Review of "Coupled ice sheet–climate modeling under glacial and pre-industrial boundary conditions" by Ziemen et al., manuscript number cp-2014-6

The paper presents steady state simulations under pre-industrial and last glacial maximum climate conditions utilizing an AOGCM including sophisticated treatment of vegetation. The model is coupled bi-directional with a modified version of the ice sheet model PISM. The paper reports on the modifications in PISM and the used coupling scheme. It presents simulated climate characterises like simulated drop in LGM temperature. The biases in the climatic fields are shown and discussed. LGM and pre-industrial climatic fields are presented and put into a general context. Simulated LGM ice cover is compared with different reconstructions. It shows that the simulated LGM ice covers of North America and Fennoskandia are reasonable, although the Laurentide ice sheet does not extend far enough to the south and there is too much ice in Eurasia in their simulations. In addition, the capability of the ice sheet model mPISM to simulate ice steams – in particular the Hudson and Mackenzie ice streams – is demonstrated.

General Remarks

The paper contains sufficient material and deserves publication in Climate of the Past, although the paper needs a revision. Of course, the progress reported in the paper is partly a technical one. It is demonstrated that coupling of several comprehensive models is feasible and computational times of several thousands (in T31 resolution) are possible today. Additionally, the paper contains a number of scientific results. However, sometimes all these details are overwhelming. Indeed, the major findings of the paper should be presented more condensed. In my opinion, the paper should have been more carefully proof-read before its submission. Below you find my concerns, comments and recommendations.

Major Comments

1. Certainly, there is an imbalance/asymmetry in the modelling approach presented in the paper. While the model components for the atmosphere (ECHAM5), ocean (MPIOM), vegetation (LPJ) and dynamics of the ice sheets (mPISM) are state-of-the-art or almost state-of-the-art, the representation of the surface mass balance of the ice sheets is rather simple, because it employs the PDD model for representation of surface melt. This empirical approach was originally developed for the Greenland ice sheet. I hope you are aware that the Northern American continent is a rather different place than Greenland. This is important, because the measurements, which are the basis for the PDD parameters, were done in Greenland. Therefore, these measurements implicitly assume present-day Greenland climate. For example, the diurnal cycle, which affects ice sheet melting, over the North American continent is quite different from that over Greenland. In your simple PDD approach for surface melt, you do not treat these differences. Maybe, this is the reason why you change the values of lapse rate and the PDD factor for ice as compared to Reeh (1991). The melt rate is about a factor of two higher than the standard value for the Greenland ice sheet. It deserves at least a mention why you changed the parameters and what you intended to improve with this change.

2. Of course, the poor representation of surface mass balance (and other processes) in your approach, might have its reason in limited human resources and the many components of the Earth system can only implemented step by step. However, in your model application

you study dynamical ice sheets and it is important to capture adequately their mass balance. Because surface mass balance is one key component of your model application, the more comprehensive representation of the other components of the Earth system loses its relevance in your approach. This is a real drawback of your study. Therefore, you really have no reason to claim that Earth system models of intermediate complexity (EMICs) cannot be used for detailed studies of the climate dynamics (section 3.9). EMICs are often more balanced in the grade of description of their components compared to your approach. I would propose to view this more positive. EMICs and comprehensive models can be both used for a detailed study of the climate system. They can even complementary to each other.

3. A further point worthy to mention is the steady state setting in your simulations. Such a setting appears more or less reasonable for pre-industrial boundary conditions, because the Holocene climate was rather constant, at least compared to glacial-interglacial climate changes. It might be common practice (as mentioned in the introduction of the paper) or not to assume steady state at a culmination point like the LGM, but as a matter of fact there were strong changes in ice volume before and after the last glacial maximum. Therefore, the steady state assumption is not too favourable, when ice dynamics are included in a modelling approach. By the way, possibly EMICs could have helped you to find a transient setup for your model. Most certainly, it is the steady state setting which caused the strong drift in the ice volume of the ice sheets in Eurasia when the setting with constant LGM boundary conditions applies. This point is not sufficiently discussed in the paper.

4. Concerning the strong drift in ice volume under LGM climate conditions and having in mind the biases of the GCM, it would be interesting to know, whether such a drift exists for pre-industrial conditions too. Looking at Fig. 2a, I observe a rather cold bias over Alaska. I wonder how a time series plot of ice volume (as Fig. 12) would look for simulation PI-mPISM?

5. The LGM Greenland ice sheet (page 586, Section 3.7.1): This section together with Fig. 1 is somewhat misleading, because it is suggested that it is possible to simulate small scale features like the outlet glaciers of the Greenland ice sheet with a horizontal resolution of 20 km. Kong Oscar Glacier for example has a width of about 4 km and Kangerdlugssuaq of about 9 km; see e.g. Box and Decker (2011). Because the horizontal resolution of your ice sheets is 20 km, it is impossible to simulate these outlet glaciers. I am not quite sure which features you show in Fig. 1. Most possibly the depicted feature are troughs on the continental shelf over regions where there is ocean at present-day. It should clarified what is depicted in Fig. 1; for example, I it cannot be the *present-day* Kong Oscar Glacier (4 km in width), which is seen in that figure.

