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Interactive Comment

Interactive comment on "An ice sheet model of reduced complexity for paleoclimate studies" *by* B. Neff et al.

B. Neff et al.

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Revision Memo to Bas de Boer (18 September 2015)

Thank you very much for this constructive, detailed and positive review. It will significantly improve the quality of our manuscript.

In the following, we provide answers and clarifications to the most important comments, and give an outlook on how we plan to address shortcomings in the original version of the manuscript. A comprehensive reply will be provided with the revised manuscript after completion of peer-review.

Reviewer:
3. Are substantial conclusions reached? *No. Mainly the model itself is presented and tested with different climate forcing fields and compared with reconstructions of ice sheet topography.*

Response:
While it is true that a large part of the manuscript is dedicated to a thorough description and testing of the new model, we also show applications that are made possible by its numerical efficiency. The hysteresis simulations represent idealized tests that quantify ice sheet stability for different ice sheet volumes. To our knowledge, no comparable study exists for Northern Hemisphere ice sheets. Previous studies address this important research question with idealized models that do not explicitly represent the relevant physics (Paillard, 1998) or with time slice simulations (Abe-Ouchi et al., 2013). We believe that applications such as this in addition to the fundamentally important work of developing new tools should be considered substantial contribution to the published literature. The new tool and the analysis method we present enable a wealth of new research questions that are not feasible with existing models because of their numerical cost. We acknowledge that the original manuscript does not emphasize this point sufficiently and propose to revise the text accordingly.

Reviewer:
Bedrock relaxation *A more standard way of calculating the bedrock change is the ELRA (Elastic Lithosphere, Relaxed Asthenosphere) model rather than the local isostatic model used in the manuscript (see Le Meur and Huybrechts, 1996). At least a more elaborate discussion of the ELRA model, which can be readily implemented since you use a 2-D domain, would be good to include at some point. Other referencing that might be useful (e.g.): Zweck and Huybrechts (2005), Van den Berg et al. (2008). I do not require you to change your bedrock model, but please add a note on this also in the Conclusions and outlook for future work.*

ESDD
6, C626–C632, 2015

Interactive Comment

Response:
This is a very valuable comment. We will revise the manuscript to include a discussion of the bedrock relaxation model and the relevant literature. We will also explore the possibility to use a more sophisticated treatment of bedrock relaxation in a new additional simulation. However, as discussed in the present version of the manuscript, the usage of a constant climate forcing reduces the impact and importance of a relaxing bedrock. Changes in ice volume appear to be relatively insensitive to changes in the bedrock relaxation time scale. Therefore, we do not expect large changes with a different relaxation scheme either.

Reviewer:
Surface melt *Can you explain why you choose to use the PDD melt model with only one parameter and not two? Again, other types of models can be discussed. As shortly touched upon in the conclusions (bottom of page 1420).*

Response:
The ice sheet model has been written with a strong focus on simplicity and numerical efficiency, but also with very low requirements with regard to the expected forcing fields. Therefore, the model does not calculate the extent of the snow cover nor does it expect such data as a boundary condition in its present form. The second reason why we have not used a more complex version of the empirical PDD method to describe the surface mass balance is that we are currently working on a physical model to describe the snow and firn layer, extending work by Greuell and Konzelmann (1994) and Reijmer and Hock (2008), as outlined on page 1420.
There is a very general argument to be made on the use of empirical parameterizations with multiple free parameters. Since PDD parameters are only very weakly constrained by physics, they can be chosen from a very wide range of potential values. Adding a second parameter and another degree of freedom to the optimization will

ESDD
6, C626–C632, 2015

undoubtedly enhance the agreement with observed fields at the cost of computational efficiency and further weakening the physical basis of the model. In summary, we found that a single PDD factor yields results of sufficient quality for the envisioned applications of our model. We decided to rather use the minimal version of PDD to avoid overfitting while concentrating our efforts on a true physical surface mass and energy balance model for future applications.

