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Climate of the Past Discussions

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 the referee for the thorough review. Below we provide a detailed reply to all comments.

Specific comments

Referee's specific comment (1) Page 1119, lines 12-18: "Barron and Pollard (2002) and Pollard and Barron (2003) ... could therefore not explain the mechanisms behind the oceanic circulation changes seen in data between stadials and interstadials." This was not quite the purpose of those studies. Cf. Barron and Pollard (2002), the Stage 3 modeling effort was devoted to investigating the impact of boundary conditions on the MIS3 climate and to producing MIS3 simulations that best fit the climate reconstruc-

tions. As a matter of fact, with respect to the millennial-timescale variability, this is the maximum one can do with equilibrium simulations: to identify the most important climate components and forcings, and propose configurations of parameters (boundary conditions) resulting in the best agreement possible between simulations and data.

Reply: We have changed the paragraph containing the sentence the referee quotes from page 1119, lines 12-18. The sentence now reads: "Barron and Pollard (2002) and Pollard and Barron (2003) concluded that MIS 3 variations in orbital forcing, Scandinavian Ice Sheet size, and CO2 concentrations could not explain the differences between a cold state and a milder state registered in the records." We changed the words interstadial and stadial in the next sentence by "the milder and the colder state". We avoided the use of the terms "stadial" and "interstadial" throughout the text when referring to what Barron and Pollard (2002) and Pollard and Barron (2003) called "cold" and "warm" states. Finally, we changed the last sentence of this paragraph, which now reads: "Their experiments were thus not designed to explain the mechanisms behind the oceanic circulation changes seen in data between stadials and interstadials (e.g. Dokken and Jansen, 1999)."

Referee's specific comment (1) continued To really explain the mechanisms driving the switch between the cold and warm phases of the DO cycles, one probably needs, however, transient simulations. The same is true for addressing the frequency of DO events, so the reference to this issue should be removed from conclusions (bottom of page 1139).

Reply: We have removed the reference to the recurrence of DO events from the conclusions.

Also, lines 14-18 on page 1117 should be modified: "It is presently not clear, however, why DO events were much more frequent during MIS3 than during the following MIS 2 .... Therefore, we analyze in this paper some characteristic features of the MIS3 climate and compare them to the LGM climate ...". Instead of the comparison between MIS3

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and 2, I believe it would be better to refer directly to the difference between LGM and MIS3: DO events have not occurred during full glacial, as well as during full interglacial conditions, but they have occurred during the transition between these extreme climate states. Therefore, the LGM (full glacial, no DO events) will be compared to MIS3, a part of the last glacial-interglacial transition period characterized by frequent DO oscillations.

Reply: The referee rightfully points to the absence of DO events during LGM, while some did occur during MIS 2. Indeed, our work provides a comparison between MIS 3 and LGM, so we changed the sentences on page 1117 quoted by the referee, which now read: "It is 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."

Referee's specific comment (2) The relationship between DO/HE events and climate changes in continental domain should be considered with care. A great similarity exists, indeed, between rapid variations in North-Atlantic and Greenland records on one hand, and in many continental records on the other hand (see for ex. the compilation by Voelker et al., 2002). However, establishing a clear correlation between them is still "work in progress", one of the major difficulties being the accurate dating for the later. Note that the Stage 3 studies by Barron and Pollard, focusing on Europe, talk about "warm and cold MIS3 episodes", not about stadials and interstadials. Section 4.4 "Freshwater forcing required to mimic stadials": How was the 0.3Sv value chosen for the freshwater flux? Is it the minimum value for which the THC is shut down in the model?

Reply: Indeed, the mechanistic connection between DO events on the one hand and HE events on the other is work in progress, though it is accepted that both are transient

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changes. Therefore, we do not claim that MIS3-HE represents an actual HE event. Rather, our MIS3-HE represents an MIS 3 state with shut down AMOC and note that the simulated climate in this experiment is more consistent with stadials during which HE events took place. Our freshwater forcing was chosen to be 0.3Sv to ensure AMOC shutdown, as it was found from a hysteresis experiment that shutdown is obtained at around 0.22Sv and that 0.3Sv could be a realistic number for freshwater release associated with HE events according to Roche, Paillard and Cortijo, 2004, Nature. We have modified the first paragraph of section 4.4 p. 1134 for clarity, which now reads: "To investigate the sensitivity of our MIS3-sta climate to freshwater forcing, we perform a third sensitivity experiment in which we perturb the MIS3-sta climate with a strong, additional freshwater flux in the mid-latitudes of the North Atlantic Ocean to ensure a shut down of the Atlantic THC. From a hysteresis experiment (not presented in this study), we found that in LOVECLIM, the LGM and MIS 3 sensitivity of the meridional overturning strength to freshwater perturbation did not differ, with a shutdown occurring at around 0.22Sv. Resumption of the AMOC took place at around 0Sv freshwater forcing. Our MIS 3 experiment with collapsed AMOC (MIS3-HE), forced with a constant 0.3Sv freshwater flux is setup as an idealized analogue for a Heinrich event. To not indefinitely decrease the global ocean's salinity in this equilibrium run, we allow for a global freshwater correction. As a result, no global sea level rise due to freshwater input is simulated and the salinity of the North Pacific increases. Here we only briefly compare climate conditions in the Atlantic sector between MIS3-HE and MIS3-sta, to ensure that the limitation of freshwater correction does not strongly affect our results."

