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Interactive Comment

Interactive comment on "The modern and glacial overturning circulation in the Atlantic oceanin PMIP coupled model simulations" *by* S. L. Weber et al.

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General comments

This manuscript definitely deserves publication in Climate of the Past Discussions, albeit after some revisions.

This manuscript reviews previous results from nine coupled model simulations that participate in the Paleoclimate Modeling Intercomparison Project (PMIP). The central issue is the response of the Atlantic meridional overturning circulation (MOC) to forcing factors characteristic of the Large Glacial Maximum (LGM). By drawing their results not just from one, but a number of models - General Circulation Models (GCMs) as well

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as Earth-System Models of Intermediate Complexity (EMICs) -, the authors put their study on a much wider basis than previously possible. The authors find that in these models during the LGM the MOC is mainly controlled by the density contrast between Antarctic Bottom Water (AABW) and North Atlantic Deep Water (NADW), and that it is less influenced by the net surface freshwater flux (or net evaporation) over the Atlantic Ocean.

G1 Having the "general reader" in mind, I think it would be helpful to be more explicit on the purpose of this paper. Is it a realistic simulation of the LGM thermohaline circulation? In this case a detailed comparison to the available proxy data would be needed. Is it (what I suppose) mainly a model intercomparison exercise? This would be a perfectly valid purpose, but it would require some further explanation on what a model intercomparison project is about and more details on the participating models to have an higher impact (on the expert reader as well).

G2 The authors state in the abstract that "the simulation of the Atlantic thermohaline circulation (THC) provides an important benchmark for models used to predict future climate changes". Similarly, they claim in the introduction that "the simulation of the glacial climate provides an important test for general circulation models (GCMs) used to predict future climate changes." I of course have written sentences like these. However, we know that the forcing factors were very different (e.g. low CO_2 and the presence of ice sheets in the case of the LGM, high CO_2 in the case of future climate changes). What then in the opinion of the authors is the relevance for future climate change?

G3 I think in writing about the meridional overturning circulation (MOC), we should not neglect the large body of literature that discusses its ultimate driving processes (see e.g. the recent review by Kuhlbrodt et al. 2006). A (near-) steady state such as the LGM cannot be maintained by surface buoyancy fluxes alone. Wunsch (2002, 2003) therefore states that the MOC is often mislabeled as the "thermohaline" circulation. With respect to the study of abrupt climate change, Wunsch (2006) raises a number

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of concerns that may also be relevant with respect to the study of the LGM: (1) In his view, existing climate models do not have the resolution, either vertical or horizontal, to properly compute the behaviour of fresh water and its interaction with the underlying ocean. (2) Some models still use the physically unappropriate salt-flux boundary condition instead of a real freshwater flux boundary condition (Huang 1993). (3) Almost all models are run with fixed diffusion coefficients.

Possibly all participating models (maybe except for the CCSM3.0 model) lack the resolution to adequately simulate the response to changes in continental runoff. A number of models probably still uses the salt-flux boundary condition (so do the models I use...). A few models may invoke vertical diffusion coefficients that depend on stability, but do they take into account changes in the energy available for mixing from winds or tides?

G4 Kamenkovich and Goodman (2000) developed a scaling relationship for AABW transport that may be relevant for this paper. According to their theory, if the vertical diffusivity is constant, then the AABW transport T_a depends on the vertical extent of the flow H_a and the vertical density contrast $\delta\rho$. I wonder to what degree their results apply to the LGM values shown in Figure 5. Kamenkovich and Goodman (2000) also mention surface salinity at the Antarctic coast as one factor that determines $\delta\rho$. In this connection, if I may do so, I would like to mention that we (Paul and Schaefer-Neth 2003) explicitly investigated the role of Antarctic (actually, Weddell Sea) sea-surface salinity on the Atlantic MOC.

G5 About "cause and effect": In five out of nine models the strength of the MOC appears to be positively correlated with the density of AABW at its source region. In their conclusions, the authors call the density difference between AABW and NADW "a major controling factor". However, in my opinion a correlation or scaling relationship alone does not prove a causal relationship. In an equilibrium situation, how can we attribute cause and effect to the various processes at work?

