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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.

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This manuscript presents a simulation of the Last Glacial Maximum (LGM) and a number of sensitivity experiments using an Earth-System Model of Intermediate Complexity (EMIC). The EMIC is the successor of the ECBilt-CLIO model, contains a dynamic vegetation component with two plant-functional types (PFTs) VECODE and is now called LOVECLIM. The first part of the manuscript provides a comparison of the LGM simulation with paleo-proxy data, the much shorter second part describes a series of sensitivity experiments.

A method is presented that relates biomes to PFTs, which turns out to be useful in

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a comparison with proxy data. Accordingly, a broad discussion of the LGM climate in terms of the seasonal amplitude of surface air temperature, hydrological cycle and vegetation cover is given.

A surprising result is the decrease of Antarctic Bottom Water inflow into the Atlantic Ocean from 7 Sv in the control simulation to 2.6 Sv in the LGM simulation, or by 60%, which apparently is not in contradiction with the available "ocean circulation proxy data" (i.e. ^{231}Pa/^{230}Th), but in conflict with the traditional interpretation of "water mass proxy data" (e.g. δ^{18}O, δ^{13}C).

I mainly recommend to reconsider the conclusions and check to what degree they are supported by the model-data comparison. It is well possible that the model is in agreement with some proxy data, but at odds with some other. Furthermore, I think there is a great chance in extending the model-data comparison to the series of sensitivity experiments, at least with respect to a sub-set of the proxy data. Finally, I suggest to add two more figures (the meridional overturning streamfunction of the control experiment and the difference between the simulated and reconstructed SST/sub-surface temperature in the northern North Atlantic Ocean).

General comments

G1 I think that we must carefully distinguish between current or circulation boundaries (e.g. between northward flowing surface and thermocline waters and a southward flowing deep western boundary current in the Atlantic Ocean) and water mass boundaries (e.g. between North Atlantic Deep Water/NADW and Antarctic Bottom Water/AABW). These boundaries are certainly related to each other, but they are not quite the same.

While a circulation boundary corresponds to a level of no motion (which cannot be inferred from the meridional overturning streamfunction based on the zonally-averaged flow), a water mass boundary is associated with a strong gradient in water mass properties and affected by horizontal and vertical mixing as well as end-member values (these gradients may even differ between different water mass properties).

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). However, they may indicate the boundaries between the different water masses. In this respect, I take the strong vertical gradient in the δ^{13}C as compiled by Curry and Oppo (2005) as indicative of an LGM water mass boundary between deep and bottom water shallower than today.

To my knowledge, the ^{231}Pa/^{230}Th ratio is a proxy for the meridional overturning rate of NADW, not of AABW. Thus, as outlined by the authors, the ^{231}Pa/^{230}Th evidence may indeed argue for a meridional overturning of NADW stronger than today in the upper 3 km of the Atlantic Ocean, but weaker than today below. However, it does not exclude a stronger AABW inflow, nor is it inconsistent with an LGM circulation boundary between deep and bottom overturning cells shallower than today.

Thus it is true that the "circulation pattern is not inconsistent with evidence from the [^{231}Pa/^{230}Th] proxy data" (p. 1129, lines 9–10), but the differences in the deep water characteristics between the model and as inferred from the porewater data by Adkins et al. (2002) should not be disregarded. Both data sets are equally sparse, both give rather indirect clues about past circulation and water mass characteristics, but *a priori* we cannot trust one data set more than the other.

In the light of this conflicting evidence, I do not think that the authors may conclude that their LGM "mode of ocean circulation [...] is not at odds with evidence from the proxy data" and might be "close to what the oceanic circulation was like during the LGM" (p. 1133, lines 18–20).

The LGM simulation may be consistent with the available "ocean circulation proxy data" (but how about the results for the Florida Straits from "paleo-geostrophy" calculations by, e.g. Lynch-Stieglitz et al. 1999?), but not with the "water mass proxy data".

G2 Unless the meridional overturning of NADW breaks down completely, as conjectured for a strong "Heinrich" event, I do not expect a large (on the order of several °C)

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influence of the mode of ocean circulation on the global surface climate (cf. Seager et al. 2002, but see also Rhines and Häkkinen 2003). Hence, it would not come as a surprise if “the mode of ocean circulation [...] if incorrect” would “not drastically impede the quality of the surface climate simulated” (p. 1133, lines 15 and 17).

However, I do expect some influence in the proximity of the deep and bottom water formation regions in either hemisphere. Variable rates of NADW formation should have an effect on the climate of the North Atlantic realm. Thus the sensitivity experiments should not be discussed in isolation, but also be compared to paleo-data evidence. I suggest to compare the regional surface climate of the sensitivity experiments with the reconstructed North Atlantic SST/sub-surface temperatrure and biomes in western Europe and on the east coast of North America.

Specific comments

S1 “we conclude that the balance between water masses obtained is consistent with the available data although the specific characteristics (temperature, salinity) are not in full agreement” (Abstract, lines 16–18): The model does only carry temperature and salinity as ocean tracers. If model and available proxy data are not in “full agreement” with respect to these two tracers, in what sense can “the balance between water masses obtained” be “consistent with the available data”? Do the authors rather mean the agreement between the meridional overturning of NADW in their LGM simulation and the ^{231}Pa/^{230}Th 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?

S3 Is it really that “most” PMIP models produce an increase in overturning rate under glacial boundary conditions (p.1115, line 25)?

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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 δ^{13}C probably provide a long-term average.

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 the agreement with the MARGO SST/sub-surface temperature data deteriorate (cf. p. 1121, line 20, underestimation of sea-ice cover)?

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 nort as Svalbard). Furthermore, it would be great if the authors could add a true difference plot (model minus data).

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)?

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)? Furthermore, may the "off" state be unstable under LGM boundary conditions because the internal variability is in some way unsually high (Fig. 9)?

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).

Technical corrections

T1 P. 1110, line 22: atmospheric greenhouse gas (not: gases) concentrations

T2 P. 1110, line 26: Change "Modification of the insolation parameters is set as for 21

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kyrs B.P.” to, e.g. “Orbital parameters correspond to 21 kyrs B.P.”.

T3 P. 1111, line 11: seasonal (not: seasonality) amplitude

T4 P. 1111, line 15: no space before comma

T5 P. 1113, line 15: Change to, e.g. “the simulated changes in vegetation cover are responses (not: respond) to the cooler and dryer (not: coooling and drying) conditions.”

T6 P. 1116, line 13: Incomplete sentence, starting with “paragraph”

T7 P. 1118, line 12: not (instead of: no) simulated tree cover

T8 Fig. 3, p. 1147: “meridional” with small “m”

T9 In most figures, I find the numbers at map boundaries, color keys or contour lines generally too small to be readable with ease.

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