

Response to Reviewer #1:

Please see response to the referee's comments in blue font.

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 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).

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

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 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.

> Thanks for the insightful comments and very relevant references. Following the reviewer's suggestion, we have expanded the discussion with a more in-depth comparison of our results with published MIS3 simulations and proxy records; these include the modelling studies of e.g. Merkel et al., 2010; Xiao Zhang et al., 2014; Kawamura et al., 2017,

and proxy studies of, e.g. Böhm et al. 2015; Sessford et al., 2018. There are certainly more MIS3 proxy based studies, however we choose to focus on comparison to the Nordic Seas region.

As for the reviewer's comment on comparison with previous LGM simulations, we do agree that the two periods are distinct, e.g. MIS3 features reduced ice volume and a relatively warm climate compared to the LGM. We have carefully revised the manuscript, and removed the discussion on the comparison of SST and AMOC strength with LGM PMIP studies. We retained the comparison of near surface temperature - considering the close proximity of the two periods in time, and the much larger number of LGM proxy/modelling studies, we think it is useful to make the comparison (e.g. Van Meerbeeck et al. 2009), with caveats in mind of course. We've therefore added the following to the updated manuscript: "Compared to the amount of MIS3 studies, there is a rich literature on both the simulation and reconstruction of the LGM climate. With both similarities as well as apparent differences with regard to the external forcing and the climate, it can be useful to compare the climate of the two periods..."

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.

> Following the reviewer's comment, we have edited the abstract making it consistent with the conclusion and including the reference to model dependency.

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?

> NorESM family of models is based on CCSM4, and NorESM1-M is the first documented version of the family. NorESM1-F is developed based on the NorESM1-M version. To clarify, we've rewritten the first sentence in Section 2.1 as "The NorESM family is based on the Community Climate System Model version 4..."

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?

> The global ice sheet data used in our simulation was kindly provided by Lev Tarasov; the data documenting the individual ice sheets were either published separately (as cited in the manuscript) or unpublished (e.g. the Eurasian ice sheet). Regarding Barents Sea, intuitively it should be open with a 70 m reduction of sea level; however, it is likely that there could be grounded ice that actually closes the sea - geological evidence on the existence of grounded ice is sparse and not firm though, as mentioned in the manuscript. Given that the reconstructions are uncertain and the potential impact is significant, we have chosen to discuss this in more detail in the manuscript.

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?

> There is not any 'hard' threshold to evaluate the model drift, as far as we are aware of; this is true for both modern as well as paleoclimate simulations. We believe that the trends of the evaluated metrics are acceptably small, although we do think that a longer integration would certainly be an advantage. Unfortunately, we are limited by computational time to run the model longer. However, in the revised manuscript we have removed the sentence "For the NorESM MIS3

simulation, we deem the aforementioned global mean ocean cooling trend to be 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.

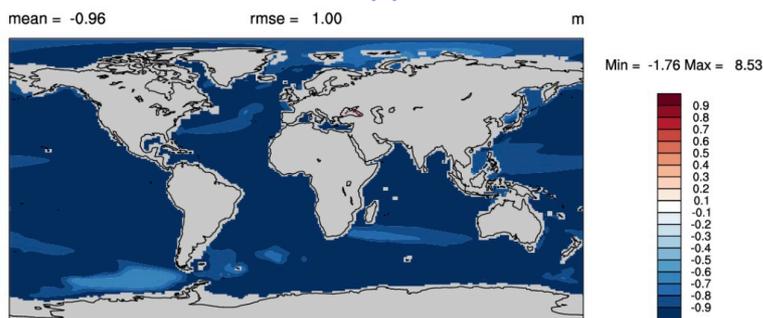
> We respond to the reviewer's comment regarding AMOC below.

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

> There are indeed MIS3 studies available for comparison. As discussed above, we have added the following to the revised manuscript: "For comparison, The CCSM3 MIS3 simulation (with 35 ka boundary conditions) by Merkel et al. (2010) reported a cooling of 3.4 °C, whereas using a different version of CCSM3 configured with 38 ka boundary conditions, Zhang et al. (2014b) reported a cooling of 3.5 °C. A stadial simulation with 44 ka boundary conditions (Brandefelt et al., 2011) shows a much cooler climate, e.g., 5.5 °C compared to the recent past."

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?

> The NAC shifts during MIS3 compared to that in PI can be seen from the sea level anomaly map shown below (MIS3 minus PI). The shift is not apparent from the barotropic stream function which is the vertical integration of volume transport for the whole water column, whereas NAC is located in the upper few hundred meters.



