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Climate of the Past Discussions

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

Anonymous Referee #1

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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.

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 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.

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

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

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

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

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?

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

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

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?

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

p575, l19: Diving straight into a numerical comparison of the model's pre-industrial cli-

mate 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.

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?

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?

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.

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.

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.

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.

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

drawn from this section

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

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

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

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