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Climate of the Past Discussions

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Interactive Comment

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.

C. J. Van Meerbeeck et al.

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We would like to thank Dr. Ganopolski for his thorough review. Below we provide a detailed reply to all comments.

General comments

Referee's general comment (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

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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 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.

Reply: The referee rightfully mentions that our study does not involve explaining the recurrence of DO events, as has also been mentioned by Anonymous Referee #2. Therefore we have modified the last paragraph of the conclusions which now reads: "Our findings contribute to understanding the mechanisms behind Dansgaard-Oeschger events. In our model, the cold state with freshwater forcing is more consistent with observed stadial climate than the one without. In this view, stadials would be colder intervals in an MIS 3 state generally warmer than LGM. We need to design physically consistent climate modelling experiments based on boundary conditions that are realistically representing the period of interest. We confirm once more that insolation differences in a glacial are important, which we have shown for MIS 3 as compared to LGM." With respect to mechanisms, our study introduces the possibility of an amplification of Greenland temperature change during MIS 3 through presence or absence of Labrador Sea sea-ice and convection, at least in our model. If DO events involve displacements of the southern limit of sea-ice in the North Atlantic, then the above finding may explain part of the underestimation of Greenland temperature change in simulated glacial abrupt climate events (e.g. Flückiger et al., 2008, Clim. Dyn.). Moreover, in our model it seems that our MIS 3 states, which are in quasi-equilibrium with the forcings, resemble interstadial climate, suggesting that stadials are perturbation of a generally mild climate during MIS 3. While we agree that equilibrium experiments cannot be designed to directly study mechanism involved in transient climate change as DO events and HE events, our MIS3-HE experiment merely shows that this quasi-equilibrium climate state is more consistent with climate conditions associated with stadials, especially when HE events punctuated these stadials. With respect to differences between freshwater forcing sensitivities of LGM and MIS 3, we have conducted a so-called hysteresis experiment in which a freshwater flux which very slowly increases until a shutdown of the AMOC is reached and then slowly decreasing the flux until AMOC resumption is reached. The changes in freshwater flux are slow in order to keep climate in quasi-equilibrium. In our simplified exercise, we ran through the hysteresis loop in 20,000 model years. We can confirm the referee's expectation that in LOVECLIM, the sensitivities of the LGM and MIS 3 AMOC strength to freshwater forcing are nearly equal. Shutdown from a strong AMOC is reached at around 0.22Sv; while resumption takes place around 0Sv in the collapsed AMOC section of the hysteresis loop. As no clear differences were found, we did not include the results of these experiments in the manuscript.

Referee's general comment (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.

Reply: As already mentioned in the reply to referee's general comment (1) above, the variability of the AMOC strength does not change markedly (in terms of hysteresis) between LGM state and MIS 3 states. However, the regional climate effect of presence or absence of convection in the Labrador Sea is of influence when one is interested in Greenland temperature change. We modified the sentence quoted by the referee and the following sentence in the text, which now read: "With Labrador Sea deep convection in our MIS3 simulations, the east-west structure of the Atlantic Thermohaline Circulation was different from the LGM case. The regional climate of the Labrador Sea area and surroundings (including Greenland) could become more sensitive to meltwater perturbations. Investigating this sensitivity is beyond the scope of the paper and is the subject of an ongoing study."

Referee's general comment (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.

Reply: We agree with the referee that DO2 occurred shortly before the LGM. DO2 is characterized by a relatively small temperature rise into Greenland Interstadial 2 as seen in Greenland ice cores. To avoid misunderstanding and improve clarity, we use the comment of Referee #2 who stated that no DO events occurred during LGM. We therefore modified our statement on p. 1117, lines 14-18 into the following: "It is

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presently not clear, however, why DO events were so frequent during MIS 3, while being nearly absent around the Last Glacial Maximum (LGM). Here, the LGM is considered to be the period between roughly 21 and 19 ka ago with largest ice sheets of the last glacial. Therefore, we analyse in this paper some characteristic features of the MIS3 climate and compare them to the LGM climate, using climate modelling results." As in our model no deep convection takes place in the Labrador Sea in the LGM state, but does in the MIS 3 states, the climatic character of simulated DO events in our model may be different in the key region of Greenland, where DO events have been defined.

Referee's general comment (3) continued 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.

