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Interactive Comment

Interactive comment on "Coupled ice sheet–climate modeling under glacial and pre-industrial boundary conditions" *by* F. A. Ziemen et al.

F. A. Ziemen et al.

florian.ziemen@mpimet.mpg.de

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We thank the reviewer for the positive evaluation of this manuscript and for the constructive criticism. In the following we provide a point - to point response to the individual comments. Reviewer comments are typeset in indented blocks.

> 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.

We have shortened and focused the paper.

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.

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We are aware of the imbalance between the sophistication of the model components and the surface mass balance scheme. We kept the surface mass balance simple to reduce the complexity of this project.

We attempt to represent some of the differences between the different regions by interactively computing the monthly temperature standard deviations from the six-hourly climate model output. This computation does include the differences in the diurnal cycle between the different regions mentioned by the reviewer. We chose the higher ice melt rate because of successful applications of a melt rate of $12\,\mathrm{mm\ water\ equivalent}$ in earlier simulations with ECHAM3/LSG/SICOPOLIS(Vizcaíno, 2006; Vizcaíno et al., 2008). It is one of the defaults in SICOPOLIS (Greve et al., 1999).

> 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.

We do not intend to disqualify the use of EMICs in the study of the climate system. We do however see a difference in the level of detail that is possible between GCM based

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studies and EMIC based studies. We rephrased this paragraph to clarify.

> 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.

We are aware of the limited applicability of a steady state approach to the LGM. We expanded our discussion of this point in Sect. 3.8. However, we intentionally chose this approach to separate the interactions between ice and climate from the effects of a time-varying external forcing. In this respect we followed the example of Calov et al. (2002).

> 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?

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We listed the drift of the different regions (averaged over the last 1000 ISM years) in table 4 as well as in the text. The requested plot (for the full 10 000 years) is attached for reference. We consider the additional information provided by this plot as rather small and would therefore prefer not including it in the publication.

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.

We rephrased the paragraph in question and the figure legend to make it clear that we use the geographic names purely to identify the locations of the ice streams and we are not claiming to resolve small scale outlet glaciers at a model resolution of 20 km.

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?

We use an enhancement factor of 3. PISM uses the Glen-Paterson-Budd-Lliboutry-Duval flow law with

$$A_t = 3.61 \times 10^{-13} \exp\left(-6.0 \times 10^4 / RT^*\right), \text{ if } T^* \leq 263.15\text{K}$$

and

$$A_t = 1.63 \times 10^3 \exp\left(-13.9 \times 10^4 / RT^*\right) \text{ if } T^* > 263.15\text{K}$$

Where $T^* = T + b(h - z)$ is the pressure adjusted temperature (for temperate ice the melting point temperature of 273.15 K)

$$A_t(\omega) = A_t(T_m)(1 + 184.0\,\omega) \quad ,$$

where T_m is the melting point temperature, and ω is the melt water fraction (in PISM parameterized via the enthalpy of the ice) (Bueler and Brown, 2009; Aschwanden and Blatter, 2009; Aschwanden et al., 2012). The water fraction is limited to 1%, and can thus reduce the viscosity (and thus the ice velocity) by a factor 2.84. We use a vertical layer spacing of 100 m. In some obtain regions, the temperate layer has a thickness of one grid cell. The shallow ice velocities generally stay below 500m/yr. We use a linear sliding law with a friction coefficient of $1\,\text{yr}\,\text{m}^{-1}\,\text{Pa}^{-1}$ that allows sliding if there is basal water and deformable sediment available to lubricate the ice sheet.

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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.

We are thankful to the reviewer for the thorough check of our model results. We are however somewhat surprised about the numbers stated and the conclusion about our results being wrong. As stated in the table caption, we define the Arctic Ocean as the area north of Fram Strait. Maybe different basin definitions are the source of the disagreement? From the comment it seems that the reviewer includes the GIN sea in the Arctic Ocean. We added a statement *north of Fram Strait* in the text at this location to clarify our definition.