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

> This is mainly because a new river routing map is generated in our MIS3 configuration due to the change of land-sea mask, leading to large catchments and several arguably 'artificial' rivers flowing into the South China Sea. We've added the new river routing map in the supplementary material, and have also updated the original text accordingly as "The fresh surface water in the South China Sea during MIS3 is due to increased runoff, that is related to the newly generated river routing in this region owing to the change of land/sea mask."

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.

> Following the reviewer's comment, we have removed the LGM comparison here (see also our response to the reviewer's general comments). Instead we focus on comparison to MIS3.

We have added the following MIS3 reference to the revised manuscript: "Zhang et al. (2014b) reported a similar strengthening of AMOC during MIS3 (38 ka boundary conditions), e.g., 15.4 Sv which is 1.5 Sv stronger than their PI control simulation, and is much weaker than our simulated strength of AMOC at MIS3. Zhang et al. (2014b) also simulated a shallower upper cell of AMOC, in contrast to the NorESM simulation."

We have also included a comparison with Böhm et al (2015): "The AMOC during interstadials is accompanied by active deep water formation in the North Atlantic, with persistent contributions from the northern sourced water."

(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.

> Intuitively, one would expect a larger increase of Atlantic salinity, given a stronger AMOC and closure of the Bering Strait. However, it is possible that the MIS3 circulation can move salt across basins, for example from the Atlantic to the Pacific, causing the salinity increase in the North Atlantic < 0.6 g/kg. One possibility is that the enhanced AMOC leads to a larger salt export at the southern basin boundary, if the change in surface salt import from Indian Ocean does not fully compensate for the change in NADW related export. Klockmann et al. (2016) also showed less increase of salinity (relative to the global addition of salt due to sea level lowering) in the North Atlantic although with a stronger AMOC at LGM (c.f. their Fig. 5f). While the effect of closing the Bering Strait is clearly visible in the spatial SSS changes (Fig. 9) – showing e.g. a freshening in the Bering Sea and tendency to more saline Canadian Arctic and western North Atlantic – it is not unlikely our model underestimates this effect as a consequence of the model's routing of freshwater on land. The river routing map (added to the supplementary material) shows large catchments for the Canadian Arctic and North Atlantic and only a small catchment for northeast Pacific. Likely the lack of Atlantic salinity response can be mitigated by manually correcting/tuning the routing map (e.g. based on geological evidence) to route more freshwater into the North Pacific instead of Arctic, something we will consider in future studies. Another potential factor may be changes in the moisture transport across Central America. Given the size of the current paper, we think a more detailed

investigation of the hydrological cycle covering the above aspects should be better done elsewhere.

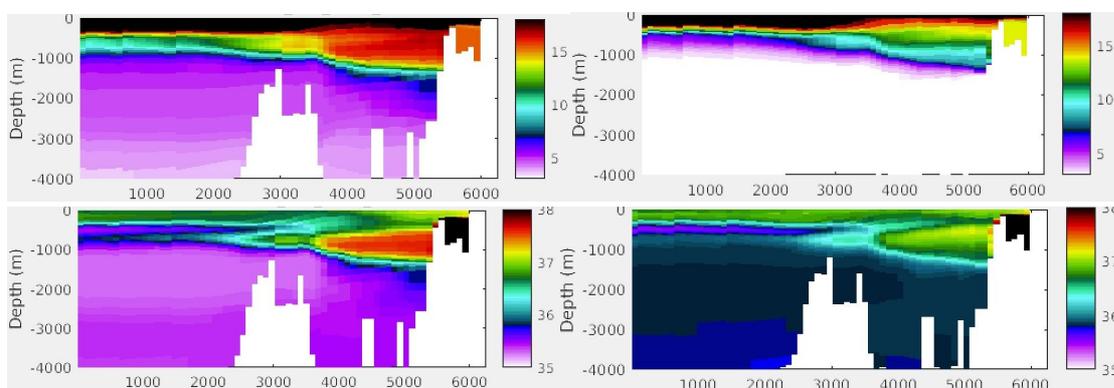
(3) If more warm NADW is present below 3000m, where does the very cold anomaly in the deep North Atlantic come from? Is that Overflow water then?

> First of all, the global ocean is colder during MIS3 than PI (e.g. 1.7 deg C). Also, AABW, although reduced in volume in the deep Atlantic, is colder and contributes to the cold anomaly therein.

It is a possibility that overflow water contributes to the cold anomaly in the deep North Atlantic since large parts of the Nordic Seas are not ice covered in the MIS3 simulation allowing dense water formation in high latitudes to continue.

(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.