Reply: Ganopolski (2003), Claussen et al. (2003) and Jin et al. (2007) employ the CLIMBER 2 model also used by Ganopolski and Rahmstorf (2001) to simulate transient climate change under changing MIS 3 insolation and ice sheet extent/topography. We have employed the key results of Ganopolski (2003) in our revised discussion sections 4.4 and 4.5. In response to the referee's general comment (3), the second paragraph on p. 1136 has been revised and now reads: "We infer from our results and other studies (e.g. Ganopolski and Rahmstorf, 2001) that climate change resembling the observed differences between stadials and interstadials can be obtained when changing the Atlantic THC, through the strength of meridional overturning in the North Atlantic. In our MIS 3 climates, a relatively strong freshwater perturbation is required to alter

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the Atlantic THC. Our findings are corroborated by those of Prange et al. (2002), who found that in an ocean general circulation model, the glacial THC can only remain slowed down or shut down with a strong additional fresh water flux. In the experiments of Ganopolski and Rahmstorf (2001) based on an LGM reference climate, imposing a strong freshwater flux of 0.1Sv resulted in a shutdown THC, while only a small negative forcing was imposed to obtain their warm and strong simulated interstadial THC mode, respectively small positive forcing for their cold (but strong) simulated stadial THC mode. In the stadial mode, convection was confined to the North Atlantic south of the sea-ice margin, while no NADW was formed at high latitudes. However, the LGM winter sea-ice extent may not have been as southerly in the MIS 3 background climate as during LGM. Consequently, convection possibly would not have been confined to the North Atlantic, but also present in more northern locations as the Nordic Seas as is found in our model. Ganopolski and Rahmstorf (2001) obtained Nordic Seas convection in their interstadial mode, as sea-ice retreated northward. More alike their stadial situation, in our MIS3-HE, winter sea-ice cover pushes more southward at some locations in the North Atlantic than in the LGM. In Ganopolski (2003), the simulated MIS 3 stadial states strongly resemble that of Ganopolski and Rahmstorf (2001), while using transient MIS 3 forcings as opposed to LGM forcings in the earlier study. Their results imply that the southward extent of sea-ice during stadials does not depend on insolation changes or ice sheet size. In our fully three-dimensional model, however, southward winter sea-ice extent is strongly asymmetric between the Labrador Sea and the Nordic Seas, the latter being partly ice-free in the LGM state (Roche et al., 2007). Compared to the LGM, in our model the sea-ice cover in MIS3 is less extensive, with a partly ice-free Labrador Sea in winter and a more northerly positioned sea-ice edge in the Nordic Seas. This implies that the sea-ice cover and the ocean state depend on varying glacial insolation and ice sheet size changes." In addition, we inserted an extra paragraph after paragraph 1 of section 4.5 on p.1137 which reads: "A first attempt at modelling glacial abrupt climate events in a physically consistent way was undertaken by Ganopolski (2003), Claussen et al. (2005) and Jin et al. (2007) in an earth system model of intermediate complexity incorporating a two-dimensional ocean model. By applying transient MIS 3 forcings, they obtain a Greenland temperature evolution not unlike the observed changes associated with DO events. In their model, the simulated DO events are a robust phenomenon under a broad range of NH ice sheet volumes. However, their exercise could be improved by applying all known boundary condition changes. Furthermore, employing three-dimensional Ocean General Circulation Models would provide insight on longitudinally asymmetric changes in overturning, e.g. the presence or absence of Labrador Sea convection."

Referee's general comment (3bis) 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.

Reply: See our reply to Referee's comment (3) continued and revised sections 4.4 and 4.5 of the manuscript.

Referee's general comment (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 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 changed by factor four between stadial and interstadial conditions and follows Greenland record? I think this strong assumption requires some justification.

Reply: The referee rightfully points to the different temporal dynamics of CO2 concentrations compared to stadial – interstadial transitions. CO2 concentrations do not necessarily follow the Greenland temperature curve. Rather, they may follow Antarctic temperatures, although temporal resolution of the CO2 record (Indermühle et al., 2000) does not preclude higher CO2 concentrations during Greenland Interstadials 8 and 14. The dust forcings imposed at each grid cell were calculated as a single multiplication factor of 0.2 for MIS3-int and 0.8 for MIS3-sta of the LGM dust forcing map from Claquin et al. (2003). We recognise that the very much simplified parameterisation introduced in the methods of this manuscript has been devised empirically. To further clarify the selection of GHG concentrations and the parameterization of the dust scale factor, we have modified paragraph 2 of p.1122, which now reads: "MIS3-sta (MIS3-int) was additionally forced with average MIS 3 stadial (interstadial) atmospheric GHG concentrations and top of the atmosphere albedo due to elevated atmospheric dust concentrations (see Table 1). The GHG concentrations we used in the setup