With the smaller ocean basin during the LGM, we obtain an area of 5.6863×10^{12}m^2 for the Arctic Ocean. The average heat loss during the full 3000 years of the LGM experiment is 4.2287×10^{13}W, corresponding to 7.436W/m^2. Since measurements only exist for present day, we'll have a look at those: Serreze et al. (2007, (paragraph 65)) lists $6\,$W/m^2 total heat loss for the present day Arctic Ocean (including the Barents

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sea and Arctic Ocean shelves that were covered by ice sheets in the LGM), $3\,\mathrm{W/m^2}$ of this being supplied by ocean heat flux convergence and $3\,\mathrm{W/m^2}$ by ice outflow. Our result of $7.4\,\mathrm{W/m^2}$ seems to be in perfect agreement with these numbers, and definitely not drastically underestimating the Arctic ocean heat losses as claimed by the reviewer. As stated in the text, the Arctic Ocean is constantly covered with thick ice in our experiment. This drastically limits the heat losses to the atmosphere. Adding the GIN sea to the equation, we would obtain an additional heat loss of 7.4403×10^{13}W. The model's total of 120 TW gets into the range of the 150 TW stated by the reviewer.

> 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.

We removed the whole section. We describe the science results of other studies in the introduction.

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.

We removed the citation of Calov1994 as suggested.

> 2. Page 571, line 20: What is Psolid ?

Solid precipitation (snowfall). We added the missing definition.

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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.

We adapted these parameters from Vizcaíno et al. (2008). There they were based on Greve et al. (1999). We added the missing units (ice (equiv.)) in the manuscript and refer to Greve et al. (1999).

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.

We rewrote the section.

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

PISM guesses a thermal distribution in the ice when bootstrapping from climate, ice thickness and surface topography. This is meant to speed up the adjustment to a realistic temperature distribution. We are fully aware that it does not replace properly spinning up the model for studies of the ice dynamics. However, in our pre-industrial experiment we mainly intended to test the surface mass balance scheme and coupling and therefore did not focus on a perfect internal temperature distribution of the Greenland Ice Sheet. We therefore decided to start from this educated guess and then let the model evolve for 10 kyrs.

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Quoting from the documentation of PISM0.3 (`iMbootstrap.cc`, T = temperature, H = ice thickness, g = geothermal heat flux, k_i = thermal diffusivity of ice, k_r = thermal diffusivity of bedrock):

Within the ice, set

$$T(z) = T_s + \alpha(H - z)^2 + \beta(H - z)^4$$

where α, β are chosen so that

$$\frac{\partial T}{\partial z}\Big|_{z=0} = -\frac{g}{k_i}$$

and

$$\frac{\partial T}{\partial z}\Big|_{z=H/4} = -\frac{g}{2k_i}.$$

The point of the second condition is our observation that, in observed ice, the rate of decrease in ice temperature with elevation is significantly decreased at only one quarter of the ice thickness above the base.

The temperature within the ice is not allowed to exceed the pressure-melting temperature.

Note that the above heuristic rule for ice determines $T(0)$. Within the bedrock our rule is that the rate of change with depth is exactly the geothermal flux:

$$T(z) = T(0) - \frac{g}{k_r} z.$$

Note that z here is negative, so the temperature increases as one goes down into the bed.

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.

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Several means more than 100 000.

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?

We corrected the text to state that this refers to the northern half of the east coast (north of 70°N), where there is perennial sea ice and the surface elevation is drastically overestimated. There is substantial melt along the southern east coast. Thank you for asking.

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?

No, we therefore have rephrased to remove the causality and simply state that the ridge is displaced to the east.

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.

We removed the attribution to the GHG change.

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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.

The mask is displayed in Figure 1 of the manuscript. For easier comparison we have also overlain it onto the pre-industrial Greenland velocity map and attached (Fig. 2). In the small print size, this version of the figure becomes a bit hard to read. We would therefore prefer to keep the print version as before.

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.

We included it for completeness and did not regard it as too overloading since it is constantly at about zero. We have removed it in the new version.

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.

