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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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We thank the referee for his thoughtful and extensive review and will follow most of his suggestions for improvement in the revised paper. The referee comments are repeated below, together with a point-by-point reply.

GENERAL COMMENTS

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).

The primary purpose of this paper is to carry out a model intercomparison study, focussing on the MOC response to glacial conditions. This is the first study of its kind, as LGM simulations with coupled AO-GCMs are only feasible since a few years. Models show a wide range of responses and mechanisms are not well understood. Therefore, we restrict this study to the description of the responses and analysis of the underlying mechanisms. This purpose is better defined (in the introduction section) in the revised paper.

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 CO2 and the presence of ice sheets in the case of the LGM, high CO2 in the case of future climate changes). What then in the opinion of the authors is the relevance for future climate change?

There is a direct relevance in validation of models through paleoclimatic simulations, in the general sense that we can have higher trust in models that can handle climatic change well. It is still under debate whether it is possible to draw direct inferences from the LGM climate for the future, as forcing factors are indeed different - see e.g. Hargreaves et al. (2006). We make this twofold relevance (or lack thereof) more explicit (in the introduction section) in the revised paper.

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 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?

We acknowledge that this is an important point: studies with existing climate model remain primarily as indicators of processes that can be operating, but with no evidence that they dominate (Wunsch, 2006). Our conclusions thus hold for the reality of climate models rather than the real world climate system. A caveat (with reference to Wunsch) is given in the revised paper, while we have changed the label "thermohaline" to "meridional overturning" circulation. However, we feel that an extensive discussion of this point is beyond the scope of the paper, precisely because of the large body of conflicting theories.

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 Ta depends on the vertical extent of the flow Ha and the vertical density contrast. 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 the vertical density contrast. 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.

It would certainly be of interest to compare the change (LGM minus control) in the strength of the deep reversed cell to changes in the vertical density contrast and the vertical extent of the deep northward flow in the 9 models. We will examine this scaling relationship and report the results in the revised paper, if relevant. We will also examine to what extent changes in Antarctic surface salinity determine this scaling relationship as well as the consistent signal in Fig. 5, bottom left (with reference to relevant papers).

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?

Obviously, the existence of a scaling relationship alone does not prove a causal relationship. However, we do note that in some models (eg CCSM and ClimC for the density contrast between NADW and AABW, Hadl2 and MRI for net evaporation) only one such scaling relationship exists while other factors are absent or show the reverse relation. In these cases definitive conclusions can be drawn (assuming that we did not overlook important mechanisms). In other cases, attribution is less certain. However, it seems likely that density contrasts determine the flow rather than vice versa and we do believe that scaling relationships are a strong indication of a causal relation. Rephrased in the revised paper. Establishing "causal" relationships isn't the purpose of an intercomparison paper. However, the presence of strong correlations derived from intercomparison analyses may stimulate further studies in this direction.

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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 13C values below 2500 m depth between 20 and 45N. 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 18O 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 13C 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).

We agree and will rephrase the short description of the data in section 3.

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.

Integration times differ from model to model, as do spin-up strategies. Some models start from a cold state of a previous model version, some from a warm state (section 2). Total integration times have limited information value, as the switch to a new model version can induce substantial drift in an equilibrated glacial state. In our experience the deep ocean temperature reaches equilibrium first, while the deep ocean salinity seems to have a longer adjustment timescale. We compute the Atlantic freshwater budget (section 4), which results in an estimate of the imbalance for the control and the glacial state. Fig. 4 shows clearly that most models have equilibrated. However, some models definitely cannot have equilibrated in the deep ocean salinity. This will be rephrased in section 2 of the revised paper, with reference to section 4.

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

In the revised paper we will for each model include a reference for the analysed LGM state, if it is published already, or a reference for the control state. Also, we indicate in the Table for which models outputdata are available in the PMIP database. In this manner we link to all the relevant model information, without expanding the present paper with technical details. The reason for not including these details in the paper is that it is not clear how possible differences between models (in eg diffusion coefficients, surface boundary conditions) would impact the results or how it would help in the interpretation of those results. Many technical aspects influence the models response to glacial boundary conditions, a complete evaluation is outside the scope of this paper. Model version and the use of flux corrections, which has direct relevance for the budget analysis, are included in the revised section 2.

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 S0 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).