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G5 A convincing case for a relatively larger contribution of AABW as compared to NADW was made by Duplessy et al. (1988), Labeyrie et al. (1992) and Sarnthein et al. (1994) based on low δ^{13}C values below 2500 m depth between 20° and 45°N. Since AABW cannot cool below the freezing point, it must have been saltier than today today to balance the higher density of NADW; this conclusion was first drawn by Zahn and Mix (1991) based on δ^{18}O values of benthic foraminifera. I think that these studies deserve to be mentioned in addition to the more recent work by Curry and Oppo (2005) and Adkins et al. (2002).

However, passive tracers that have largely unknown end-member values such as δ^{13}C provide almost no information about oceanic volume transport, as pointed out by Legrand and Wunsch (1995) and made very clear by Rutberg and Peacock (2006).

Specific comments

S1 The authors state that the simulations “have been integrated long enough to have the deep ocean to adjust to glacial boundary conditions” (page 926, line 7). It would be desirable to state how long they have been integrated and what the remaining temperature and salinity trends in the deep ocean were.

S2 In the light of **G1**, **G3** and **S1** I recommend to expand Table 1 and include the following information:

- not only the name, but also the version of the model used
- references for the individual control and LGM simulations
- the explicit range of the horizontal resolution (in °) and vertical resolution (in m)
- the type of surface boundary condition for salinity, and whether flux corrections were used
- whether or not a rigid lid or a free surface was employed

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- whether or not the vertical diffusion coefficient depends on stability
- the length of the simulation.

S3 With respect to Equations 1 and 2, I wonder if they equally apply to models that use a salt-flux boundary condition for salinity, and models that use a real freshwater flux boundary condition for salinity (and usually have a free surface). What is the reference salinity S_0 – is it the global or Atlantic basin mean salinity of the model in question? Actually, I could not find Equations 1 and 2 in the paper by Rahmstorf (1996). His Equation 7 for the overturning component of freshwater transport reads:

$$F_{\mathrm{OT}} = -\frac{1}{S_0} \int \overline{v}\overline{S}\, dz \; . \tag{1}$$

S3 How do the values for $M_{\mathrm{az}} = 0.38$ Sv and $M_{ov} = -0.20$ Sv cited from Weijer et al. (1999) compare to more recent estimates by, e.g. Wijffels (2001)?

S4 With respect to Figure 3, I wonder how the simulated salinity profiles compare to observations. Where are the watermass or circulation boundaries in these plots? In this connection, how do the simulated density gradients ρ_{atl} and ρ_{SN} compare to observations?

S5 The authors state that “that the response of the freshwater budget at 21 kyr BP is more determined by oceanic processes than, by for example, changes in precipitation, river run-off or sea-ice formation”. But what factors would ultimately drive the formation of AABW in the Southern Ocean - would it not be the net sea-surface heat and freshwater fluxes, in particular in the Weddell Sea?

S6 The authors conclude that “most models exhibit increased stability during the LGM” (p. 935). It is not clear to me what this conclusion is based on. Reference is made to a hysteresis diagram, but no hysteresis diagram is shown. Furthermore, it can probably only be computed for the most efficient among the participating models.

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S7 The altered river pathways and an atempt to account for the mass balance of the continental ice sheets in the HadI2 run possibly make it more realistic than other runs, including the HadI1 run. Therefore it think that the role of “net evaporation” over the Atlantic basin should still be considered seriously.

S8 Rahmstorf (1996) as well as Rahmstorf et al. (2005) state that there is no unique definition for an absolute value of the freshwater flux. How can this be reconciled with the claim that the ECBilt and UVic models “are close to the bifurcation point where the collapsed THC exists” (p. 940)?

Technical corrections

T1 Occasionaly, the wording appears to be bit awkward and makes it difficult to understand the contents of a sentence: “the mechanism put forward in the literature for a glacial THC reduction in one model also plays a dominant role in other models” (abstract). The density of AABW “is determined by the balance between the opposing effects of salinity and temperature on the density of AABW versus that of NADW” (abstract). “The THC mostly shoals (deepens) in those simulations that show a decrease (increase) in THC strength” (p. 928). “The intrusion of AABW increases (decreases) for decreasing (increasing) strength of the Atlantic overturning in all models [...]” (p. 928).

T2 How can the content of Table 3 be presented in a more intuitive way? The way it is done now I find rather confusing.

T3 Personally, I find the annotations to the axes in Figures 4 and 5 somewhat “cryptic”. I suggest to use the same symbols in the annotations as in the figure captions and the main text. Furthermore, I think more care must be taken in distinguishing between a density difference (e.g. ρ_{atl}) and its change (e.g. $\Delta\rho_{\mathrm{atl}}$).

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