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# *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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Response notation as follows:

Referee comment (RC1): standard text

Author comment (AC): *italicized text*

RC1: The biggest problem with the present manuscript and my reason to advice rejection is the ice dynamical aspect of the modelling. As the authors write themselves P10,6, the relaxation time of 5000 years is not enough for the model to reach steady state. The chosen period is clearly too short for the ice flow to sufficiently respond to the imposed SMB forcing. This means that the ice sheet is in an arbitrary state after 5000 years as it relaxes from the assumed initial reconstructed geometry to balance with the imposed forcing. This is a fundamental flaw in the experimental setup.

*AC: Our experimental design was chosen so that we could compare the simulated areal footprint of the ice sheet to that of the driving GCM. The modeled surface mass balance and areal extent stabilize surprisingly quickly, with the exception of Beringia which can take tens of thousands of simulated years to stabilize (Figure R1). We do note in the text (P10,6) that the ice sheet is not in steady state thermodynamically, but we felt a short simulation period was sufficient for our purposes. We apply the ice sheet model as a sensitivity tool to validate the GCM climatology. To address the concerns from both reviewers, our revised manuscript will analyze the models after 50,000 years of simulation where the ice sheet is in or near steady state. This will most likely require the modification or removal of the discussion of ice volume (Section 3.4) as comparing simulated volume after 50,000 years of constant climate forcing relative to ICE-6G is not appropriate.*

RC1: Although the ice sheet model is described as thermodynamic, I see no evidence that the ice temperature is evolved or even initialised. This would have to be clarified.

*AC: Both reviewers are correct, in that we omitted a description of how the ice sheet temperature was initialized. Ice temperature was initialized at 0 $^\circ C$ as the ICE-6G reconstruction provides no information on ice temperature or flow vectors. This is a limitation of the experimental design. It is clear from further analysis (Figure R2) that it takes tens of thousands of years for the model to recover from the initial non-physical shock of initializing with 0 $^\circ C$ ice, to use RC2's wording. This will be addressed in the revised manuscript and Figure R2 will be included in SI. Even though the lower levels of the ice sheet stabilize after 50,000 years, temperature continues to drift in the upper levels in some models. We feel that the model temperature profile is sufficiently spun up after 50,000 years and that will be an appropriate window for our revised analysis.*

RC1: However, it is important to realise the main difference between the options discussed to arrive at an initial state (btw. I don't understand the second option for initialisation at P4,1 without a reference). While the result of the first approach is a fully self-consistent (thermo-)dynamic ice sheet model state, this is not the case for the given choice of imposing a reconstructed geometry.

*AC: A climate index (or glacial index) method of initialization is desirable from an ice sheet modeling perspective because it allows the ice sheet to develop slowly over a long period of time and produces a thermodynamically self-consistent model state. However, as RC2 correctly points out, simply linearly interpolating the climate forcing between LGM and pre-industrial is not realistic. Interpolating between LGM and pre-industrial climate states is a stopgap when very long transient glacial cycle simulations are not available, but GCM snapshots are (i.e. PMIP3). Initializing CISM2 with large ice sheets with no internal structure is also physically unrealistic, but the climate forcing is consistent with the initial ice sheet geometry. Both initialization methods have unphysical limitations, but we choose the later method because we are interested in if the PMIP3 model simulated climatology would support the large North American ice sheets they are driven by, as opposed to creating realistic ice sheet inception and chronologies (i.e. marine isotope stage events). We will clarify these points in the revised manuscript.*

RC1: Furthermore, differences in the SMB forcing between climate models imply that each GCM generates its individual response time scale dependent e.g. on the spatial SMB gradients. It is possible that the North American ice sheets were never really in balance with the climate during the LGM (something to discuss), but assuming an arbitrary period to evolve from an arbitrary initial state is certainly not an acceptable solution to this problem.

*AC: It is clear at this point that an analysis window of 5000 years was too short, but we disagree with the reviewer that the initial state is arbitrary. The initial geometry and volume corresponds to the ICE-6G reconstruction, which is an approximation for the* *static ice sheets used to drive the GCMs. The benefit of this is the forcing temperature and precipitation climatology are consistent with the initial ice sheet geometry. As we noted above, initializing the ice sheet model with no internal structure is a limitation, but we feel this will be mitigated by analyzing the ice sheet model output near steady state.*

RC1: Another problem with the present setup (that would at least need to be acknowledged) is that the "coupling" between climate and ice sheet model is reduced to the lapse rate effect on temperature. The further away the ice sheet geometry evolves from the ice sheet that was prescribed for the GCM, the less reliable are the climatic fields entering the calculations. This is in particular a problem where the land type changes e.g. from ice sheet to land cover, or vice versa.

*AC: This is implicit with one-way offline coupling (P5,4-8), but we will clarify the point in our revised manuscript. This limitation applies to both a climate index method and our own reconstruction based initialization method and can only truly be addressed through two-way coupling where a dynamic ice sheet model is included in an Earth System Model or EMIC.*

RC1: I believe a study relying entirely on the SMB component of the ISM would arrive nearly at the same conclusions as the present manuscript.

*AC: In a previous version of our manuscript we did compare the SMB at the first step of the ice sheet model to the SMB after 5000 years (essentially what the reviewer suggests). The contours of the Laurentide and Cordilleran Ice Sheets can largely be arrived at from SMB alone, but this is not the case for CNRM-CM5 and MRI-CGCM3 and is not always the case for ice development in Beringia.*

RC1: The statements P4,16 are clearly ignoring any flow of the ice, which indicates that the dynamic aspect of the ice sheet model is not really considered in the discussion anyway.





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*AC: These statements are overly simplistic, and will be revised in our manuscript, but we do not feel these statements are indicative that the overall paper ignores ice dynamics. See P7,12-16; P8,15-19; P8,20-P9,2 for examples.*

RC1: Further support for that approach could be drawn from the July panels of Figure 2, which give a good impression where a feasible ice sheet can exist. In that case, however, the SMB model (here PDD) would have to be treated with much more detail and a clearer correspondence with the underlying GCM results would be in place.

*AC: As with initial SMB, July temperature corresponds to locations of simulated ice presence in broad terms, but not in the details or in models that have marginal climatologies.*

RC1: An interesting additional check would be what climates the GCMs produce as present-day conditions (positive SMB for Greenland, ...) to distinguish models that are generally warm biased from models that are warm biased only at the LGM but OK for the present and the same for cold biases for the two periods.

*AC: Evaluating the piControl derived ice simulation is outside the scope of our paper which uses CISM2 as sensitivity tool to validate the PMIP3 LGM climatology.*

RC1: I am afraid this is unfortunate timing, but some, if not much of this work will likely be superseded by the CP discussion paper by Niu et al. https://doi.org/10.5194/cp-2017- 105.

*AC: The Niu et al (2017) paper largely focuses on the COSMOS model with limited discussion of PMIP3 GCM forcing. Their paper developed pseudo glacial volume histories via a glacial index, which was not our goal. We do not feel our analysis and discussion of PMIP3 LGM simulations (particularly our discussion on albedo) are in any way superseded by the Niu et al (2017) paper.*

Please also note the supplement to this comment:

https://www.clim-past-discuss.net/cp-2017-102/cp-2017-102-AC1-supplement.pdf

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-102, 2017.



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