> yes; the figure below shows a cross-section from the western Atlantic to the Mediterranean along the same latitude: left column - PI; right column - MIS3; top row - temperature; bottom row - salinity. One can see that the change of Mediterranean outflow during MIS3 contributes to the temperature/salinity anomalies at 500-800 m.



A weaker signature of Mediterranean outflow in the MIS3 simulation is plausible as evaporation over the Mediterranean basin is expected to be

reduced under the colder climate i.e. less outflow/inflow is necessary to maintain the freshwater balance of the Mediterranean Sea.

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

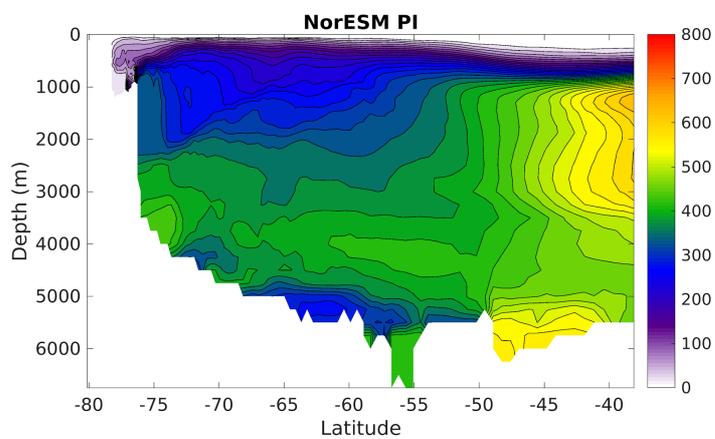
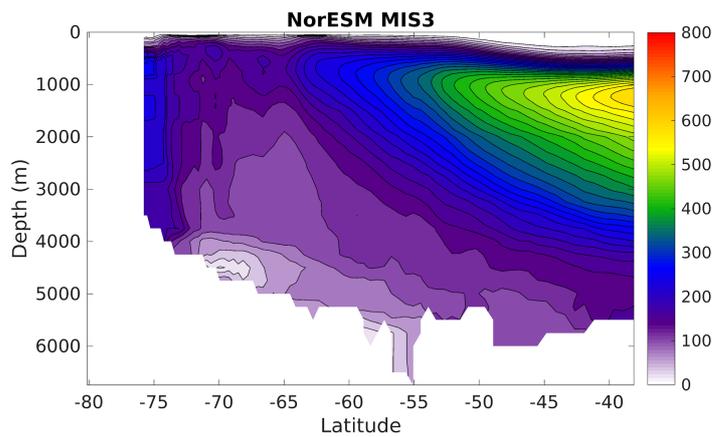
> Yes - it is the time since the water mass last made contact with the surface. We have included this in the revised manuscript.

(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.

> The reviewer is right. The increased sea ice formation and the associated brine rejection contribute to the formation and salinification of AABW, but one cannot directly link it to enhanced ventilation.

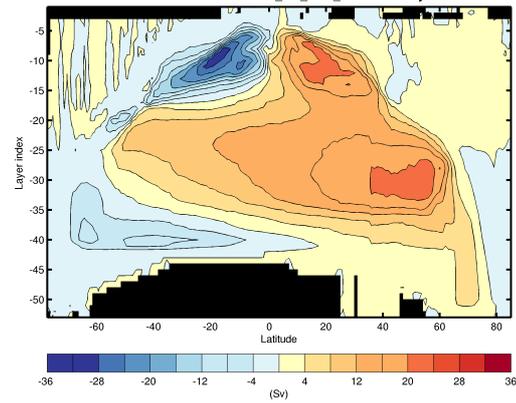
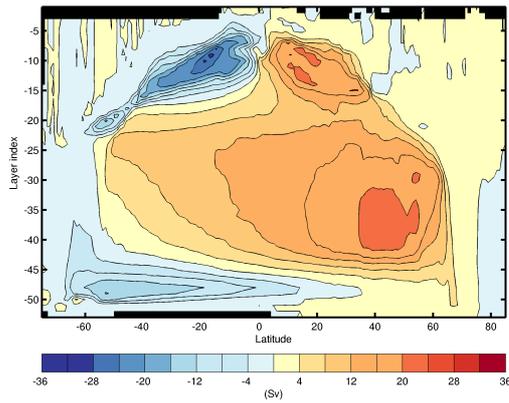
The model does show an enhanced ventilation in the Southern Ocean during MIS3, e.g., the figure below shows the zonal mean ideal age in the Southern Ocean for the MIS3 and PI experiments. It is evident from the figure that AABW is more ventilated during MIS3 compared to PI. The strengthening of ventilation can be caused by processes such as changes in air-sea heat/salt flux, wind etc.

