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Climate of the Past Discussions

# *Interactive comment on* "Two ocean states during the Last Glacial Maximum" *by* X. Zhang et al.

**Anonymous Referee #2**

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This study assesses the consequences of using two different oceanic states to build glacial conditions to be used as initial condition for climate simulation studies. To this end the present-day Levitus climatology and a data set representative of glacial conditions are used. The corresponding climate states are found to differ both in terms of ocean circulation (with strong and weak Atlantic overturning circulations (AMOC), respectively) and tracer distribution. The second case (weak AMOC state) is found to agree better with glacial paloceanographic reconstructions. The response of the system to North Atlantic freshwater perturbations is found to differ considerably depending on the use of these two states as initial condition. Starting from the strong AMOC state, once the perturbation ceases, the AMOC recovers on centennial timescales to its initial value, while starting from the weak state the AMOC overshoots. The weak AMOC state is suggested to be unstable due to persistent upwelling in the Southern Ocean, which pushes it toward the strong AMOC state.



While external forcing and surface boundary conditions for the Last Glacial Maximum (LGM) have been thoroughly assessed in the past, up to now there has been no discussion as to what the initial ocean state should be. This study suggests the oceanic initial conditions could have relevant implications for i) the (quasi-equilibrium) state of the system, explaining the differences between the glacial AMOC simulated by different models within the Paleomodelling Intercomparison Project (PMIP), ii) its sensitivity to external pertubations, and iii) its stability. The experimental design is original and the results are very interesting. Nevertheless, I think several issues require improvement before the manuscript can be accepted for publication.

General main comments (see also the specific comments below):

1) The authors should try to frame better the context of the study. That is, is it relevant to all glacial abrupt climate changes or mainly deglaciation and why?

2) One of the main results is that the weak LGM AMOC is unstable due to persistent upwelling, but this is not illustrated in depth. The authors claim the strong AMOC state would be achieved in several thousand years starting from the weak one. I understand that due to computational limitations the runs cannot be continued that long, but additional results should be shown to support this claim.

3) The weak LGM AMOC is here only reachable through the use of specific (glacial rather than Levitus) initial oceanic conditions and unstable due to persistent upwelling in the Southern Ocean. However, both features (the unreachability and the instability) seem to be model dependent. Other models do seem to attain stable weak AMOC LGM conditions. The authors should comment on this.

4) The text requires a thorough revision to improve the writing. I have attempted to point out the required corrections, but I think this task rather corresponds to the authors.

Specific comments:

P 3016 (Abstract):

Lines 2-4 (this is related to my first general comment): The sentence “the understanding of the underlying dynamics is still limited, especially with respect to abrupt climate shifts” is unclear. The connection between deglaciation and abrupt climate shifts should be made clearer. If the authors are referring to the abrupt character of deglaciation this could be stated more clearly.

Lines 5-6: Verb is missing. I assume the authors mean something like “A fundamental issue is how to obtain an appropriate climate state”.

Lines 13-14: “Spatial configuration” of what? I suggest rephrasing as “spatial configuration and strength of the AMOC as well as its transient response to freshwater perturbation. . .”. Here I think it would be better to explicitly mention the more specific results in the abstract and their precise implications.

p 3016, line 26: I suggest rephrasing as “has found a variety of stability properties of the AMOC in conceptual models, which were subsequently confirmed in more comprehensive models”.

p 3017:

Line 1: Rahmstorf (2005) is not really a review, it is a model intercomparison. I suggest rephrasing.

Line 2: I suggest mentioning explicitly that this bistability is found under present climate conditions: as the authors mention on p 3022, several model studies suggest the hysteresis and stability properties of the glacial climate could be different from present.

Line 6: The sentence “Different responses of AMOC states... require the knowledge of the exact position of present climate” is not clearly formulated. I assume what is meant is that anticipating the response requires knowledge of the position of that state.

Line 10: The sentence “However, the principal difficulty for climate models is to determine the proximity of our present climate to potential thresholds (Rahmstorf et al., 2005)” is confusing, please clarify.



Lines 12-14 (also related to general comment 1): Is “Since the last glacial period” really meant here? What is the focus of the study? Deglaciation, glacial abrupt climate changes, or both? In the latter case, glacial abrupt climate changes are not restricted to the last glacial period. Also, to my knowledge there is not really much evidence for abrupt changes of the AMOC (that is, provided by ocean circulation proxies) besides that associated to deglaciation (McManus et al. 2004). Here, as in the abstract, the authors should be more specific. The references should be revised accordingly: it is somewhat inconsistent to refer here to a model study such as Knorr and Lohmann (2007), which although relevant for deglaciation is not a model study and thus not really providing observational evidence, which I assume is the intention here. Citing other reconstruction studies would be more appropriate.

Lines 27-29: I am not sure this statement can be made “According to the bifurcation theory”. The authors should clarify what is meant here or simply suppress the expression.

P 3017: Lines 3, 6: I suggest replacing “utilized” by the more simple term “used” Line 9: Insert “the” before “horizontal”

P 3018:

Lines 17-18: The statement “with respect to the blending product of ice sheet reconstruction, and changes in ocean bathymetry. . .” is confusing, please rephrase.

Line 20: I suggest inserting commas before and after “LGMW and LGMS”. Also, these terms indicate weak and strong AMOC, respectively. Even if at this stage it should not be evident one could anticipate it in order to make the connection more logical.

Lines 21-22: There is no verb here, I suggest replacing the full-stop before “One” by a comma.

P 3019:

Line 4: The text states “the fully coupled model was run 3000 and 3000 yr”, please

correct.

