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Climate of the Past Discussions

Interactive comment on "Application of an ice sheet model to evaluate PMIP3 LGM climatologies over the North American ice sheets" *by* Jay R. Alder and Steve W. Hostetler

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Reviewer: Irina Rogozhina, University of Bremen

The manuscript of Adler and Hostetler evaluates the performance of eight climate simulations of the Last Glacial Maximum included in the PMIP3 and one in-house climate simulation over North America and Greenland. They use a thermodynamic ice sheet model CISM to argue that two GCMs produce excessively warm conditions over the region that could not possibly sustain realistic geometries of the former North American ice sheet complexes. Although the other seven climatologies are generally consistent with existing reconstructions of the LGM ice sheet extents in North America, four of

them produce an undocumented ice cover across Beringia. To evaluate the robustness of their conclusions, the authors assess the sensitivity of their modeled ice sheet geometries to the choice of degree-day factors in the positive degree day model and an ice flow enhancement factor.

Regardless of its rather simple modeling approach, I find this study insightful. In particular, this work shows that modeled climate conditions are often inconsistent with the prescribed ice sheet boundary conditions and that ice sheet models could serve as valuable tools for evaluation of climate models in areas where paleoclimate proxy data are absent. Thus, I believe it merits publication in CP, albeit after moderate revisions. I have a number of suggestions on how to improve the analysis presented and fill the gaps in the model description.

The model description is incomplete and has to be improved. Even though the model setup is partly adopted from Gregiore et al. (2012), all parameter values and a short description of different model components should be included in the present manuscript (e.g., the description of the GIA model, underwater ice scheme and basal sliding law are missing; there is no mention of the geothermal flux forcing; daily temperature standard deviation value used in the PDD scheme is not provided, etc.). It is not clear how the authors initialize their ice sheets. While their initial ice configurations are based on the ICE-6G reconstruction, it is not clear what the authors prescribe inside of these ice sheets – how are the initial ice temperatures and ages derived? I suggest that in their revised manuscript the authors carefully detail their simulation design and methods.

Improvement of the main set of experiments:

The authors have suggested an interesting approach to an ice sheet model initialization in an attempt to start simulations from ice configurations, which are broadly consistent with the boundary conditions prescribed in climate models. We all struggle with the choice of model initialization strategies, when only equilibrium GCM outputs of certain time slices are available, whereas ice sheet configurations during these time intervals represent products of the preceding long-term climate history and a gradual ice sheet buildup. At this instance I disagree with reviewer 1 that the glacial index approach is a consistent way to initialize an ice sheet model, since it implies that (i) climate conditions of let's say MIS3 can be derived as some intermediate state between the LGM and present-day climate fields; (ii) climate conditions at the peak MIS5e could be extrapolated beyond the warmth of the Holocene period using the LGM and present-day climate fields; (iii) Greenland (or Antarctic) ice core reconstructions can reproduce air temperature (and precipitation) variability across the entire globe. Climate proxy data and geological evidence from several regions around the world show a very different picture, for example Central Asia and the Russian Arctic (including Beringia discussed in the text). Glacial expansions in the Kara Sea region during the last glacial cycle were obviously asynchronous with the rest of the continental glaciations in the Northern Hemisphere (Patton et al., 2015). Hence, I feel it is important that the ice sheet modeling community (concerned with both former and present-day ice sheet reconstructions) starts looking for new methods to initialize their ice sheet models. The suggestion of Adler and Hostetler is a good initial step towards this goal. Although their idea is interesting, it may be meaningless from the physical point of view, especially if the authors do not ensure that the ice sheet model has had sufficient time to recover from the initial shock arising from unrealistic initial thermal and dynamical ice regimes. In essence, their initial ice sheet configurations are empty shells, which are inconsistent with the physical laws underpinning the ice sheet model used. Since some of the North American ice sheets were very thick at the LGM (comparable in thickness with the present-day Greenland and Antarctic ice sheets), they may need a rather long initialization time (several tens of thousands of years) to forget this initial non-physical shock. I believe that the integration time the authors have adopted is insufficient to achieve this goal. One way to demonstrate that the initial shock is not altering the general conclusions of the study is to perform an additional test forced by one of the GCM model outputs, which are not entirely unrealistic (e.g., GENMOM or MPI). In this test the ice sheet model should be initialized over a time period of 20-50 thousand years

using a time-invariant climate forcing and keeping the ice thickness fixed as is done for the present-day ice sheets (e.g., Pattyn, 2010). After this initialization, the authors could run a forward simulation with a free ice thickness evolution and compare their modeled extents to the ones presented in the current version of the manuscript. Another way would be to run their simulations for a longer time period (20 thousand years at the least). According to the current state of knowledge, a large portion of North America had been buried under ice sheets over a period of more than 50 thousand years during the last glacial cycle. Hence, longer integration times would not hurt.

Improvement of sensitivity tests:

The authors have performed an extensive analysis of the uncertainties in the degree-day factors but have disregarded potential effects of the daily temperature standard deviation (in the PDD scheme), which is rather uncertain in nature (e.g., Fausto et al., 2009; Seguinot, 2013; Wake and Marshall, 2014). Their choice of a snow-to-rain fraction (100% vs 0%) and a meltwater retention scheme could be also objected. Should not these be included in their sensitivity tests? I realize that such tests would add many simulations to the story but running them with a resolution of 40 km and a SIA-only model is more than feasible. All sensitivity tests could be placed in the supplement (including the tests of sensitivity to degree-day factors)

Typos and concerns: Page 1: PMIP3 -> correction suggested by reviewer 1 Page 3, line 15: An awkward statement: 40-km North American domain - refomulate Page 7, line 23: prescribe -> prescribed Page 8, line 10: Do you really mean “steep domes” here? Maybe “sleep margins”? Page 8, lines 11-12: This statement is difficult to believe. Could we see a proof of it in the supplement? Page 8, line 28: Typo: mantle Page 10, line 1: Why are margins idealized? Is it not a product of the initialization chosen? Page 13, line 14: delete one “is”

References:

Patton, H., K. Andreassen, L. R. Bjarnadóttir, J. A. Dowdeswell, M. C. M. Winsborrow, R. Noormets, L. Polyak, A. Auriac, and A. Hubbard (2015), Geophysical constraints on the dynamics and retreat of the Barents Sea ice sheet as a paleobenchmark for models of marine ice sheet deglaciation, Rev. Geophys., 53, 1051–1098, doi:10.1002/2015RG000495

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