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

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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.

> This is a useful and interesting paper in the developing field of two-way coupled climate–ice-sheet models, and is certainly worth publishing CP in my opinion. The analysis of the simulations is generally appropriate and clearly laid out. I would suggest that its two main weaknesses are a lack of focus on the principle message and conclusions that the authors are trying

to convey, and perhaps not enough information on how sensitive the model may be to some critical parameter choices in the setup.

The lack of clear focus can be seen in microcosm in the abstract, which has no actual conclusions, simply a list of climate metrics of their LGM simulation. Throughout, though, the admirable detail in the characterisation of the model state doesn't often enough follow through to tell us what we can learn about the LGM climate, real icesheet-climate interactions or how they are represented in such a model. This is perhaps unfair, as useful links between the different components of the climate system are analysed in places, but my over-riding impression was that the paper could be improved by maintaining focus throughout on some concrete conclusions.

We have rewritten several parts of the paper to provide a stronger focus on our main message, the introduction of the new coupled model system and it's validation against observations and proxy data.

A more scientific issue, rather than perhaps presentational, concerns the basic modeling setup. The challenges in climate-icesheet coupling laid out by the authors in their introduction are indeed steep, and non-ideal empirical parameterisations and fixes, such as the PDD method used here to calculate ablation, the simplistic calving or the way representing icesheet surface albedo effects on the coarse GCM grid are currently unavoidable. It is important to know, however, how the parameters - often unrealistically held constant in space and time - that go into these shortcuts have been chosen, and how sensitive the model's results might be to different choices. As Gregory et al 2012 (reference in the ms) show, the large-scale results from coupled GCM-icesheet studies can be significantly affected by the choice of parameters in the PDD scheme and elsewhere. A full sensitivity study

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to the setup of all the parameterisations chosen is, of course, unfeasible (although studying climate-icesheet feedbacks at more of a process level through such an exercise might be a useful focus for a paper such as this), but some feel for how sensitive the model results are, particularly in the light of unrealistic features such as the East Asian icesheet produced here, would give an idea of how confident one might be in the results of a model such as this.

The East Asian icesheet and the glaciation of the Rocky mountains are very robust features of this coupled model. The simulated glaciation of the Rocky Mountains and parts of Eastern Siberia for present day has also been observed in a model version applying the same climate model coupled with an energy balance model to SICOPOLIS (Vizcaíno et al., 2010). It seems therefore to be more related to the climate model than to the coupling. The Scandinavian part of the Fennoscandian Ice Sheet has proven fairly sensitive to the PDD parameters. More aggressive melting would have led to its destruction. Less melting would have led to more / faster glaciation of eastern Eurasia and the Rocky mountains.

To address some less general issues, in the order in which I encountered them in the paper:

page 564, line 6: The consistency of the ice-sheet and climate state achieved in a model such is this is non-trivial, and opens up a whole area of study of glacial climate feedbacks that hasn't really been possible until now - more might be made of this in the paper

The main intent of this paper is to introduce the coupled model and describe the base-line climates. We will focus on different interactions in follow-up publications. We have studied Heinrich events in this setup (Ziemen, 2013) and are currently studying the deglaciation.

> p564,l15: Is the temperature reported the global annual average?

Yes, we first added the missing information and then removed the whole sentence.

> p565, lines 6,14: "substantially different" and "reasonably steady state" are imprecise, relative terms, dependent on the observer and what aspect of the climate system you're interested in. And, as the authors later note, (p590) treating the LGM icesheet as being at a steady state by spinning them up with the same forcing for 30kyr is not really correct

We rephrased the terms in question. The ice sheet had a much longer spinup (under constant LGM forcing). The experiment is 30 kyr long. We rephrased the description of the experiments to clarify this.

> p566,l3: "recently observed" implies the need for a reference

We added the missing reference.

> p570,l17 (and later): The model setup section refers to changes in sea-level being fed through to the coupled model as a whole, implying changes in the size of the ocean basins and potential wetting/drying of land surfaces, but no further details are given as to how this is done. This sort of on-the-fly domain changing is a substantial challenge for most global atmosphere-ocean models, and I wonder how the authors have dealt with it.

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We added the missing information in section 2.4. *We keep the ocean bathymetry and land sea mask constant. The freshwater fluxes change the surface height of the ocean. In ECHAM5 we reduce the land surface elevation when the sea level rises, and vice versa.*

> p571: The model uses a PDD scheme, producing melt that is non-conservative of energy with respect to the GCM, downscaling both temperature and precipitation to the ice-sheet topography in a way that is unavoidably inconsistent with the GCM gridbox means they are based on. In a fully coupled system, significant non-conservation of water/energy can cause spurious climate artifacts, especially if allowed to build up over runs of many centuries - can these conservation issues be quantified or discussed here?

