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Earth System Dynamics Discussions

Interactive comment on "Minor effect of meltwater on the ocean circulation during deglaciation" *by* G. Lohmann et al.

Anonymous Referee #1

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The manuscript addresses an interesting question about the effect of ice sheet melt on the Atlantic Meridional Overturning Circulation (AMOC) and climate during deglaciation and the role of the pathway in which it enters the ocean. It concludes that the glacial meltwater from the Laurentide icesheet does not affect the AMOC if introduced to the ocean bottom but the AMOC is much reduced when it enters at the ocean surface. The process by which it is assumed to reach the ocean bottom is through a hyperpycnal flow. The study is potentially interesting but the representation is not clear. The two biggest problems is that the experiments are not well described and it is not obvious what the authors' own contributions are and what is input from previous studies.

The paper would cause less confusion if the authors:

- Say what is new in this study and what has been done in previous work. - Describe

in detail how new results are derived and give a summary in the conclusions if not the abstract of all new work. - Make clear what is not new and keep those parts separate from the results section.

Overall suggestions for about the methodology are divided into the various models bits and followed by some line by line comments.

Atmospheric boundary conditions: The authors say the ocean model is coupled to an energy balance model of the atmosphere. In its most basic form, an energy balance model balances energy coming in and out of the atmosphere to calculate the temperature. The reader is thus to believe that there is a free atmospheric boundary condition. However, in the methods sections it appears that the ocean surface boundary conditions (confusingly referred to simply as the climate background conditions) are set by a previously run atmospheric general circulation modeling study and that the oceanic boundary conditions are switched at fixed time intervals corresponding with stadials and interstadials versions of the atmospheric study. When the SST boundary conditions for the atmospheric model is described, phrases like "we use the CLIMAP(1981)" on line 17 is used. This makes it sound like the authors are performing the atmospheric simulation as part of this publication. Are these SST data sets used to restore SST in the ocean model or only used in the atmospheric study? If not, is the ocean forced purely but heat and freshwater fluxes and if so, how are they calculated? Mention of an energy balance model should be omitted or explained.

Ice sheet model: The main question here is which part of this model and results are from the current study, and which part is from previously published work (Zweck and Huybrechts, 2005)? The time series of freshwater fluxes is described in detail in the results section along with the description of the three figures relating to it. How it compares with the Fairbanks reconstruction is also describe in some detail. It is not obvious what about it is new and what is from previous work without reading all the referenced papers (which should not be necessary). If these are indeed from a previous study then it should be described in the methods sections or under model description and not in

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the results section as it is peripheral to the main study. If however the authors have produced these time series through their own analysis and it has not been published before in a similar form, the model should be described in more detail. For instance, what process in the model produces such an abrupt meltwater pulse? There were no breaking ice damns in the model I presume, so why did the runoff increase like that? In the result section it is said that the temperature increased at peak runoff. But I thought the model was forced by gradual transition between a glacial and interglacial time slice. So why the sudden warming? In summary, if this work is new, it should be elaborated upon and if it isn't, it should be described as model input and not results and the details or the model data comparison should not be discussed unless it is made clear why they matter for this study.

Ocean Model: What is length of the run? It would be good to know more about the details of the introduction of the freshwater. At a minimum the reader should know the depth and vertical extent of the bottom layers in which the freshwater is introduced as that would give an idea of "instantaneous entrainment". Are there partial steps? Also, is all runoff in the model mixed into the bottom according the hyperpycnal flow mechanism? Is this done by a negative salt flux? How deep could one normally expect a hyperpycnal flow to reach? I mean, is it realistic that it travels hundreds of kilometers under the water without losing its momentum and without turning right to run along the slope in balance by Coriolis. Has any attempt been made to estimate (back of envelope perhaps?) the stabilization depth of such a density current? Has different equilibrium depths been tried?

Line by line comments of various degrees of importance.

P808,l9: Sometimes the background climate state seems to refer to the atmospheric state and fluxes and sometimes to the ocean state. Which is it? There is not really a switch between the oceanic conditions right? Just a switch between the atmospheric forcing to ST conditions during the YD? But it is said the two ocean simulations are taken as background climate conditions.

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P808,l14. What is meant with 'calculated' here? How is it derived? Is this novel stuff or just plotting model output from previous study?

