very helpful here (see also general comments), as the MIS3 AMOC is expected to be very different from the LGM AMOC. A comparison with the LGM AMOC and hydrography would be more helpful in the discussion, when speculating about reasons for a stable or unstable AMOC. If available, MIS3 comparisons would be more helpful here. From Böhm et al (2015) it should at least be possible to get a qualitative picture of the distribution of northern and southern sourced waters from the eNd measurements.

(2) I am surprised, that the North Atlantic salinity does not increase more than the global average of 0.6 g/kg. If the mechanism that makes the MIS3 AMOC stronger than the PI AMOC is the same in NorESM than on Muglia&Schmittner (2015) and Klockmann et al (2016/18), I would have expected a much larger salinity increase both at the surface and in the deep North Atlantic.

(3) If more warm NADW is present below 3000m, where does the very cold anomaly in the deep North Atlantic come from? Is that Overflow water then?

(4) Can the anomaly at 500-800 m really be attributed to the Mediterranean Outflow? I would expect the outflow at depths around 1100 m.

(5) What is ideal age? Is it the time since the water mass was in contact with the surface?

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(6) If the AABW formation is determined by increased sea-ice formation and brine-rejection, how can it be so well ventilated? I understand from the Ferrari (2014) paper, that AABW was very poorly ventilated because it was upwelled under the ice with little exchange with the atmosphere, and that this is one reason for the glacial CO₂ draw-down.

(7) If AABW ventilation and formation increases but the lower overturning cell weakens with less AABW reaching the North Atlantic, where does the AABW go? Is there more AABW in the Pacific?

p.13, ll.11-14: Same as comment (6) above: I understand from the Ferrari (2014) paper, that AABW was very poorly ventilated because it was upwelled under the ice with little exchange with the atmosphere, and that this is one reason for the glacial CO₂ draw-down. So how does that fit with a well ventilated AABW?

p.13, ll.27-31: Same as comment (2) above: If the mechanism that makes the MIS3 AMOC stronger than the PI AMOC is the same in NorESM than on Muglia&Schmittner (2015) and Klockmann et al (2016/18), I would have expected a much larger salinity increase both at the surface in the subpolar gyre and in the deep North Atlantic.

p.14, ll.12-13: Xiao Zhang et al (2014) on the other hand find that MIS3 is close to disequilibrium in their simulation.

p.15, ll.1-11: The sensitivity simulations appear quite short, especially the ones with the reduced ice sheets. What determined the length of the simulations? I would argue that the simulations are not in equilibrium, yet. Especially the simulation with 140 ppm could well still be declining – e.g. some of the simulations in Klockmann et al (2018) had about 1500 years of spin up, before the state transitions occurred. I would disagree with the statement that the AMOC strength is unaffected. The responses are small, but both ice sheet reductions and the very low CO₂ lead to an AMOC weakening. Whether the weakening is strong enough to produce a stadial climate state is then another question.

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Figures: I would say there are already almost too many figures. But still I would like to ask for a figure showing also the deep water formation sites on the Northern hemisphere. I think they could be very helpful for understanding the AMOC stability. I personally would find that more informative than e.g. the insolation in Fig. 1.

Technical comments:

p.1, ll.8: remove parentheses around 'by ~13%'

p.1, ll.21-23: reformulate sentence for clarity

p.3, ll.32: Add a reference for HAMOCC

p.5, ll.5 and throughout the text, for the convenience of the reader, take care to distinguish between ice sheets/land ice and sea ice. Sometimes only ice is used.

p.5, ll.10: MSI3 should be MIS3

p.14, ll.24: Remove parentheses around citation

Figure 11: solid contours should indicate negative values and dashed contours positive. They are mixed up in the caption.

Figure 14: This may be a matter of taste; I find it more appropriate to have non diverging colourmaps for the absolute T and S sections in (a) and (c).

References: Xiao Zhang et al (2014), Instability of the Atlantic overturning circulation during Marine Isotope Stage 3, *Geophysical Research Letters*, <https://doi.org/10.1002/2014GL060321>

Kawamura et al (2017), State dependence of climatic instability over the past 720,000 years from Antarctic ice cores and climate modeling, *Science Advances*, DOI: 10.1126/sciadv.1600446 + Supplementary Information

Sessford et al (2018), High-Resolution Benthic Mg/Ca Temperature Record of the Intermediate Water in the Denmark Strait Across D-O Stadial-Interstadial Cycles,

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<https://doi.org/10.1029/2018PA003370>

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-165>, 2018.