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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.

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Reply to referee 2
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): The major selling point of the manuscript is the attempt to constrain the Greenland melting rate for the early LIG. In my view, the authors are much too overambitious, if not to say somewhat naive. Obviously, the problem is strongly underdetermined, i.e. we have too many unknowns, for instance the sensitivity of the model to freshwater forcing, height-size of the ice-sheet, exact locations of meltwater *discharge, and so on. Moreover, the potential existence of multiple-equilibria of the ocean circulation (as mentioned in the Introduction) would strongly hamper the approach. The conclusion that the authors were able "to constrain the possible melt rate of the GIS to a flux between 0.052 Sv and 0.13 Sv" (page 2764, line 16) is too bold and needs to be towned down. In my opinion, this is the weakest aspect of the manuscript and all related paragraphs should be removed or substantially revised.*

Reply to comment 1): We agree with the referee that 'constraining the melt rate of the GIS' should not be the main focus of this manuscript. We acknowledge that the problem is strongly undetermined and will make this clear to the reader. However, performing sensitivity experiments such as presented in this manuscript is one of the ways to come closer to detangling this difficult problem in palaeoclimatology. Following the advice of the 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 both GIS melt rates reconstructed for the LIG and melt rates predicted for the future. 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 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 2): The model-data comparison in Section 3.6 is not very convincing. It is quite difficult to see a "good correspondence over the central and eastern part of the North Atlantic". The corresponding figure 7 should be improved. It shows a huge area of green color where it is not clear whether the anomaly is positive or negative. The color coding should therefore be changed (the same holds for figures 4 and 5). Moreover, filling the "proxy circles" in figure 7 with two colors is quite confusing.
Reply to comment 2): We apologize for the missing explanation of the 'filling of the proxy circles'. In the revised manuscript we have made clear in the caption of figure 7 as well as in the main text, that the colours depict the uncertainty in the reconstructed temperature anomalies. "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." (Text added to section 3.5).
Following the advice of the referee, we changed the colour coding of the figures 4, 5 and 7. In the new colour coding it is clear whether the anomaly is positive or negative. With these improvements, the combination of figure 7 and the accompanying text in section 3.6, provides a good explanation of the resemblance between the model-data mismatch and the temperature anomaly fingerprint of regime 2.

Comment 3): To my knowledge, there is no proxy evidence for a substantial cooling in the Labrador Sea for the LIG (e.g. Hillaire-Marcel et al., 2001, Nature) and this poses a real problem that has to be discussed in much more detail. Probably it reflects a too high sensitivity of Labrador Sea convection in this particular model.
Reply to comment 3): There is indeed no proxy evidence from the LIG for the simulated substantial cooling in the Labrador Sea. A more detailed discussion is indeed necessary and has been added to the manuscript. We agree with the referee that one

of the possible explanations is a too high sensitivity of Labrador Sea convection in this model. LOVECLIM1.2 does however perform well (see also lines 11 and 12 on page 2769 of the manuscript) in terms of the sensitivity of Labrador Sea convection in warm climates (cf. Present-day and early Holocene simulation). Another explanation of the mismatch between model and data could relate to the uncertainties in the chronology of the proxy record. In order to make this issue clear to the reader we added a couple of lines at the end of section 3.6: "This interpretation depends heavily on the surface temperature mismatch over the Labrador Sea. However, temperature reconstructions have not revealed cooler conditions in this region for the early LIG. According to our simulations, the shutdown of deep convection in the Labrador Sea leads to lower sea surface temperatures in that region. The discrepancy between the simulated and reconstructed climatic setting in the Labrador Sea can have different causes. Possibly the sensitivity of deep convection in the Labrador Sea to GIS melt is too high in this model. Alternatively, cooler early LIG sea surface conditions have been misinterpreted as being part of the preluding deglaciation. An intercomparison of different climate models or a better age control on the proxy-records can potentially resolve the issue."

