

Interactive comment on “Interaction of ice sheets and climate during the past 800 000 years” by L. B. Stap et al.

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Formal review of “Interaction of ice sheets and climate during the past 800 000 years” by Stap et al., ms. cp-2014-71

== General comments ==

The manuscript presented is both important and interesting. It places itself as the natural continuation of previous work by de Boer et al., 2010. The grand advantage of the model presented resides in its simplicity and possibility to test physical hypotheses that are unattainable by more complex models. As such, it is an important study. Apart from a few sections, it is well written and, in my view, should be acceptable for publication after revisions. I have nonetheless substantial comments that are listed below in order

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of reading. I highlight the major ones by a specific mark “(***)”. Given the amount of missing discussion to previous work, I feel obliged to request major revisions, though I have no doubt that they can be achieved by the authors.

== Specific comments ==

1) p. 2549, line 1-10.: the authors are referring to the temperature records from ice cores. This is misleading. What is commonly used as a temperature record in Greenland are the water isotopes records which are not quite temperature, especially in Greenland. A quick look at recent publications discussing the matter show considerable differences: for example compare Kindler et al. (2014) with a glacial / interglacial change of 15°C and Simonsen et al. (2011) with an amplitude of 20-22°C. These are only two examples. Please rephrase that paragraph to take into account this particular aspect.

References cited:

Kindler, P. et al. : Temperature reconstruction from 10 to 120 kyr b2k from the NGRIP ice core, *Clim. Past*, 10, 887-902, doi:10.5194/cp-10-887-2014, 2014.

Simonsen, S. B. et al. : Past surface temperatures at the NorthGRIP drill site from the difference in firn diffusion of water isotopes, *Clim. Past*, 7, 1327-1335, doi:10.5194/cp-7-1327-2011, 2011.

(***) 2) The manuscript in its entirety deals with the foundation of the Milankovitch theory, namely the link between the changes in incoming solar radiation and the volume of ice-sheet (or sea-level). I am rather surprised that classical references of previous work on the Milankovitch theory are simply ignored: Milankovitch himself of course, Calder (1974), Imbrie and Imbrie (1980) or Paillard (1998) to cite a few (Paillard, 2001 is a good starting point for more on this). Using only recent references as done in the manuscript gives the false impression that the field is an emerging one.

References cited: Calder, N, *Arithmetic of ice ages Nature*, 52, 2 16-218, 1974.

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Imbrie, J., and J.Z. Imbrie, Modelling the climatic response to orbital variations, Science, 207, 943-953, 1980

Paillard, D., The timing of Pleistocene glaciations from a simple multiple-state climate model, Nature, 391, 378-381, 1998.

Paillard, D., Reviews of Geophysics 39 , 3, 2001, pp. 325-346

(**) 3) An even more surprising aspect is the complete lack of reference and comparison to earlier very similar work by the group of Louvain-la-Neuve with the LLN-2D model. They also used a simplified ice-sheet climate model and addressed the question of the ice ages. This comment is not limited to the introduction, but should be addressed both in the introduction and in the discussion part. Good starting points are Gallée et al. (1992) and Berger et al. (1999).

References cited:

Gallée et al., SIMULATION OF THE LAST GLACIAL CYCLE BY A COUPLED, SECTORIALLY AVERAGED CLIMATE-ICE SHEET MODEL .2. RESPONSE TO INSOLATION AND CO₂ VARIATIONS, JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES, Volume: 97 Issue: D14 Pages: 15713-15740, 1992

Berger, A., X. S. Li, and M.-F. Loutre, Modelling Northern Hemisphere ice volume over the last 3 Ma, Quat. Sci. Rev., 18, 1-11, 1999.

4) p. 2552, line 19-21: could you specify whether the land cover type is interactive or not?

5) p. 2554, line 7: "To obtain a better transient temperature response" Better than? Relative to?

6) p. 2554, line 26-27: Why do you choose one ice-sheet being parabolic and the other linear? Shouldn't this choice be variable in time?

7) Coupling scheme (p.2556, line 19 and further): why do you choose to communicate

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between the models only every 1,000 years? It cannot be for computation reasons as is usually done, since both models are very fast ...

