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Climate of the Past Discussions



# *Interactive comment on* "Sensitivity of the North Atlantic climate to Greenland Ice Sheet melting during the Last Interglacial" *by* P. Bakker et al.

**P. Bakker et al.**

p.bakker@vu.nl

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Reply to Anonymous Referee 1
The comments of the referee are gratefully acknowledged.
Please find a detailed reply to all comments below. Text within quotes represents lines from the revised manuscript.

*Comment 1): Rates of SLR. A large part of the motivation for this study appears to be the potential for high rates of sea level rise during MIS5e. The authors quote the upper-bound estimate of 2.5m/century globally from Rohling et al. (2008) as possible for at least a short period of time. But then rates of FWF this high are applied in the simulations continuously, which implies that the global 5-9m of Eemian sea level rise* *would have impossibly occurred within 2-4 centuries. Kopp et al. (2009) show that an average rate of 0.6-1.0m/century globally is more plausible. Thus, if there were in fact episodes of rates as high as 2.5m/century sea level rise, they must have been very short (probably less than a century time scale) and necessarily transient in nature. Therefore, I think in the introduction, these rates should be discussed more realistically – now the data appears somewhat misrepresented. Moreover, if 0.29 Sv translates to 2.5m/century, then it would seem that even a level as high as 0.1 Sv would be very high and that a portion of the runs considered could be directly discarded.*

Reply to comment 1): We agree that this part needs further clarification which we have now implemented in the revised manuscript. Even though a rate of sea level change of 2.5m/century has been reconstructed for a period for approximately 400 years (Rohling et al., 2008), the maximum applied GIS melt rate of 0.29Sv is very large indeed. Even though such large GIS melt fluxes are most likely features of centennial scale variability at most, they do represent the upper boundary of reconstructed GIS melt rates which we thus included it in the analyses for completeness. We included a reference to Kopp et al. (2009) as well as the following lines: "GIS melt rates in the upper part of this range are unlikely to have occurred for over hundreds of years during the LIG. Nevertheless, to fully describe the possible impact of GIS melt on the LIG climate we investigated the entire range of fluxes."

*Comment 2). The model is run for 500 years with a constant FWF level and the average of the last 100 years is used for evaluation – meaning the model is in quasi-equilibrium. How can results from such an experiment be used to gain insight into transient changes in circulation and climate during the Eemian? This may be a valid approach for the lower values of FWF (that could possibly be considered as the average output from the GIS over the time period), but it's not clear to me how this is appropriate for most of the FWF values applied in these experiments. I would suggest that the authors clarify the justification for the experimental setup used here. I think the manuscript would benefit from a more clear explanation about the time scales involved*



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*(eg, time scale for melting Greenland versus that for collapse and restart of the MOC), as well as justification of how the experiments handle these time scales appropriately.*
Reply to comment 2): We agree it should be stressed in the manuscript that the presented experiments are very much idealized, designed to test the sensitivity of the overturning circulation to melting of the GIS in a climate state bound by boundary conditions and forcings containing different levels of uncertainties, i.e. different chronologies and different confidence in the early LIG. To stress the limitations of this idealized approach we included the following lines in the revised manuscript: "Running experiments with constant forcings and of limited, 500 year, duration implies a number of limitations. Most importantly, it cannot be inferred if the simulated climatic conditions are stable for a prolonged period of time or stable under transient forcings." With regard to a discussion of the time scales involved, we do not agree with referee 1. In the introduction of the manuscript (lines 11-27 on page 2767) a range of reconstructed GIS melt rates is given together with the uncertainties involved in the age control. From this, it follows that the time scale involved in GIS melting is highly uncertain. Because of this, we do not see how the manuscript would benefit from a discussion of the different time scales. If the editor however disagrees with our reasoning, we will include it nonetheless. For clarity, in our simulations, MOC collapse and restart occur on a centennial time scale. Therefore even in the simulations involving very high GIS melt rates, with time scales in the order of a couple centuries, there is no discrepancy between the different time scales involved.

*Comment 3). Constraining the FWF. It is concluded that the FWF associated with Regime 2 is most likely, since the overturning in the Labrador Sea for this regime is comparable to reconstructions. However, the forcing needed to reach Regime 2 will crucially depend on the initial state of the circulation. This is especially true because deep-water formation and the AMOC can respond non-linearly to the applied forcing. Furthermore, the authors are attempting to constrain FWF rates based on quantities (Labrador Sea deep water formation, AMOC strength) which are themselves very* *poorly constrained. Thus it seems unlikely that constraining the FWF is possible. A more plausible approach could be to constrain the overturning to a realistic regime, and only consider the FWF in the context of a sensitivity analysis.*

Reply to comment 3): We acknowledge that many uncertainties are involved in both the model-dependency, the reconstructions of the AMOC strength and the formation of deep waters in the Labrador Sea. In accordance with the advice of both referees we have now modified our approach by focusing on the simulated surface climate regime instead of constraining the magnitude of the melt rate. In section 3.5 of the results we change the title into: "Constraints on LIG deep ocean circulation". Accordingly, in this section we solely conclude that, by comparing the reconstructed and simulated deep ocean circulation of the early LIG, regime 2 is the most likely climate state in our simulations. As the according melt rate of the GIS is highly dependent on both the model and on the setup of the scenario, we now only consider the FWF in the context of a sensitivity analysis and move the possible implications to the discussion section.

