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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Climate of the last glacial maximum: sensitivity studies and model-data comparison with the LOVECLIM coupled model" *by* D. M. Roche et al.

D. M. Roche et al.

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Response to general comments from anonymous reviewer 3 and to reviewer A. Paul

Comments raised both by the anonymous reviewer 3 and by Dr. A. Paul are closely related to each other, and mainly concern the oceanic circulation we simulate at the LGM. We will therefore respond to general comments from reviewer 3 and Dr. A. Paul jointly. Specific comments are treated separately.

One common concern expressed by the two reviewers is the lack of discussion of the obtained LGM circulation with the δ^{13}C data from Curry and Oppo, 2005. This is

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modified in the revised version in which we discuss the implication of the δ^{13}C to our simulated oceanic state.

As mentioned by Dr. A. Paul, δ^{13}C distribution at the LGM *may indicate the boundaries between the different water masses* and the *strong vertical gradient in the* δ^{13}C *as compiled by Curry and Oppo (2005) is indicative of an LGM water mass boundary between deep and bottom water shallower than today*. Reviewer 3 also expressed this concern, stating that *in my understanding, delta 13C data [...] suggest that the overturning cell associated with NADW formation becomes shallower at LGM than the present*. We agree that δ^{13}C data from the LGM indicates that there is a (strong) boundary between an upper and a lower water mass between 2 and 3 kilometers. However, which water mass is the upper one and which is the lower one is subject to discussion. Especially as *passive tracers with uncertain end-member values (such as* δ^{13}C *) provide limited information about circulation boundaries, as pointed out most recently by Rutberg and Peacock (2006)*. The "classical" oceanic circulation view of the LGM indicates that the Atlantic shows the interplay between two water masses: an upper one called GNAIW (Glacial North Atlantic Intermediate Water, to differentiate it from the deeper present-day NADW) and a deeper one called GAABW (Glacial Antarctic Bottom Water) assumed to originate from the Antarctic with much lower δ^{13}C values. This latter water mass has very low δ^{13}C values as it is formed from pre-formed waters with low δ^{13}C values and they are formed mainly under sea-ice preventing considerable ventilation with the atmosphere.

How does our LGM state compare with such a sketch? We do not have two water masses in the Atlantic, but three: one Antarctic deep water mass (GAABW) and two northern water masses, a dense one formed in the Nordic seas (GNADW) and one formed south of Iceland (GNAIW). As in the "classical" case, the end-member values are unknown. It is however noteworthy that the waters formed in the Nordic seas are formed in sea-ice covered regions. If the δ^{13}C was simulated in our model, this water mass ought to have quite low δ^{13}C content. Therefore, our Atlantic ocean is indeed partitioned, as seen from the δ^{13}C (Curry and Oppo, 2005), between an upper,

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well ventilated water mass (GNAIW) and a lower less ventilated water mass. This latter is composed of a mixture of GNADW and GAABW, depending on the latitude we are looking at (mainly GAABW until 20 degrees North, more GNADW northward). Our simulated state, although with a deep north Atlantic ventilated from the northern hemisphere does not seem to contradict the δ^{13}C data, at least from a qualitative point of view.
It should be noted that the overturning stream function indicates how the ocean is ventilated (in our case, indicates that the deep northern Atlantic is ventilated from the Nordic seas) but is not directly linked to the δ^{13}C distribution (this latter being a passive tracer, see comment by A. Paul). Therefore, the fact that deep waters formed in the northern hemisphere are reaching the bottom of the Atlantic ocean is not contradictory with data evidences, provided that the lower part of these waters are formed in a way to provide low δ^{13}C content. As discussed already in the paper, *the 231Pa/230Th evidence may indeed argue for a meridional overturning of NADW stronger than today in the upper 3km of the Atlantic Ocean, but weaker than today below* (A. Paul).
We therefore believe for all these reasons (which are more detailed in a new version of the manuscript that the LGM circulation we obtain in LGM ocean is not contradictory with data evidences. However, we agree with both reviewer 3 and A. Paul that stating – as we did in the previous version of the manuscript – that the circulation obtained is "close to what the oceanic circulation was like during the LGM" is an overstatement (corrected in the revised version of the manuscript).

Another common concern of the two reviewers is the link between the sensitivity experiments and the rest of the paper. Both reviewers are asking for a comparison between the states obtained in the sensitivity experiments and the paleodata, to evaluate what state better fit the data. However, it should be bear in mind that the sensitivity experiments do not represent valid LGM states, in a physical sense. Indeed, we are modifying some of the parameters well out of the range of plausible physical values (e.g. 50% reduction/increase of the wind drag on sea-ice). Therefore, even if

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one of the states could provide a LGM closer to the data, it would not be a "LGM" in the strict sense and therefore could be use only as an indication of the processes at work. Even so, it is worthwhile to compare to the data, and we added a paragraph in this sense to the revised version of the paper. The goal we pursued was to try to find out a state with a different balance between the Atlantic water masses, closer to the "classical" LGM. The result is that the model doesn't present any state coherent with such picture close by in the parameter space.