These equations can be found in Weijer et al. (1999), who show that the value for the reference salinity does not influence the depth-integrated values of Mov and Maz. A constant value of 34.7 PSU was used for all models. This reference is added near Eq. 1 and 2 in the revised paper. All models actually use a salt-flux

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boundary condition.

S3 How do the values for Maz = 0.38 Sv and Mov = 0.20 Sv cited from Weijer et al. (1999) compare to more recent estimates by, e.g. Wijffels (2001)?

Wijffels (2001) gives the transport at 30S wrt the transport at 60N, so values are not directly comparable to Weijer et al. (1999). However, this is not an update but based on the same "raw" data: values from Weijer et al. (1999) are based on Holfort (1994, PhD thesis), while Wijffels (2001) uses Holfort and Siedler (1997; basically the later paper version of the same PhD thesis).

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/contrasts compare to observations?

We will look into this using Levitus temperature and salinity data.

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?

Yes, local surface fluxes must ultimately drive AABW and NADW formation. However, the Atlantic freshwater budget (integrated over the basin) shows larger changes in the oceanic transport terms than in the surface flux term.

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.

This conclusion is based on the change in Mov (LGM minus control value) in

most models, see under S8. We do not compute hysteresis diagrams, but use results from the cited references to infer the position of each model on the hysteresis curves.

S7 The altered river pathways and an attempt to account for the mass balance of the continental ice sheets in the Hadl2 run possibly make it more realistic than other runs, including the Hadl1.5 run. Therefore it think that the role of "net evaporation" over the Atlantic basin should still be considered seriously.

All models account for the mass balance of the continental ice sheets (assumed to be in equilibrium). Most models do this by imposing zero growth (a fixed limit of 1-2 m for snow depth). When the snow depth exceeds this limit, excess snow melts and becomes run-off. This results in a time-varying freshwater flux to the oceans adjacent to the continental ice sheets. In Hadl2 a time-mean freshwater flux is prescribed, calibrated during the spin-up phase so that a long-term zero growth is achieved. Net evaporation maybe an important factor, but in the present set of models its changes are mostly too small to play a decisive role. We note here that the present version of the paper contains a value of 0.13 Sv of the Hadl2 prescribed freshwater forcing for the LGM. However, this is a value used during a certain phase of the spin-up run. The value for the equilibrated state is given in the revised paper, it is 0.054 Sv for the LGM (and 0.033 Sv for the control state).

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)?

Rahmstorf (1996) notes that the bifurcation between the mono-stable branch and the bi-stable branch is not exactly at the value Mov(30S)=0. However, it is hypothesized to be very close to this value. Numerical experiments with intermediate-

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complexity models (Vries and Weber, 2005; Marsh et al., 2006) have confirmed this hypothesis. A more theoretical approach is taken by Dijkstra and Weijer (2006, submitted to Tellus), who show that the bifurcation occurs at Mov(30S) minus Mov(60N). The latter term results in a small offset, confirming again that the bifurcation is close to Mov(30S)=0. Based on these studies, we propose Mov as a diagnostic for the existence of the OFF state in a model. Our claim that the ECBilt and UVic models "are close to the bifurcation point" is simply based on the value of Mov for the LGM in these models. This will be stated more clearly in the revised section 4.5

Surprisingly, Rahmstorf et al. (2005) do not consider the value of Mov in their model intercomparison study of hysteresis behavior, stating that it is unclear what determines the position of each model (on the mono-stable or bi-stable branch).

TECHNICAL CORRECTIONS
T1 Occasionaly, the wording appears to be bit awkward and makes it difficult to understand the contents of a sentence.

We will see to this.

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.

It took some time to work out this Table. We agree that it is complex and are open to suggestions for improvement.

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 and its change).

We will see if this can be improved.

REFERENCES

Hargreaves, J.C., Abe-Ouchi, A., and Annan, J.D.: Linking glacial and future climates through an ensemble of GCM simulations, Climate of the Past Discussions, 2, 951-977, 2006.

Marsh, R., Hazeleger, W., Yool, A., and Rohling, E.: Stability of the Thermohaline Circulation under millennial CO_2 forcing and two alternative controls on Atlantic salinity, Geophys. Res. Letters, under review, 2006.

Vries, P. de, and Weber, S.L.: The Atlantic freshwater budget as a diagnostic for the existence of a stable shut-down of the meridional overturning circulation, Geophys. Res. Letters, 32, L09606, DOI 10.1029/2004GL021450, 2005.

Interactive comment on Clim. Past Discuss., 2, 923, 2006.