8) (***) p. 2557-2558: the choice of starting point is rather strange, why are you doing this for the PD and PI climate? We know that the present-day ice-sheet is not at long-term equilibrium with the pre-industrial climate and even less with the present-day ... Running 50,000 years with present-day climate (350 ppm) seems extremely strange. I do not think that the justification of looking good with ERA-40 is enough in that case to justify this approach. Using the PI is a relatively standard procedure for coupled climate model, but PD?

9) (***) p. 2559, line 13-20: you really need to add a zoomed in version of your figure centred on the last 100 kyr. If not it is quite impossible to relate your text to the figure.

10) (***) p. 2559, line 14-16: "The coupled model captures the 40 kyr-fluctuations in sea level at 90 and 60 kyr ago better than the model of De Boer et al. (2010)." I disagree on the basis of the figure presented. If the Red Sea record is your reference on figure 4 (blue dots) then the best approximation of those dots is the green line (DB10), not the red line ...

11) p.2559, line 18-20 "In most glacials, the model seems to underestimate the sea level drop." I am sorry, but I do not see what you are pointing to on the figure. At the LGM, you have the right amplitude, not the right timing ; at the previous glacial maximum, you have too much sea-level drop ... not enough at the one before that. So the sentence seems to be inaccurate. In any case, if I remember correctly, the Antarctic ice-sheet is estimated about 12-15 meters of sea-level drop at the LGM: how can this account for large differences in sea-level?

12) p. 2559, around line 25: how is the scaling chosen for $\delta^{18}\text{O}$ / temperature?

13) p. 2560, line 4-6: "In reality, local temperature differences can be much larger than ..., especially over areas prone to large albedo and height changes such as

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Greenland". At least this is incorrect for the last glacial – to present anomaly, since Greenland did not change much since the LGM. This may be true if you melt partially Greenland like during the Eemian though. A better example is the Laurentide ice-sheet in this respect.

14) (***) p. 2560, line 9-16: your model seems to have an overturning influence on temperature which affects mostly the south, not the north. This is exactly the opposite to what is shown in more complex models (GCMs ...). How do you reconcile this aspect? One could argue that this is an consequence of oversimplification of your modelling tool.

15) (***) p. 2560, line 20-25: This is difficult to understand. Do I get correctly that your model simulate a reduction of WAIS during glacials of 4 meters s.l.e. And that the EAIS is increasing by a comparable, opposite 4 meters? If that is correct, then that means that the complete Antarctic contribution is ... zero? If yes, please state it clearly. State as well that this is likely to be the opposite expected, since colder conditions should dry out the EAIS and therefore yield to a slight reduction in volume there, while the WAIS is expanding to the shelf break ... Also link to my previous remark #11

16) p. 2561, line 10-12. I do not understand why local effects may amplify the deep ocean response. What is the controlling factor of the deep water temperature changes is the surface temperature at their source, not local effects where the deep water is considered?

17) p. 2561, line 18-27. I have to admit I did not follow your scaling argument nor its underlying physical meaning. "... a 0.2 scaling factor relates local deep-ocean temperature anomalies to local atmospheric temperature anomalies." This cannot be a general rule, since there is in general no relationship between the local atmosphere and the isolated deep ocean. This may only work for deep water formation regions, as mentioned in #16. Could you please explain this better?

18) p. 2562, line 9-25: I am a bit puzzled by the physical interpretation of fixing the

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location of the overturning and its strength independently. In reality of course it cannot be the case (not even in more complex models) since the deep water strength is dependent on the surface conditions where the formation is possible. How robust can your result for the differing north / south response be here?

19) p. 2564, line 7: "... only ice volume" I guess you mean "albedo" here?

20) (***) p. 2565, bottom: this is one location where discussion of the comparison to the LLN-2D results is drastically missing.

21) p. 2567-2568: very interesting discussion, well done.

22) p. 2569, line 7: Peltier & Fairbanks is a sea level reconstruction, not ice-sheet if I am not mistaken. Peltier (2004) is an ice-sheet reconstruction.

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Interactive comment on Clim. Past Discuss., 10, 2547, 2014.

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