Clim. Past Discuss., 8, C1513–C1518, 2012
www.clim-past-discuss.net/8/C1513/2012/

Climate of the Past Discussions

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

Anonymous Referee #1

Received and published: 12 September 2012

REVIEW OF "Two ocean states during the Last Glacial Maximum" by Zhang et al., submitted to Climate of the Past

The authors report results from two simulations of the Last Glacial Maximum with the same climate model. The two simulations are obtained by integrating the model for 3000 yrs with the same glacial boundary conditions (as defined by the PMIP3 protocol) but with different initial conditions. A first simulation is initialized from an ocean state that is constrained from a modern hydrographic climatology. The other simulation is initialized from an ocean state that is derived from a previous solution of an ocean-only model subject to glacial boundary conditions. For each simulation, average results for the last 100 years of model integration are considered. The authors find, among other results, that the two different initial ocean states lead to simulations with significant differences in water mass distribution and meridional circulation in the Atlantic Ocean.

I think that this manuscript (ms. hereafter) has scientific merit. The fact that different climate models subject to identical glacial boundary conditions produce solutions with sizeable differences in ocean circulation has been a puzzling result from model intercomparison projects such as PMIP2 (e.g., Otto-Bliesner et al. 2007; Weber et al. 2007). The present ms. seems to provide evidence that such differences may be due, at least partly, to differences in the initial conditions which have been assumed for the ocean component of the climate models. This appears as a significant result for the long-standing problem of the simulation of LGM climate. On the other hand, the present ms. suffers in my opinion from significant caveats which prevent me to recommend its publication in its present form. General and specific comments are reproduced below. I hope these will help the authors revise their interesting work.

GENERAL COMMENT

To my eyes, the major limitation of this ms. is that the physics behind its major result is not identified and discussed. Why do different initial ocean states used in climate model simulations forced with the same glacial boundary conditions lead to sizeable differences in water mass distribution and circulation? From my reading of the ms., I could not isolate the physical cause(s) for the dependence of the final equilibrium ocean state on the initial ocean state (I am here assuming that the final 100 years of climate model integration under glacial boundary conditions are indeed at or close to a steady state). The dependence is documented (with clear figures, it should be stressed), but the reader is unfortunately left without a coherent and clear explanation thereof.

The authors might want to explicitly address the following questions in a future version of the ms. First, could the dependence of equilibrium ocean state on initial ocean state in the glacial simulations be simply due to the fact that the ocean has not reached equilibrium with the imposed boundary conditions in these simulations? From my experience, the rate of approach to steady state in coarse-resolution models such as the one employed in the present study is determined by the vertical (or diapycnal) diffusion of buoyancy. Whereas baroclinic Rossby waves lead to a relatively quick adjustment of the temperature and salinity fields, the adjustment to vertical diffusion is very slow. The associated time scale is O(D**2/k), where D is a vertical scale of the motion and k a vertical (or diapycnal) diffusivity. Assuming D = 1000 m and k = 0.0001 m2/s, the time scale for vertical diffusion would be about 3 centuries. This value is lower by one order of magnitude than the integration time of 3000 years in the glacial simulations, suggesting that the ocean state in these simulations could be close to steady state. Nonetheless, the above estimate of the time scale of ocean adjustment to vertical diffusion is very approximate, and the scaling argument cannot definitively exclude the possibility that the temperature and salinity fields in the glacial simulations are still evolving after 3000 years of integration. In this case, the "memory" of the imposed initial conditions would not have been completely lost. I think that this possibility should be discussed explicitly in a future version of the ms.

Second, the ocean model used in this study is a primitive equation model that includes the non-linear terms associated with the advection of horizontal momentum, temperature, and salinity (Marsland et al. 2003). Are non-linear terms ultimately responsible for the two ocean states obtained with identical glacial boundary conditions, as intuition gained from conceptual models would suggest? As it is well known, the occurrence of multiple equilibria in Stommel's (1961) two-reservoir model, which might represent a low latitude ocean and a high latitude ocean, is due to the fact that the flow between the reservoirs is set to vary with the density contrast between them. Could the two ocean states reported in the present ms. be related to similar physics (associated to density advection) or to physics illuminated in subsequent but still conceptual models of ocean circulation?

Finally, could the authors suggest an explanation for why pre-industrial simulations do not show the same level of dependence upon initial ocean state as that of the glacial simulations (p. 3021)? Overall, I think that the ms. would greatly benefit from a dynamical interpretation of the model results, based on a deeper analysis of these

results and (or) of results from other numerical experiments. Idealized experiments with the model could be conducted and analyzed in order to isolate the physics, in coupled models, behind the dependence of ocean equilibrium state upon ocean initial state for different climate boundary conditions.

SPECIFIC COMMENTS

1) It is stated at several places in the ms. that elements of the Atlantic circulation during the LGM, such as the meridional overturning circulation and the formation of NADW, were different from the modern according to sediment data (e.g., Abstract, p. 3021, p. 3023). Consequently, the authors seem to imply that the glacial simulation with shallower Atlantic meridional overturning circulation (AMOC) is more consistent with sediment data and is thus to be preferred. However, inversions of sediment data have consistently shown that the bulk of these data do not require a circulation state that is different from the modern, if due consideration is given to data and model errors (LeGrand and Wunsch 1995; Gebbie and Huybers 2006; Marchal and Curry 2008; Burke et al. 2011). Whereas an inversion of benthic foraminiferal d13C and Cd/Ca data showed that these data are consistent with a shallower AMOC (Winguth et al. 2000), these authors did not explore whether the same data could also be consistent with another circulation state. In a review of sediment data and model studies (forward and inverse), Lynch-Stieglitz et al. (2007) concluded that the AMOC "was neither extremely sluggish nor an enhanced version of the present-day circulation", although "evidence from multiple water-mass tracers supports a different distribution of deep-water properties, including density, which is dynamically linked to circulation" (their Abstract). Given our incomplete understanding of the AMOC at the LGM, the authors should probably exert caution when discussing the possible (in)consistency of their glacial simulations with sediment data.

2) Admittedly, I cannot see the value of the freshwater perturbation experiments in the current context (section 3.3). These experiments are time-dependent solutions and do not seem to provide any direct insight into the causes for the differences in ocean

states between the two glacial simulations, LGMs and LGMw (which again I assume are equilibrium or quasi-equilibrium states). Unless the rationale for the freshwater perturbation experiments can be clarified, I would suggest that section 3.3 be removed from the ms.

3) It is concluded from the simulations reported in the ms. that the ocean state during the LGM was a transient state (p. 3024). Although it is certainly plausible that the glacial ocean was not in equilibrium with glacial boundary conditions (say, at 21 kyr BP), I cannot see evidence for this transient character in the ms. Again, unless the evidence can be clarified, I think that the conclusion should be removed from a future version of the ms.

TECHNICAL CORRECTIONS

I think that the writing could be improved in many places. Numerous suggestions have been included in a hard copy which has been sent to the editor handling the ms. (Dr. V. Rath).

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Interactive comment on Clim. Past Discuss., 8, 3015, 2012.