We've therefore modified the text accordingly, and included the figure below into the supplementary material.

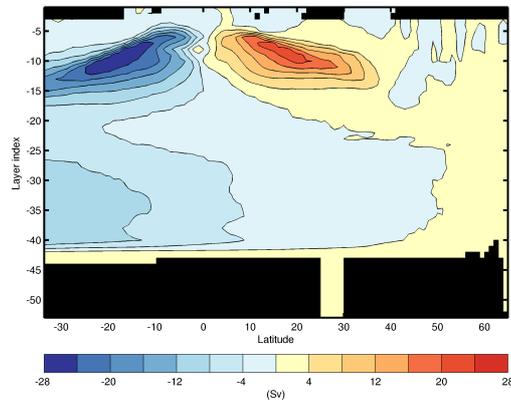
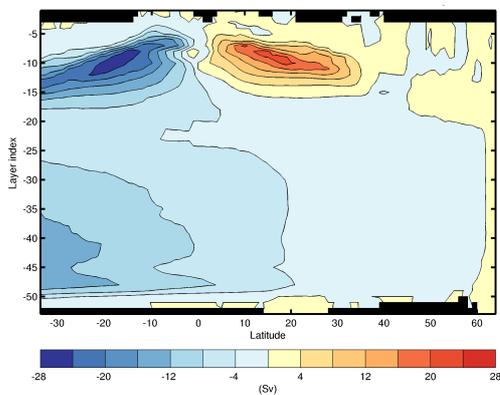


(7) If AABW ventilation and formation increases but the lower overturning cell weakens with less AABW reaching the North Atlantic, where does the AABW go? Is there more AABW in the Pacific?

> Yes. The lower overturning cell associated with AABW in the Atlantic is weakened, but globally it is strengthened - see below the global MOC in isopycnal space for MIS3 (left panel below) and PI (right panel below).



In the Pacific and Indian Ocean, it is also clear that MIS3 (left panel below) features a stronger deep cell associated with AABW compared to that in PI (right panel below).



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?

> Please see our response to comment (6) above.

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.

> Please see our response to comment (2) above.

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

> Yes, we agree that the MIS3 simulation by Xiao Zhang et al (2014) is close to disequilibrium, although this study explores the AMOC instability with the freshwater approach. We have added the following into the updated text: “In addition, with freshwater flux as the external forcing, the MIS3 simulation reported by Zhang et al. (2014b) is close to disequilibrium and model bi-stability.”

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.

> We agree with the reviewer that the sensitivity experiments are not long enough for quasi-equilibrium states. Computing resource is one concern. We terminated the sensitivity experiments with reduced ice sheet, as we did not see any sign/trend of further AMOC reduction and growth of sea ice for these two experiments. We also agree that the 140 ppm experiment, still with a declining trend, has the potential to reach a weak mode of AMOC. However, given that 140 ppm is already a very low level (e.g. compared to that in Zhang et al., 2017 & Klockmann et al., 2018) and that the sensitivity experiment has run for > 1000 years, it indicates that our simulated MIS3 climate is far away from the bifurcation point. We have added the following to the discussion: “..., despite that the simulations are limited in length (200 to 1000 years) and we therefore cannot fully exclude that further equilibration could bring the climate into a more unstable regime.”

It is true that we should not claim that the strength of AMOC is not affected in the sensitivity experiments. We have modified this in the updated manuscript, e.g. "... AMOC is only slightly reduced" for the ice sheet sensitivity experiments, whereas for the CO2 sensitivity experiments, "the AMOC, although weakened by several Sv, still remains strong".

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.

> Following the reviewer (and another reviewer)' comment, we have moved some text/figures to the supplementary material to enhance the readability of the manuscript. We have also added a map showing both the MIS3 and PI mixed layer depth (an indication of deep water formation region) into the supplementary material.

————— Technical comments: —————

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

> removed.

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

> We have rephrased the sentence as "Correlated with the rapid warming of Greenland temperature (up to 15 $^{\circ}$ C within a few decades during the stadial-to-interstadial transition), the North Atlantic and Nordic Seas are subject to abrupt climate transitions as interpreted from a number of marine sediment cores...".

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

> added, e.g. Maier-Reimer (1993), Maier-Reimer et al. (2005)

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.

> We've searched through the manuscript to make sure that different types of ice are distinguished.

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

> corrected.

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

> removed.

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

> revised.

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).

> We would prefer to stay with the current colour maps in this figure, as we would like to have a consistent colour map with previous NorESM evaluations, e.g., Bentsen et al. (2013), Guo et al. (2019).

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