

Interactive comment on “How did Marine Isotope Stage 3 and Last Glacial Maximum climates differ? Perspectives from equilibrium simulations” by C. J. Van Meerbeeck et al.

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Using an Earth system model of intermediate complexity the authors performed simulations of several glacial climate states, including LGM and "typical" MIS3. They found that due to smaller ice sheets, higher level of GHGs and less atmospheric dust, MIS3 climate was in average appreciably warmer than LGM. In addition, due to different orbital configurations, MIS3 climate was characterized by a much stronger seasonality in the Northern Hemisphere. The authors also found that small differences in the concentration of GHGs and dust forcing between stadial and interstadial states have a minor impact on climate and cannot explain large temperature differences inferred from pa-

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leoclimate records. The authors concluded that their simulated MIS3 climate is more similar to typical interstadial conditions. The only way the authors found to simulate climate similar to typical stadial state was "to kill" completely the Atlantic meridional overturning circulations by adding a large anomalous freshwater flux into the North Atlantic. I believe, this is a rather interesting paper and I would recommend it for publication in CP after a moderate revision.

General comments

1. In the last paragraph of the paper the authors stated: "Our findings contribute to understanding the mechanisms behind Dansgaard-Oeschger events and their frequent recurrence during MIS3". Actually I do not believe this paper has much to do with the mechanisms or recurrence of DO events. After all, DO event were abrupt warming events recorded in the northern North Atlantic and Greenland. At the same time, the MIS3-HE experiment described in the paper is a standard "water hosing" experiment which simulates a cold (not warm) event. Numerous experiments of this sort have been performed already during the recent decade with different models (including AOGCMs) for different magnitudes of freshwater perturbations, locations and different climate states. In this respect, the only novelty of the reviewed work is that the author performed their water hosing experiment for the realistic MIS3 boundary conditions. Since the authors did not describe similar water hosing experiments for modern and LGM states, it is impossible to conclude from this work how important (if at all) is the background climate state for climate response to the shutdown of the THC. From several papers reporting water hosing experiments performed with the comprehensive AOGCMs for present-day and LGM conditions it does not seem that climate response to the shutdown of THC differs dramatically even between these two extreme climate states. With this I do not want to say that the new water hosing experiments are useless. They are useful at least because they demonstrate time and again to the remaining skeptics that the Atlantic thermohaline circulation is an important player in the climate system. However, the water hosing experiments neither can explain abruptness of temperature

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rise during the onset of DO event, nor the transient character of the warm phase of DO events. In addition, simulated in water hosing experiments temperature change over Greenland is usually considerably smaller than that derived from paleoclimate records for the stadial-interstadial temperature change in Greenland (8-15C). I suspect that the same is true for the differences between MIS3-HE and MISS3-sta experiments. And, obviously, water hosing experiments cannot explain the recurrence time of DO events.

2."With Labrador Sea convection in our MIS3 simulations, the sensitivity of the Atlantic Thermohaline Circulation to freshwater forcing should be different from LGM" (Page 1139). I agree that it should, but how much and in which direction? Was the THC during MIS3 more or less sensitive to the freshwater flux as compared to LGM? The issue of sensitivity of the THC to freshwater forcing was not address in the paper at all. It is not even known whether there is any significant difference in the THC sensitivity between present day and LGM climate states in the LOVECLIM model.

3."For this reason, we argue that LGM should not be used to simulate DO events. Rather, one should start from a climate state obtained under MIS3 boundary conditions". In this case I must disagree. Why necessarily MIS3? DO events occurred not only during MIS3 but also during MIS2 (DO2 event occurred just before LGM), and MIS4, and MIS5, and during previous glacial cycles and, probably, during most of Pleistocene excluding interglacials. Therefore, the major challenge is to find the mechanism which can explain such robustness of DO events. As far as the earliest studies of DO events are concerned, indeed, they (for example Ganopolski and Rahmstorf, 2001; hereafter GR01) were performed using LGM conditions, simply, because these boundary conditions were readily available. However, the authors should be aware that in our more recent works (e.g. Ganopolski, 2003 and Claussent et al., 2005) we simulated DO events within a broad range of the Northern Hemisphere ice sheets size/volume, and found that DO events are rather robust phenomenon in our model. At the same time, Wang and Mysak (2006) simulated DO events within a range of different climates by varying CO₂.