We added information on the conservation properties. Freshwater fluxes are conserved in our setup, heat fluxes not.

> p572,l19: "We first detail on" is not good English

We rephrased.

> p573: I found the description of the modification of ECHAM's albedo scheme rather unclear.

We tried to clarify.

> p574,l1: Results from LGM-mPISM-W are barely referred to later, and no real conclusions seem to be drawn from them. Could more be made of this?

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The main difference between LGM-mPISM-W and LGM-mPISM is the change in the Arctic Ocean Sea ice. We discuss this in section 3.6. Otherwise we did not observe any significant changes.

> p574, l9: The asynchronous coupling period is noted before the asynchronous coupling scheme has been introduced

We added information at the end of section 2.4.

> p575, l19: Diving straight into a numerical comparison of the model's pre-industrial climate with a variety of observations from the late 20th century without some justification of why this might be a valid thing to do feels a bit jarring.

We added the missing justification.

> p576,l17: There is an assertion here of need for 5km resolution to get Gulf Stream separation right with an implication that this alone would fix the north Atlantic SSTs - could this at least be referenced, if not nuanced?

We rephrased and added references.

> p578, l20: Given the biases in climate and icesheet representation that have been shown for the pre-industrial, it might be worth discussing the implications of that these biases are likely to have in the LGM climate?

We added comments relating to the Siberian glaciation at the end of the long term drift section (3.7). We comment on the Alaskan glaciation in the description of the Cordilleran part of the Laurentide Ice Sheet in section 3.7.2.

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p579, l24: as noted later, spinning up the LGM icesheets toward a steady state for 30kyr with a constant forcing is not really the correct experimental procedure for getting things like the internal temperature profile, and thus the flow behaviour, correct - I think this is an appropriate place to caveat the spin-up technique used here.

We extended the description of the spinup. The spinup was substantially longer than 30 kyrs.

p580,l10-17: I can see why want it might be useful introduce some overview of the final results here, but this paragraph feels awkward and out of place.

We added an introductory sentence that smoothes the transition and clarifies why this paragraph is needed in this place.

p581, fig 2: The surface temperature responses of the model are clearly an important thing to quantify, and I found the all-blue colour scale of the bottom two panels of figure 2 didn't make that easy, particularly over the polar/ice-sheet regions that are likely to be of most interest to readers of this paper. An explicit comparison with the LGM temperature reconstructions at the locations of the various Greenland/Antarctic ice-cores would be of interest.

We redrew the plots with a modified color scale that is more optimized for the ice sheets and focus on the northern hemisphere. We obtain an LGM cooling of about 13 degrees in the interior of Greenland. This is clearly below the reconstructed -21 degrees for the ice cores (Cuffey et al., 1995; Dahl-Jensen, 1998). Kim et al. (2008) showed that an increase in resolution (from T42 to T170 in their study) strongly increases the modeled

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Greenland cooling under LGM conditions (from 16 to 23 degrees in their study). Since we run the model on a coarse T31 resolution, we prefer to focus more on the large-scale effects instead of individual locations.

> p581,l20: The "split" experiment briefly described here is arguably the most realistic setup in the paper, as it cuts out the influence of the spurious East Asian icesheet that full interactivity grows. Much more could be made of this setup, depending on what the authors choose to be the main focus of the paper - this setup of course removes the essential climate-icesheet consistency of the model state.

The split experiment actually is not realistic, since the glacier mask does not match the topography. Its only purpose is to split the difference between the ICE-5G experiment and the coupled experiment into the effects of topography and those of the glacier mask. It shows that a large part of the climatic difference stems from the differences in the topography that affect the large scale atmospheric circulation.

> section 3.6: Whilst containing a wealth of detail, no particular conclusions seem to be drawn from this section

We shortened and focused the description.

> p587,l18: "large time fractions" is not good English

We rephrased.

> Figure 8: The seaice extent lines vs continental outlines are not always easy to differentiate

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We adjusted the figure.

> Figure 4: Assuming we're most interested in precipitation over ice-sheets, the domain shown and colour-scale in this figure could be improved.

We focused the plot on the northern Hemisphere and adjusted the color scale.

References

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Interactive comment on Clim. Past Discuss., 10, 563, 2014.