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of MIS3-sta and MIS3-int are based on typical concentrations found in the ice core records for stadials, respectively interstadials 8 and 14 (Indermühle, 2000; Flückiger et al., 2004). We made a stack of all records during these intervals and selected the (rounded) mean value of the spline functions in the stadials, respectively interstadials as final GHG concentrations. The very much simplified dust forcing was calculated by multiplying the grid cell values of the LGM forcing map of Roche et al. (2007) with an empirical dust factor corresponding to a best-guess of the average atmospheric dust-content (following the NGRIP d18O record–NorthGRIP Members, 2004) during an MIS 3 stadial or interstadial. The factor is inferred from an exponential transfer function of the NorthGRIP d18O record (we derived Eqs. 1-3), which explains most of the anticorrelation between the NorthGRIP dust and d18O records. The dust factors are based on findings of Mahowald et al. (1999) and Mahowald et al. (2006) that, on average, globally the atmospheric dust content was about five times lower during interstadials compared to full glacial conditions. In the Greenland ice core records, dust concentration peaks during stadials did at times attain LGM values. Applying this to our parameterisation, would give a dust factor of 1 in such cases. However, averaged over the duration of a stadial, the dust content seems slightly lower than 21 ka ago. Therefore, we opted for a stadial average of 0.8. The transfer function is:"

Referee's general comment (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.

Reply: We have taken into account the referee's advice, and reduced the amount of numbers given in the text of section 3. Specifically, we have removed the num-

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bers which can be clearly seen from the figures or tables, unless they improve the understanding of the manuscript. The sentence quoted by the referee now reads: “The geopotential height is reduced by down to -500m2/s2. Around this anomalous low, an increase in clockwise wind motion of up to 60% occurs between the anomalous low and anomalous highs over Greenland and Northern Russia.” Other sentences throughout the manuscript have been rewritten to improve readability.

Referee's general comment (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.

Reply: We request full page figures, which should more than double the size of the figures published in CPD.

Referee's specific comments

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

Reply: Here, the Northern Hemisphere July Surface Air Temperature is meant. The sentence has been modified and now reads: "A striking feature of our MIS3 simulations is the enhanced Northern Hemisphere seasonality, July surface air temperatures being 4°C warmer than in LGM."

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.

Reply: We have modified the text and have added the reference P.U. Clark et al., 2007.

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

Reply: It concerns July insolation at 65°N. We modified the text as such.

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

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Reply: We have added the Pollard and Barron (2003) reference to the list and cross-checked any possible further inconsistencies between references in the list and those in the text.

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).

Reply: The glacial GHG and dust concentration differences mentioned in the quote by the referee refer to differences within MIS 3. We have made the change in the text and removed the mentioning of “Interestingly”, as indeed, the little effect is not surprising.

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?

Reply: In our version of LOVECLIM, VECODE is not coupled to ECBilt through evapotranspiration. The vegetation cover feedback to ECBilt is through surface albedo changes.

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.

Reply: We have modified the text as advised by the referee.

Page 1133, line 13. "Our finding are consistent with Baron and Pollard". I do not

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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.

Reply: In response to the referee's comment, we have modified the sentences on p. 1133, lines 13-20. They now read: "We conclude that temporal variations in GHG and dust concentrations were less important during MIS 3 than other potential climate forcings. It is thus very unlikely that GHG and dust concentration changes played a major role in explaining temperature changes during MIS 3. Barron and Pollard (2002) and Pollard and Barron (2003), who did not change CO2 forcing from LGM in their simulations, concluded that the temperature difference between LGM and MIS 3 conditions registered in the records could not be explained by variations in orbital forcing or in the Scandinavian Ice Sheet size. In contrast, decreasing North Atlantic and Nordic Seas SSTs between a warmer and a colder state to simulate an extended southward distribution of sea ice, explained part of range of temperature differences between the two states."

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.

Reply: We have removed all three references from the text and replaced them by Knutti et al. (2004), Stouffer et al. (2006) and Flückiger et al. (2008).

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).

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Reply: We have revised paragraph 2 of page 1136 in response to referee's general comments (3) and (3bis).

Page 1137, line 17-18. Water hosing experiments for present-day and LGM conditions have been performed not only with simple models but also with several state-of-the art coupled climate models.

Reply: We have modified the text and the sentence p. 1137, line 17-18 now reads: "Moreover, due to computational costs, only simple models have been used so far in transient experiments of glacial abrupt climate change (Ganopolski, 2003)."

"...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?

Reply: As mentioned in our reply to referee's general comment (1), we have modified the last paragraph of the conclusions, the text of which can be found in that reply.

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

Reply: We have made the suggested correction.

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