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3. Last paragraph on page 1136. I think there is a certain misunderstanding here. Firstly the author stated that they "infer from" their "results that transitions between stadials and interstadials involve changes in Atlantic THC". One cannot infer that from such a study. At best, one can conclude that climate change resembling reconstructed difference between stadial and interstadial states can be reproduced by changing the THC strength. Obviously, this is not the prove that the THC was the cause. Secondly, the authors wrote that in their model a strong freshwater perturbation is required to cause a shutdown of the THC which, they believe, is consistent with Prange et al. (2002) but not with our (GR01) results. That is not correct. Just compare our Fig. 1 with Prange et al. Fig. 2. In both models, a complete shutdown of the THC requires freshwater flux of about 0.1 Sv which is not a small perturbation by any means. A similar threshold for the glacial circulation was reported in Weber and Drijfhout (2007) in the ECBilt/CLIO model. Therefore, in respect of a complete shutdown, I cannot see any difference between CLIMBER-2 and EMICs based on OGCMs (see also intercomparison between different EMICs in Rahmstorf et al., 2005). The point is that in GR01 to explain DO events we proposed a completely different mechanism from the traditional concept of transitions between "off" and "on" (or strong and weak) states of the THC. In our work, DO events are explained as the transitions between two STRONG modes of the THC, "cold" (stadial) and "warm" (interstadial), which primarily differ by the locations of deep water formation and the amount of heat transported in the Atlantic ocean from middle to high latitudes. In our model, under glacial climate conditions (not necessarily LGM, see e.g. Ganopolski (2003)) this type of transition does not require a large perturbation in freshwater flux (unlike a complete shutdown which we associated with Heinrich events). Whether this type of the THC transition also exists in 3-D AOGCMs remains to be seen.

4. Experimental design. It is not clear from the paper how different GHGs concentrations were derived for "stadial" and "interstadial" experiments, especially for CO₂, which has fundamentally different temporal dynamics from the DO cycle. Secondly, is it correct (according to the Table 2) that radiative forcing of dust at every location was

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0.2 of its LGM value during "interstadials" and 0.8 of LGM value during stadials. In other world, the global radiative forcing of dust was change by factor four between stadial and interstadial conditions and follows Greenland record? I think this strong assumption requires some justification.

5. Some part of the paper, especially section 3 is hard to read because it is overloaded with numbers. I think, a number of numbers can be reduced easily because not all of them are equally important. In addition, several sentences is hard to understand. As an example (page 1126, lines 1-4): "The geopotential height is reduced by down to 500m²/s², leading to an increase in clockwise wind motion of up to 60% between the anomalous low and anomalous highs over Greenland (+200m²/s²) and Northern Russia (+300m²/s²)". Please read this section with fresh eyes and try to make it a bit more reader-friendly.

6. Figures 3, 4 and 7 is hard to read. Only with 200% zoom it is possible to see details. Please enlarge these figures and, if possible, use a more distinguishable color sequence instead of automatically generated one.

Specific comments

Page 1116, line 13. "July being 4C warmer". Which temperature is meant here?

Page 1116, line 6. I think, it would be better to use here the term "ice sheet mass balance" instead of "ablation" because the latter refers only to surface melt.

Page 1118, line 25. Please specify the latitude for which insolation numbers are given in the text.

Page 1119, line 5. Reference Pollard and Barron (2003) is absent in the list of references.

Page 1129, lines 13, 14. "Interestingly, glacial differences in atmospheric GHG and dust concentration do not affect the temperature in the same order of magnitude as ice sheet and orbital configuration do". Please be specific in what you mean under "glacial

differences in atmospheric GHG". Glacial differences in GHGs do affect temperature appreciably if we compare glacial and interglacial climates. If you are talking about two MIS3 experiments, then small temperature differences are absolutely not surprising because prescribed differences in GHGs and dust cause a rather small radiative forcing (my guess is about 1 W/m² globally).

Page 1130, line 3. VECODE model requires also precipitation as input. Is it true that evapotranspiration in LOVECLIM does not depend on surface (vegetation) type?

Page 1133, lines 12-14. "MIS3 climate was less sensitive to the GHGs ... than to other potential forcings". The term "sensitivity" has a clear meaning in climate science and is not applicable in this context. Instead, it would be better to say that temporal variations in GHGs and dust during MIS3 were less important than other climate forcings.

Page 1133, line 13. "Our finding are consistent with Baron and Pollard". I do not think Baron & Pollard papers are directly comparable with the reviewed work. Baron & Pollard did not even mention DO events (or stadials and interstadials, or abrupt climate change). In fact, they compared two time slices - 30 and 42 KaBP. Therefore they did not make any conclusion about the role of CO₂, orbital forcing and ice sheet size in driving abrupt climate changes. Moreover, they did not even consider changes in GHGs assuming that they are just too small to be important.

Page 1136, line 2, 3. All three references are not relevant for climate response to freshwater perturbation. I would suggest to cite here the papers describing water hosing experiments, e.g. Zhang and Delworth (2005), Stouffer et al. (2006), Hu et al. (2008), etc.

Page 1136, lines 30. "This was at least so without additional freshwater supply". This is not correct. To get this transition we had to apply a small negative freshwater flux. (See our stability diagram in Fig. 1b in GR01).

Page 1137, line 17-18. Water hosing experiments for present-day and LGM conditions

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have been performed not only with simple models but also with several state-of-the art coupled climate models.

"...in this study, we have shown that ... climate varies greatly with different forcings and boundary conditions" (page 1137) and "With the result presented in this study, we know that insolation cannot be neglected as an important factor of glacial climate" (page 1140). Are these really NEW findings?

Table 3, two upper lines. Make "6" superscript.

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