Referee's specific comment (3) At least in NW Europe, the (vegetation) model seems to seriously overestimate the LGM tree cover. A tree fraction of about 0.8 at LGM (if I get it right from Fig.5a) shows up there where a steppe-tundra environment would be expected, which doesn't leave much room for MIS-sta - LGM differences (Fig.5b). This lets to suppose that the simulated MIS-sta - MIS-int anomaly is even smaller - nevertheless, I think it would be interesting to show it, as it is done in figs. 3 and 4 for temperature, precipitation and geopotential heights.

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Reply: The fact that LGM vegetation in NW Europe is overestimated has been discussed by Roche et al. (2007). Essentially, there are no changes between MIS3-sta and MIS3-int vegetation. The tree cover changed at most 5% in a few locations, which was mostly the case for desert cover, except for one location in the Sahel region and one in Northern Australia where desert cover changes are up to 15%. As mentioned in the text, we do not explicitly discuss the differences between MIS3-sta and MIS3-int in the text where the parameters do not show significant change.

Referee's specific comment (3) continued I agree that climate simulations for MIS3, as for any other period of time, should be performed with boundary conditions as realistic as possible. This is difficult to do in AGCM experiments, mainly because no global reconstruction of the sea-surface conditions exists for another glacial interval than the LGM, but transient EMIC (CLIMBER-2) experiments dedicated to MIS3, with evolving insolation, ice sheets and freshwater flux into the North Atlantic, have been published recently (e.g., Claussen et al., 2005, Jin et al., 2007) and deserve to be mentioned. In the last phrase in the conclusions (page 1140, lines 2-4: "With the results presented in this study, we know that insolation cannot be neglected..."), replacing "we know" by "we confirm once more" would be more appropriate - or the entire phrase could be removed, as the direct results of this study with respect to insolation are detailed in the last paragraph on page 1138.

Reply: Claussen et al., 2003 and Jin et al., 2007 analyse the climates simulated with CLIMBER-2 by Ganopolski, 2003. All three studies are now cited in the text. We refer to our response to Referee Andrey Ganopolski's comments and to modified paragraphs in section 4.4 and an additional paragraph in section 4.5 for further discussion on these experiments. With respect to the paragraph containing the last phrase in the conclusions (p. 1140, lines 2-4), we have changed the text, 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

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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."

Technical comments:

P.1123, line 6: an additional 50-year interval P.1123, line 13: anomalies (not: anomaly) compared to LGM P.1129, line 3: remove "on our model" P.1132, line 20: MIS simulation of (not: on) Barron and Pollard P.1133, line 17: variations in orbital forcing or in the Scandinavian Ice Sheet size P.1133, line 20: temperature differences between the two (not: both) states P.1134, line 4: i.e. (not: i.c.) GHG P.1136, line 29: "As More alike": remove "As" P.1138, line 20: does (not: did) change substantially P.1139, line 4: between the two (not: both) states

Reply: we have made all suggested corrections in the text.

Figure captions: Fig.1: ... insolation anomalies (not: anomaly) ... Fig.2: ... compared to present-day ones ... Fig.4: ... color (not: colour) scale .... Grey areas (not: area's) Fig.5: color (not: colour) scale Figs 8 and 10: between the two (not: both) states.

Reply: The English standard used by EGU publications is British English, therefore, colour should be written as such. All other corrections have been made.

Figures 3, 4 and 5 should be enlarged.

Reply: Full page figures will be requested, thereby more than doubling the size of each of these figures.

Also, please check consistency between the text and the reference list. At least one reference, Pollard and Barron (2003), does not appear in the list.

Reply: We have thoroughly checked (and completed) our reference list with the references mentioned in the text and find no more inconsistencies.

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Interactive comment on Clim. Past Discuss., 4, 1115, 2008.