Specific Response to A. Paul

G1 (pursued). *the differences in the deep water characteristics between the model and as inferred from the pore water data by Adkins et al. (2002) should not be disregarded.*. We agree that more emphasis should be put on the fact that we do not simulate correctly the "water masses properties" (temperature and salinity) when compared to pore water data. A paragraph has been added in the revised version of the manuscript to outline possible explanation for this discrepancies, which may lie in the resolution of the model (and thus in the small scale processes accompanying the formation of sea-ice). We conclude by stating that "deep waters are not formed with the correct temperature and salinity" in the model. The conclusion of the data-model comparison for the Atlantic water masses now states that "although the characteristics of the deep water masses we obtain are substantially different from data inferences (northern sourced being too warm and southern sourced being not saline enough) the circulation pattern is not inconsistent with evidence from the proxy data".

G2 We do agree that a total THC shutdown would be required to obtain a (totally) different surface climate. However, we think it is good to state it there, as that might no be obvious to all readers. The sensitivity experiments are designed to look at the sensitivity of the overturning to various model settings.

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S1 We agree that the sentence mentioned was not clear and modified it to "we conclude that the simulated oceanic circulation is not inconsistent with ocean circulation proxy data, although the characteristics (temperature, salinity) are not in full agreement with water mass proxy data".

S2 *Can the authors maybe quote some related work on data assimilation that proves the significance of the "mean surface climate" for the "consistency of the simulated [climate] state" (Abstract, p. 1106, lines 18-19, p. 1107, line 2, and elsewhere), at least to some degree?* The reference (now included) to Paul & Schäefer-Neth, Quaternary Science Reviews, vol. 24, pp. 1095-1107, 2005 would fit in here.

S3 *Is it really that "most" PMIP models produce an increase in overturning rate under glacial boundary conditions (p.1115, line 25)?* As noted by anonymous reviewer 3, this is an overstatement. We corrected the manuscript with the proper mention that 50% of the PMIP models produce an increase in overturning rate under LGM boundary conditions (Weber et al., 2006, now published in this issue).

S4 *It is certainly true that the mean state may not be the most appropriate view of the ocean (p. 1116, lines 4-6), but deep-sea sediment core data such as 13C probably provide a long-term average*. We did not mean to use the variability of the ocean as a mean to compare to deep-sea sediment core data, but more from a modelling point of view. We agree that the sentence was awkwardly formulated, and is modified in the revised version.

S6 *What would be the implied changes in the ocean circulation if the land climate indeed showed a larger continentality (p. 1120, line 9) - even denser NADW? Would*

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the agreement with the MARGO SST/sub-surface temperature data deteriorate (cf. p. 1121, line 20, underestimation of sea-ice cover)?. We cannot answer quantitatively this particular point. The increased continentality leads to strange effects in the model at the ocean to atmosphere interface, so the model cannot be integrated long enough for the ocean to equilibrate. But we would expect the summer sea-ice edge to move southward and therefore the agreement between model and data to deteriorate, as noted by A. Paul.

S7 *The authors may be right to claim that they "correctly simulated the entrance of north Atlantic waters in the Nordic Seas", but in the data, these Atlantic waters appear to penetrate much farther north (p. 1126 and Fig. 8b - the Norwegian Current appears to reach as far North as Svalbard)*. As the temperatures are colder in the model north of Norway in the Nordic seas, no clear gradient can be seen, or at least, less clear than in the data. However, using the zero northward advection of the upper water masses shows that there is indeed northward water transport until Svalbard in the model, as seems to be the case in the data. This is now mentioned in the revised version of the manuscript.

S8 *Could the mismatch between model and data in the deep North Atlantic Ocean also be interpreted as a too small contribution of AABW rather than deep water formed in the Nordic Seas (p. 1128, lines 23-25)?* The mismatch in temperature between the data and the model indicates that there should be a colder water mass at 2km in the deep north Atlantic ocean than there is in the current simulation. The dominant water mass there is the one formed south of Iceland, which seems to be too warm with respect to the data. The cause is difficult to assess but two options can be stated: either this water mass should be replaced by a colder one (e.g. GNADW or GAABW) or the formation of GNAIW should be slightly different, on a seasonal basis for example. This is modified in the revised version of the manuscript.

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S9 *The hysteresis loop is an important background information on the sensitivity of a climate model, but can the authors express more clearly what the benefit is for this particular study (Section 5.2, pp. 1131)?* This is also noted by the anonymous reviewer 3. As additional freshwater fluxes are not the topic of our present study, we chose to remove this section from the revised version of the manuscript.

S10 *It would be helpful for the comparison and discussion of the model results to also show the meridional overturning streamfunction of the control experiment LH_CTRL (Figure 3).* We agree with A. Paul on this point. However, as the present-day climate is not our work, we provide this figure as an additional figure in an Appendix. The scale of the figure used for LH_CTRL is the same as for the LGM, to allow an easy comparison between the two.

We further modified the manuscript to comply with the technical suggestions provided.

Interactive comment on Clim. Past Discuss., 2, 1105, 2006.