We fully agree with the reviewer that the computational costs of a PDD model are negligible compared to those of a AOGCM. Our intention was to state that PDD is

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computationally cheap. We are aware of its limitations. We removed the statement about the computational cost and referenced van de Berg et al. (2011). The effect of insolation changes between the LGM and the present is rather small.

> 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.

As stated above, we rephrased the paragraph to clarify. We intended to refer to resolving / analyzing features that simply are sub-scale for a model like climber.

> 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.

The reviewer is correct in interpreting the colors. We have optimized the figure and rephrased the caption.

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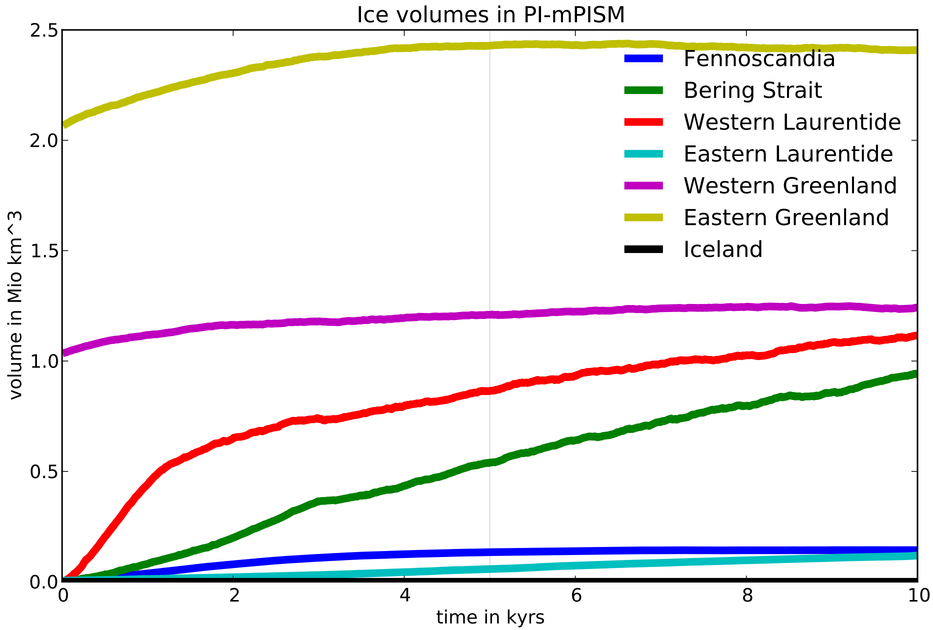

Fig. 1. {Ice volumes in PI-mPISM. The region definitions are the same as in Fig 12 in the manuscript.

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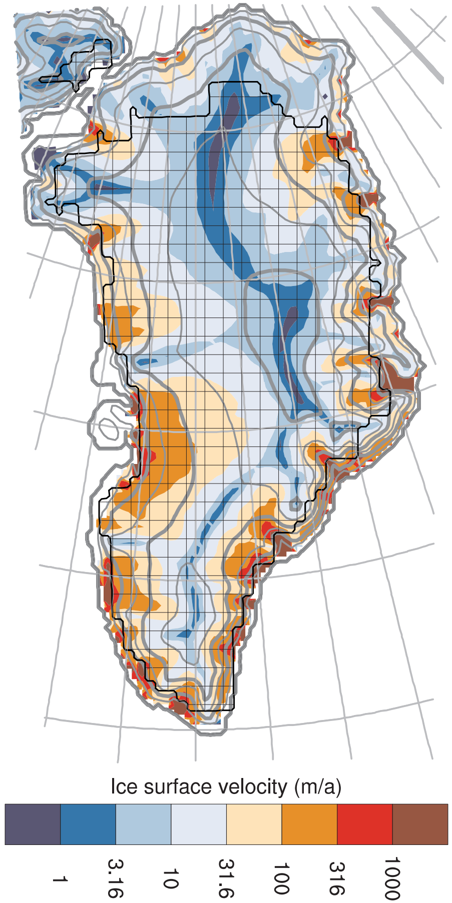

Fig. 2. Greenland flow velocities in PI-mPISM. Areas without sliding are crossed out.

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