In the first part of the discussion section we discuss how the, rather large rates of GIS melt accompanying regime 2, compare with reconstructed rates of past and future GIS melt. We included the following lines to make clear that the melt rates are model-dependent: "It is however important to note that, the resulting range of GIS melt rates is strongly model-dependent and possibly reliant on the setup of the scenario and initial conditions of the simulations. It is therefore crucial to compare these finding with similar experiments performed with other climate models and with different model setups." Nevertheless, it is important to discuss the range of GIS melt rates simulated in regime 2, as climate models used to investigate the impact of future GIS melt often have a very similar sensitivity of the AMOC to changes in the freshwater budget of the North Atlantic Region.

*Comment 4). There are many sentences that could be formulated more succinctly and the grammar improved. There are a few typos and the word order needs rearranging in several places. Also some sections seem overly long. I would suggest careful revision*

*of the manuscript with this in mind, so that the messages of the paper come across more clearly. Particularly, from Section 3.4 and onwards, the discussion becomes more difficult to follow. For example, Page 2778, line 5-9: this sentence is extremely confusing.*
Reply to comment 4): We apologize for the grammatical errors and have improved this. Furthermore, section 3.4 has been deleted while part of its contents have become part of the sections 3.2 and 3.3 in order to increase the clarity and decrease the length of this part of the manuscript.

*Specific Comment 1). Section 3: The discussion begins with the relationship between the AMOC and the FWF and the corresponding "regimes" of circulation. But it is very difficult to see such regimes in Fig. 2 and the definition of the regimes seems to be based on Fig. 3 and the sea ice extent. Consider first discussing sea ice extent in order to introduce the regimes, rather than the circulation.*
Reply to specific comment 1): We agree with the referee that it is better to discuss the sea ice extent first and afterwards the changes in the oceanic circulation and have thus implemented the suggested changes in the text and in the figures.

*Specific Comment 2): Section 3.1: Please change the wording in this section from "sudden" to "abrupt". The word "sudden" seems to imply a short time scale, when I believe what is meant is "abrupt" (ie, a small change in forcing causing a large response).*
Reply to specific comment 2): This has been corrected.

*Specific Comment 3): Conclusions: Suggest putting the conclusions in paragraph form. Also, one point appears twice.*
Reply to specific comment 3): The point which appears twice (For additional GIS melt fluxes of between 0.052 Sv and 0.13 Sv, deep convection 5 is inhibited in the Labrador

Sea while in the Nordic Seas deep convection remains unaffected. July surface air temperatures decrease by as much as 6°C over the Labrador Sea and by around 2°C over the southern part of Baffin Island and part the North Atlantic Ocean, roughly between 40°N and 60°N.) has been removed.

We think that by putting the conclusions in this form, more attention is drawn to the main conclusions. We have, however, decreased the number of points by not summarizing all results and only retain the main conclusions. This has resulted in the following text in the conclusion section of the revised manuscript:

"We simulate three different climate regimes, characterized by typical geographical patterns of changes in surface temperature and sea surface salinity, as a function of the magnitude of early LIG GIS melt rate. The main difference between the three regimes is the rate of deep water formation in the Nordic and Labrador Sea. In respectively regime 1 to 3, deep convection takes place in both the Labrador and the Nordic Seas, solely in the Nordic Seas or strong deep convection takes place in neither of them.

The simulated deep ocean circulation in regime 2 is most consistent with proxy-based reconstructions for the LIG. The rate of GIS melt needed in our simulations to obtain this climatic setting, a flux between 0.052 Sv and 0.13 Sv, is at the upper bound of literature estimates.

By comparing the simulated July surface temperatures for regime 2 with reconstructed maximum LIG summer temperatures, we show that melting of the GIS possibly delayed the LIG thermal maximum in the western part of the North Atlantic region, relative to the insolation maximum, and inhibited the formation of Labrador Sea Water during the early LIG.

Our findings stress that, in order to properly estimate of the future impact of GIS melting, more research is needed to unravel the climate evolution of this potentially crucial part of climate history."

*Specific Comment 4): Figure 1: It appears that the river outflow in the Northeast is not*

*adjacent to the land mask (so that river outflow appears in the middle of the ocean). Is there a reason for this?*
Reply to specific comment 4): The reason that one river outflow point is not adjacent to the land mask is that the adjacent cell is partly ocean and partly land. As the largest part of the cell is however covered by ocean it appears in this figure as ocean. This explanation has been added to the caption of figure 1.

*Specific Comment 5): Figures 2 and 3: Consider changing the order of these figures (as mentioned above). Currently, Figure 3 clearly shows why the freshwater forcing levels are separated into the 3 regimes that are discussed throughout the text.*
Reply to specific comment 5): We agree and have made the change appropriately.

*Specific Comment 6): Figure 7: What is represented by the two colors in the circles? It is difficult to understand whether this is explained in the caption or not.*
Reply to specific comment 6): We apologize for the error in the caption of figure 7. We have now included a line in the caption of figure 7 explaining that the colours depict the uncertainty range of the reconstructed temperature anomalies. Furthermore a line has been added to the main text of section 3.5: "The uncertainty in reconstructed temperatures is taken into account by plotting the maximum and minimum in respectively the right and left-hand-side of the symbols in figure 7."

Interactive comment on Clim. Past Discuss., 7, 2763, 2011.