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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.

Anonymous Referee #2

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Bakker et al. present a series of sensitivity experiments using the intermediate-complexity model LOVECLIM in order to analyze the effects of meltwater runoff from Greenland and ice-sheet height/size on North Atlantic climate under early LIG (Last Interglacial) boundary conditions. In general, the results of the numerical experiments provide some interesting results that may shed some light on the early LIG regional climate evolution. However, in its current form, I have some major problems with the manuscript that need to be addressed before I recommend publication.

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.

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.

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.

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.

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 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.

Minor points:

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).

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.

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 S chulz et al. (2007, Clim. Past) should be mentioned.

4) For the discussion of the model results, a figure showing the insolation anomaly would be helpful (e.g. month vs latitude).

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.

Last but not least, check the language carefully. There are numerous grammar mistakes.

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