Comment 4): I didn't find any conclusions in the Conclusions section. Please rewrite this part. Instead of repeating all the numbers of possible meltwater fluxes and temperature anomalies (which we shouldn't take at face value, see point 1), the authors should make clear what we really learned from this study.

Reply to comment 4): Following the suggestions of Referees 1 and 2, we have focused the conclusion section on take home messages rather than summarizing the main findings.

Comment 5): The statistics presented in figures 6 and 7 seems dubious. The correct way to depict statistically significant changes would be by performing a t-test. However, the 10-year running mean applied to the timeseries would affect the degrees

CPD
7, C2270–C2276, 2012

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Discussion Paper

of freedom and hence statistical significance. I actually don't see a reason for the filtering of the timeseries. Please revise your statistical approach. Note also that there a some confusing typos in the caption of figure 6 ("filter out al sub-decaled variability"; change "96% confidence" to "95% confidence"). In the captions of figures 2 and 3, it is written that values are calculated over the last 150 yr of the simulations, whereas in the text it is written that the analysis is based on 100-yr means (page 2770, line 14). Please clarify.

Reply to comment 5): Following the advice of the referee we have performed a t-test on the unfiltered timeseries. The results have been incorporated in figure 4, 5 and 7 as well as in the text.

The typos in the caption of figure 6 have been corrected.

Minor point 1): Page 2765, line 16: The statement "NADW is formed in two regions" is somewhat simplistic. In reality, the formation of NADW involves not only convective processes but also mixing and entrainmen t (e.g. Mauritzen, 1996, Deep-Sea Res. I).

Reply to minor point 1): We agree that this formulation is incorrect. We have changed line 16 to: "Nowadays, deep convection in the North Atlantic mainly occurs in two regions, the Labrador Sea and the Nordic Seas, where respectively Labrador Sea Water (LSW) and Nordic Seas Deep Water (NSDW) are formed."

Minor point 2): Page 2765, line 29: I am not aware of any "geological data" that indicate a non-linear relationship between freshwater forcing and the overturning. Be more specific.

Reply to minor point 2): We deleted the word 'non-linear' and focus solely on the strong relation between a freshwater forcing and the overturning circulation as indicated for example, by the 8,2ka event.

Minor point 3): The existence of "regime 2" suggests a high sensitivity of Labrador Sea

convection that has already been found in earlier studies. In this context the papers by Wood et al. (1999, Nature) and Schulz et al. (2007, Clim. Past) should be mentioned.
Reply to minor point 3): The findings of Wood et al. and Schulz et al. are incorporated in the manuscript: "These 3 regimes or states of the AMOC have also been found in earlier model studies (e.g. Wood et al., 1999; Schulz et al., 2007)."

Minor point 4): For the discussion of the model results, a figure showing the insolation anomaly would be helpful (e.g. month vs latitude).
Reply to minor point 4): A figure (figure 4)has been added showing the insolation anomaly for the months January and July and this figure is used to clarify the results described in section 3.2 (lines 8-13 on page 2774).

Minor point 5): Assumptions are made with respect to reduced ice-sheet height during the LIG. Is isostatic rebound taken into account when formulating the topographic boundary conditions for the model? Please clarify.
Reply to minor point 1): Isostatic rebound is not taken into account in the construction of the ice-sheet height. Although important, the resolution of the model and the uncertainty involved in the reconstructions of the ice sheet volume and height do not permit it to be included. Our approach is highly idealized and solely meant as a first approach to illustrate the possible impact of the reduced size of the ice-sheet on the climate of the North Atlantic region. Because of this, we do not intent to include a line in the manuscript detailing on this issue. In line 18-19 on page 2771 we do however make clear what the implications of this simplistic approach are: "Nonetheless, the simplicity of this approach limits the evaluation of the atmospheric response to changes of the elevation of the GIS."
Minor point 6): Last but not least, check the language carefully. There are numerous grammar mistakes.
Reply to minor point 6): We apologize for the grammar mistakes and have checked

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ones more the language in the manuscript.

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CPD

7, C2270–C2276, 2012

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