Line 10: Does lower than the preindustrial mean the preindustrial control run? If so, please specify.

P 3020

Line 2: Replace “relatively warming condition” by “relatively warm conditions”.

Line 6: Insert “the” before “Nordic Sea”.

Line 11: Insert “the” before Southern Ocean.

Lines 14-16: In this study the two water-mass configurations correspond also to two different LGM AMOC states, weak and strong. This can also be identified in the PMIP simulations shown here, with CCSM and HACDM showing relatively weak glacial AMOC, and MIROC and ECBilt-CLIO stronger ones. Is this the general case for other PMIP models? If so, it should be stated here in order to make a stronger case.

Line 24: Insert “the” before AMOC.

Lines 27: It should be mentioned explicitly that the stronger NADW and southern westerlies refer to stronger than present (PI).

Page 3021:

Lines 8: I suggest replacing “induced” by “expected”.

Line 16: replace “identical as” by “identical to”

Line 28: FWP could in principle be positive (generally referred to as hosing experiments) or negative perturbations. To make clear it is the first case I suggest adding the sign “+” to the 0.2 and 1 Sv.

P 3022:

Line 1: it should be stated explicitly where the FWP perturbation was added.



Line 2: I suggest replacing “between the two states” by “from one state to the other”.

Lines 5-7: I think a very interesting result of this study that is not well stressed is the fact that an AMOC overshoot can be obtained only by introducing a freshwater perturbation at the weak state, while this does not happen from the strong one. To my knowledge this is the first study which starting from a glacial climate perturbed with (positive) freshwater fluxes alone results in an AMOC overshoot (in Barker et al. 2010, Knorr and Lohmann 2007 and Liu et al 2009 the background climate was gradually changed from glacial to interglacial conditions). To understand the implications, it would be very interesting to see how North Atlantic (ideally Greenland) SATs are evolving. If the SAT change were abrupt, this study could point to a possible reason why coupled climate models might have failed to simulate glacial abrupt climate changes associated to an overshoot of the AMOC-

Lines 15-25: if I understand correctly this discussion, it implies that the LGMW state is not an equilibrium state, and that integrating the model further leads the system toward the LGMS state. This could be reflecting a long-term trend due simply to the fact that the model has not been integrated for long enough time, or a long-term oscillation as found in many simpler models and a few comprehensive ones (e.g. Haarsma et al. 2001, Meissner et al. 2008). The authors should mention what they believe is happening.

Line 26-28 (this relates to my general comment 2): here and in Figure 9’s caption the authors claim that LGMS would be attained starting from LGMW after several thousand years. In figure 9, 5000 years are specifically mentioned. The authors should explain how this estimation was calculated. I understand due to computational limitations the runs cannot be continued that long, but at least the AMOC timeseries should be shown for the whole period, before and after the perturbation, including the additional 500 years. Moreover, the same timeseries should be shown for the simulation starting from Levitus to show there is no comparable trend in that case.

P 3023

Line 8: suppress comma

Line 9: replace “was” by “were”

Line 13: replace “shows” by show

Lines 1-23 (related to my general comment 3): this discussion seems to imply that the LGMW state that provides a more realistic LGM ocean state might not be reachable simply by imposing glacial boundary conditions, but this result s eems to be model dependent (therefore the ‘might’). Other models do achieve steady weak-AMOC glacial states. Thus, the capability to reach this state would be model dependent. In relation to this, the authors suggest the persistent upwelling is the reason for the instability of the LGMW state, but the existence of models capable of achieving steady weak-AMOC glacial states would again suggest that the instability of this state is model dependent. Is this the case? The authors should discuss this issue, even if afterwards they provide with proxy data supporting their case.

P 3024:

Lines 1-3: “During the LGM” appears here twice, at the beginning and the end.

Lines 4-6: The authors claim the persistent upwelling could have lead to enhanced CO2 outgassing from the Southern Ocean, consistent with Anderson et al (2009) and Ahn and Brook (2008). But in their model the enhanced upwelling is apparently due to enhanced mixing, while Anderson et al (2009) claimed the CO2 increase was mainly wind-driven. The authors should comment on this.

Lines 12-13: The authors claim that at the last deglaciation the increase in CO2 could have fed back onto climate by facilitating the termination. This discussion seems to imply that the LGMW glacial state is inherently unstable due to the persistent upwelling in the Southern Ocean. Is that the case? If so, would it not be applicable to Dansgaard-Oeschger events? In relation to this, the authors should show whether the LGMS state



is or not stable.

Figures:

Figure 1: it would be helpful to show a third panel c) with a map of SST differences between LGMW and LGMS. I suggest including the timeseries of Greenland SATs as well as the AMOC throughout the perturbation experiments for both sets of initial conditions (see above).

References:

Haarsma RJ, Opsteegh JD, Selten FM, Wang X (2001) Rapid transitions and ultra-low frequency behaviour in a 40 kyr integration with a coupled climate model of intermediate complexity. Climate Dyn 17:559–570

McManus, J.F., Francois, R., Gherardi, J.-M., Keigwin, L.D., and Brown-Leger, S. 2004. Collapse and rapid resumption of Atlantic meridional circulation linked to deglacial climate changes. Nature 428: 834-837.

Meissner, K.J., Eby, M., Weaver, A.J. and O.A. Saenko, 2008: CO2 threshold for millennial-scale oscillations in the climate system: implications for global warming scenarios, Climate Dynamics, 30: 161-174. DOI: 10.1007/s00382-007-0279-0

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