P809: Here is a 20 line discussion of the comparison of the freshwater discharge with the Fairbanks reconstruction without referring to a figure showing the Fairbanks reconstruction. Is the reader suppose to look at Fig 4 here to follow the discussion? If so, please refer to that.

P809, l6. The timing of the meltwater pulse is here compared to that of Fairbanks. Is the timing of it something to be reviewed here as a result of this study or has this been previously described in another paper. It's not obvious whether the reviewer should concern themselves with the timing of the pulse. Is the timing likely to affect the conclusions?

P 809, l18: Is the discussion here concerning Fig 4?

P809, l23: How does the discharge of freshwater into the ocean lead to the creation of Lake Agassiz?

P809, l20-26: These lines would be easier to follow it there was a figure that showed what is being described. In what is the Antarctic ice sheet suppose to play a role and how? It is not included in the ice sheet model from what I understand.

P811. L2: The boundary condition switch is instantaneous which is unlikely to be a realistic change in forcing. That is not a problem for an idealized process study trying to investigate the affect of the entry level of freshwater on the ocean circulation. But, given the idealistic nature of the atmospheric forcing, why is the precise timing of the freshwater fluxes given such prominence in the discussion?

P 811, l22: Any explanation for why the circulation recovers quicker under freshwater forcing?

P811: l17-23: These lines describe the basic result of the study right? Could the authors give a little more detail about why they think the model acts in this way? Why

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does the hyperpycnal simulation recover quicker? And why does it reduce more than 3Sv more than the other simulations after the 12.7 ka switch? This is not mentioned as far as I can see. If the answer is that the first 600-800 years after a climate switch is to be ignored because the model is adjusting to an unnaturally quick climate switch, then I guess much of the comparison becomes suspicious? Can the authors comment on that (i.e., what is to be considered a realistic ocean response and what is to be considered an unrealistic period due to model response to an unrealistically quick change in boundary conditions)?

P811, l24-26; Since it is a new paragraph, mention that the discussion still concerns the hyperpycnal simulation (it does refer to that in this line I presume?). I believe the near surface salinity is only weakly affected but this is not really shown here, right? The 1100m depth salinity is not really near surface. If this is a results of the model stated but not shown it should say (not shown) in brackets and a bit more detail should be given (salinity where?, what does weakly mean?)

P811:l27–30: Is this really an increase in salinity of NADW? It seems to me from Fig. 1 that there no NADW at 1100m in the stadial run (as much as one can deduce form a 2D representation). My impression is that the increased salinity is related to the strengthening and deepening of NADW in the interstadials, i.e., the NADW resumes and transports salinity from the south. I could be wrong but it would be interesting to get a better understanding of how the salinity chances due to changes in the circulation and how much it changes due to changes in the source water or water mass properties. Perhaps some timing of the response of the deep and surface salinity in the model would be informative or similar figures to Fig 5 but at other depths? The current analysis leaves many questions and gives few answers. Of course every study cannot answer all questions but this study is suppose to be about the difference in response of the ocean to surface and hyperpycnal freshwater input so a bit more explanation is warranted.

P812, l18: Which .25Sv is referred to here? Is this discussed in the text or a new number and if new, what does it refer to?

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P812, l22-24: How much does the mixing reduce the salinity of the bottom water in the hyperpycnal simulation? Shouldn't the ocean stratification be less stable with the fresher bottom water and therefore enhance the AMOC? Or is the signal too weak in the deep ocean?

P813, l1: The conclusion are consistent with Roche et al. It would make the paper stronger if it was pointed out clearly what about the conclusion in this study is new compared to Roche et al. ?

P813, l28: Mention in what kind of study (modelling, observations or something?) the Quadfasel conclusions were drawn.

Fig 5: New IS line needed at 11.4ka Is it correct that the background climate state receives no freshwater from glacial runoff or calving and, the two runoff experiments receive quite a bit over the few hundred years of the interstadial, and yet they produce exactly the same NADW export at the end of the IS at 12.8 ka? Is there a reason for this or is just so by chance or is the freshwater just too small at that point to have any effect?

Looking at the Fig 5 legend and caption I see where the confusion of the "background climate" comes in. I suggest calling the ocean "background state" the "control state" or similar so distinguish it from the wording "background boundary conditions" which are the atmospheric forcing that drives all the simulations. The first is a result of the study and the second is the input for it.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 801, 2012.