6. Ice sheet flow and sliding parameters. Please, list the parameter values of the flow law ice (enhancement factor, parameters describing the temperature dependence of ice, etc.) and give the sliding parameters. I am writing this, because there is a problem in understanding the text on page 587, lines 1-7. I cannot confirm your sentence “This fast flow is caused entirely by internal deformation”. This is very usual. It is generally accepted that sliding velocities are the dominant component of fast flow. Has your model for some reasons very high deformational velocities? What are the values of your deformation velocities? In the next sentence you write “The inclusion of temperate ice in PISM allows for a very low viscosity, so the ice can reach high speeds by pure internal deformation.” It must be a very unusual vertical profile of horizontal velocity if an only about one meter thick temperate

basal layer dominates the velocity. Your statement cannot be right. Please, quantify this. How thick is the temperate layer? What means “very low viscosity” in numbers? What are you parameters in the flow law of ice?

7. There is a serious problem with your statement about 30% “cooling” of the Arctic ocean due the cut-off of the ice shelf ocean heat fluxes: “The heat fluxes from glacier calving and shelf basal melt contribute 30% of the total cooling of the Arctic Ocean in LGM-mPISM (Table 5)”, page 585, lines 11-12 and page 595, lines 10-14. It is unclear to which physical quantity these 30% belong. Please, clarify this. The -12.3 TW in Table 5 are rather small. The head budget in that table is not complete. What is about the 150 TW northward heat transport in the North Atlantic Ocean, which should bring heat toward the Arctic Ocean? All values in Table 5 are rather small. The table is irrelevant. The real effect of latent heat of calved ice can be seen in Fig. 8a: The *total* ice cover of the Artic Ocean changes only slightly between the two experiments (LGM-mPISM and LGM-mPISM-W). However, there is some *local* effect in perennial sea ice cover near ice sheets. This local retreat in sea ice cover deserves a mention in the paper. However, I recommend refraining from the statement about the 30% cooling, because this cannot be substantiated.

8. Section 3.9 (Comparison to other model coupling studies). This section is somewhat too technical. I would expect in this section a comparison, which focusses more on science and the *results* of other modelling studies.

Minor Comments

1. Page 571, line 10 and page 572, line 2: There is a contradiction between these two text parts. The methods by Calov (1994) and Calov and Greve (2005) are different. Most probably, you use the scheme by Calov and Greve (2005) based on the PDD method by Braithwaite (1984). Please, fix that. There is no need to cite Calov (1994) here.

2. Page 571, line 20: What is P_{solid} ?

3. Page 572, line 6: Obviously, you changed the values of the PDD parameters as compared to those given by Reeh (1991). Are the numbers in water equivalent or ice equivalent? In any case, your ice melt rate is a about factor of two (1.7 or 1.8) higher than that by Reeh (1991). Please, explain what the reason for this parameter change.

4. Page 573-575, Section Setups and experiments: the entire section should be revised. For example, it is even hard to understand how many model years you run the respective simulations.

5. Page 574, lines 14-15: What is PISM’s bootstrap method? Please, explain that.

6. Page 574, line 17-20. What means “several” here? Does it mean 38500 years? This would not be sufficient to equilibrate the temperature field inside the ice sheet.

7. Page 578, lines 12-13: I cannot believe that the present-day ablation is practically zero along the Greenland east coast. The marginal surface elevation of the east coast is not too different from that of the west coast. Further, your table 3 indicates that there is ablation, which compares well with the findings from regional climate models of the Greenland ice sheet. Is none of this melt located in the east of the Greenland ice sheet?

8. Page 579, lines 5-6: “This leads to an eastward displacement of the ridge of the ice sheet.” It is very unlikely that changes at the ice margin too drastically affect the interior of the ice sheet. This is rather determined by the precipitation field. Have you explicitly tested your assertion on the shift of the ridge of the ice sheet?

9. Page 580, line 25: But this is not only typical for CO2; this is also typical for insolation. The sentence appears strange to me.

10. Page 587, line 1-2: What do you mean with “interior of Greenland”? Is it the present-day Greenland or a part of the present-day Greenland? Please, specify this. It is important to know, where there is sliding in your model and where there is no sliding.

11. Page 591, lines 6-8: Is Iceland important for your study? In Fig. 12, the ice volume of Iceland is practically zero compared with the other ice volumes. Please, do not overload the reader with useless information and remove the Iceland ice volume curve.

12. Page 593, lines 13-15: “The PDD method is much faster and also is widely used in glaciological and coupled model studies (e.g. Gregory et al., 2012).” This statement sounds strange, because the low computational costs of PDD are completely irrelevant compared to the computational expensive coupled AOGCM. Although PDD is still used in some studies, recent analysis by van de Berg et al (2011) shows that this approach is not applicable for simulation of ice sheet mass balance under variable orbital forcing.

13. Page 594, lines 1-8: The authors apparently mixed climate dynamics with atmospheric dynamics. Indeed, in CLIMBER-2 atmospheric dynamics is highly parameterized. This does not prevent the model from “detailed studies of the climate dynamics”, as demonstrated by several studies, which were done with this model.

14. Page 620, Fig. 8a: On the printout, the light red, yellow/orange and blue lines are hardly visible. There is an error in the figure caption. Most probably, the red and dark blue lines belong to run LGM-mPISM, while the orange and light blue lines are from run run LGM-mPISM-W.

References

Box JE, and Decker DT, Greenland marine-terminating glacier area changes: 2000-2010. Ann Glaciol 52, 91-98, 2011.

Braithwaite RJ, Calculation of degree-days for glacier climate research. Z Gletscherkd Glazialgeol 20, 1–20, 1984.

van de Berg WJ, van den Broeke M, Ettema J, van Meijgaard E, and Kaspar F, Significant contribution of insolation to Eemian melting of the Greenland ice sheet. Nature Geoscience 4, 679-683, doi: 10.1038/NGEO1245, 2011.