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Interactive comment on "Deglacial ice–sheet meltdown: orbital pacemaking and CO_2 effects" *by* M. Heinemann et al.

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1 General comments

The manuscript by Heinemann and co-authors is presenting a mechanistic study with a new coupled climate – ice-sheet model on the impact of the different forcing leading to the last deglaciation. The results presented are useful and interesting and the subject in accordance with the journal scope. I particularly appreciate that the authors are honest about the issues of building up an ice-sheet (e.g. Figure 1.c) and about the model biases. Showing what does not work alongside what does is very useful in my opinion.

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I recommend however some substantial revisions before the manuscript is accepted for publication in Climate of the Past, mainly since there is a lot of missing information to enable reproducing the same experiments. In addition some model assumptions are not adequately discussed.

2 Major comments

2.1 Reproducibility

The coupled ice-sheet climate model used here as never been published before to my knowledge. Though the methods used are common, there is a lot of missing information to understand what is done and thus what are the impacts on the results. The description of the model coupling should be greatly expanded to include:

a) the mehods for downscaling between the two model grids. The ECBilt atmospheric model as a T21 grid, the ice-sheet model is 1x1. How do you transfer fields from one model to the other and back? What are the fields exchanged? It is stated that the two models exchange forest fration. Why is that? At what coupling steps? How is the albedo exchange treated between the two models? Is there an ice-sheet mask for ECBilt?

b) the PDD scheme. Which PDD is used for the coupling? Which values of the different parameters of the PDD are used? While the PDD method is common, I know at least three different PDDs that may have a quite different impact on the fields provided (cf. Charbit et al., 2013, The Cryosphere, doi:10.5194/tc-7-681-2013 for a review of the different PDD methods & impacts).

c) The setup of the climate model. I could not find how the model is setup: are you

CPD
10, C206–C210, 2014

using a glacial land-sea mask? A present-day one? Or is it transient? What is done for the Bering Strait?

d) What is the performance of the model for the obtained pre-industrial at the end of the simulation? It would be very interesting to add the Surface Mass Balance on the ice-sheet grid for the end of the run (and for the LGM for future intercomparison with different groups). How that SMB for the end of the run compare to Present-Day?

e) How are bias correction done? Are you using an anomaly approach? On page 513 (last paragraph) the impression is given that "surface temperature is corrected for its present-day bias before it is passed onto IcIES" which may be an anomaly mode. However, the next sentence states that north American temperature must be specifically corrected. Do you have another specific correction for this?

f) LOVECLIM was coupled to at least one other ice-sheet model before. How is your setup comparable to the one from Huybrechts' group? How are your results also comparable or not to theirs? (cf. Huybrechts, Quaternary Sci. Rev., 21, 203–231, 2002.).

g) on albedo again. Do you account for snow and ice albedo? If yes how different are they and how is the computation made?

h) On the α coefficient. Could you indicate in more details how the value was chosen with respect to the present-day climate? (if done as such ...).

2.2 Additional Discussions

Two aspects due to the setup of the model needs to be discussed in much more details

a) Discussion on the lack of freshwater exchange, The model setup mention that freshwater is not conserved in the given system. Since the authors are using an accelerated technique, that seems logical (you cannot conserve the volume and the flux when using accelerated climate runs). However, the implication for the growth or melting of the ice-sheets needs to be discussed in more details than is done now. It is only in the last two sentences that this aspect is mentioned.

b) Likewise, the use of an acceleration technique do not allow the ocean to be equilibrated. Again, I see no particular issue there, but I think that a parageaph at the beginning discussing what are the implications for the runs would be in order.

c) On the ice mask used. I am a bit puzzled by figure 1.b). As I understand it, it shows in grey the areas where ice growth is possible. Since IcIES do not have ice-shelves, I understand that there is a need for enabling ice-sheet growth over the Barents-Kara area and the Hudson bay, where we know an ice-sheet was present in the last glacial cycle. However, using for the rest a present-day mask seems an ad hoc assumption. A better solution would be to use a actual LGM mask, so that the Siberian shelves are exposed, the Alaskan coast enlarged (no Bering Strait) etc. The reason for using the mask given might be the wish to limit the ice expansion through the Bering Strait onto Siberia (a very common issue with almost all climate models) for example. If that is the case, then I would appreciate to have it stated in the manuscript. If it is for another reason I cannot really figure out, please state it as well.

3 Minor comments

a) In the introduction, the citations of the box / conceptual models of Kallen only is a strange choice. Many other classical box models could be cited: Calder, Imbrie, Paillard etc.

CPD
10, C206–C210, 2014

b) In your conclusions, you state that the main result is that orbital alone or CO2 alone is not enough. This is not a new result and citations to previous works showing it are in order: Gallée, Ganopolski (already cited in the manuscript).

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Interactive comment on Clim. Past Discuss., 10, 509, 2014.

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