Reviewer:
SMB *With respect to a statement on the seasonal cycle on page 1404. Please add a few lines in section 2.2 how you actually calculate SMB, this is a bit unclear. Do you calculate annual accumulation and ablation separate before adding up and interpolating to the ice grid, did you try to calculate SMB on a daily or monthly time scale perhaps?*

Response:
Temperature and precipitation data of the climate model are first interpolated onto the ice sheet model grid, before the accumulation and ablation terms are calculated individually. It is important to correct the temperature data for the changes in elevation due to the growing or decaying ice sheet and this can only meaningfully be done on the grid of the ice sheet model, hence after spatial interpolation. We use daily climatology data to better represent variations in surface air temperature and precipitation that in some regions show large variations. Also, especially in dry continental climates the bulk of annual accumulation might be due to sporadic precipitation events, i.e., a few moisture-bearing storms. We will rephrase and extend section 2.2 to include this additional information.
The sentence “The temporal resolution is one year which makes it impossible to implement a seasonal cycle in the SMB.” at the end of the first paragraph in section 2.3 refers to the time step of the ice sheet dynamics, not the SMB. As suggested by the reviewer in the specific comments, we will remove this sentence to avoid confusion.

Reviewer:
Also, I am wondering if the precipitation correction you apply in equation (5) on page 1403 is necessary when you already use an LGM precipitation field. These kind of effects on both precipitation and temperature are all included in the CCSM-LGM simulation, due to the boundary conditions that are applied (large ice sheets, lower CO2, etc.) and the use of an actual atmospheric model! I suggest you give the paper of Zweck and Huybrechts (2005) a good read. They have used both LGM and PD simulations of a GCM to force their ice-sheet model for the NH as well, using a glacial scaling factor to simulate ice volume over the last glacial cycle, also a good paper for comparison with another simulation.
Again, it should be made very clear if this desertification effect is needed when you use LGM climate forcing.

Response:
We agree that precipitation should not be corrected in regions of high elevation where the climate model topography already accounts for the same effect in a more sophisticated way. This comment is probably based on a misunderstanding due to unclear phrasing in our manuscript for which we apologize. Close inspection of equation (5) shows that the precipitation is reduced above 2000m only if the GCM topography is below 2000m also. Where the GCM topography is higher than that, this elevation is used as the reference. This will be clarified in a revised section 2.2.
Notwithstanding, we will compare our approach and results to the study by Zweck and Huybrechts (2005) in the revised discussions section.

Reviewer:
On the other hand, I think the height-desertification effect should be included when your reference climate does not include the ice sheets (for example over North America and Eurasia in the PD or PI GCM simulations). See also section 3.2 in de

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Interactive Discussion

Discussion Paper

ESDD
6, C626–C632, 2015

Boer et al. (2013) and references therein how this could be done when using a PD climatology as reference (might be something you could use for future studies).

Response:
As stated above, the height-desertification correction does take into account the different topographies of the GCM simulations. We will discuss our results with regard to de Boer et al. (2013).

Reviewer:
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? *Own contribution should be stated more clearly in the introduction. Some references are missing when discussing previous work. For example, you could add a few lines on other (ice-sheet) model studies that use an ensemble approach, for example: Robinson et al. (2011) or Stone et al. (2013).*

Response:
We will revise the introduction in order to further highlight the novelties of our approach. More specifically, since the type of model is not completely new, we will more clearly motivate the use of simplified ice sheet models and how they complement recent studies.

Reviewer:
10. Is the overall presentation well structured and clear? *The structure of the paper is good. I suggest section 4.2 is moved to the front of section 4 (so will be section 4.1), such that the model is first tested for the pre-industrial case.*

Response:
We agree that this order may appear preferable, but we believe it would mislead readers and confuse the purpose of the optimization procedure. We optimized the model

only for LGM data and use the preindustrial simulation primarily as a cross-validation test. It is possible that the best model version for the LGM produced unrealistic amounts of ice or spurious ice sheets when forced with preindustrial climate. This is not the case which supports the optimization scheme.
We will revise the manuscript toward making this rationale clearer but we would like to keep the present ordering of sections.

References:

D. Paillard (1998), The timing of Pleistocene glaciations from a simple multi-state climate model, Nature 391, 378-381, doi:10.1038/34891

A. Abe-Ouchi et al. (2013), Insolation-driven 100,000-year glacial cycles and hysteresis of ice-sheet volume, Nature 500, 190-193, doi: 10.1038/nature12374

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1395, 2015.