The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-235-SC1, 2018

Interactive comment on "A note on the influence of atmospheric model resolution in coupled climate–ice-sheet simulations" *by* Marcus Löfverström and Johan Liakka

I. Rogozhina

irogozhina@marum.de

Received and published: 17 January 2018

I have major concerns about the experimental design of this study and how the authors' choices affect the main conclusions of their manuscript. One of such choices is the lapse rate correction. A temperature lapse rate correction is used to derive the temperature forcing at the beginning of ice sheet simulations, which are initiated from the present-day ice-free topography (excluding the Greenland Ice Sheet), and to adjust the temperature forcing to the growth of ice masses throughout the simulations. I question both (1) the choice of the lapse rate correction and (2) the assumption of the initial ice-free conditions, which are undermining the very core of this study.

Problem (1): While the authors discuss potential impacts of PDD parameters, they do not question their choice of the temperature lapse rate (6.5C/km). Fausto et al. (2009) measured lapse rates as low as 4.7 and 4.6C/km during the June and July months, respectively (the months of the strongest ablation) on the Greenland ice sheet. This is nearly 2C/km below the value that the authors use throughout the year. The temperature lapse rate is used by the authors to correct the model-based air (or surface) temperatures for the difference between their LGM topography in the atmospheric simulations and ice-free present-day topography assumed at the beginning of their ice sheet simulations. This initial temperature forcing is crucial to the development of an ice sheet: If it is excessively above 0ïĆřC during the summer period, an ice sheet will not build. A difference of $\sim$2C/km would reduce the corrections of the near-surface temperatures over the areas covered by the former British-Irish and Fennoscandian ice sheets by 3-4C during the months that matter most for the ice sheet surface mass balance, but would not impact Arctic Siberia. Quoting from the study of Löfverström and Liakka, page 1, line 9: "Sensitivity experiments using different surface mass balance parameterizations improve the simulations of the Eurasian ice sheet in the T42 case, but the compromise is a substantial buildup in Siberia". This compromise does not have to be made: The choice of a lower lapse rate correction will trigger a buildup of ice masses over the British Isles and Fennoscandia when using the T42 & T31 climate datasets, reasonable choices of PDD parameters and a higher ice sheet model resolution (see below) but will still keep Arctic Siberia ice free.

Problem (2): A more important question is whether such corrections should be applied at all. While talking about "the influence of atmospheric model resolution in coupled climate-ice sheet simulations" (quoted from the title of the paper), one would rather think about whether the use of lower atmospheric model resolution contaminates the climate state in such a way that it becomes inconsistent with the modeled/prescribed ice sheet geometries (included as a topographic boundary condition in a climate model). The question is not whether this climate forcing can build ice sheets if it's heavily modified using lapse rate corrections but whether it can maintain reasonable ice sheet geometries when unmodified atmospheric model outputs are used.

While Problem (1) can be easily tackled by performing additional sensitivity experiments, Problem (2) is more challenging to resolve. The authors could test T42, T31 and T21 datasets using ice sheet modeling results from the T85 dataset “reproduced to a high accuracy” (quoted from Löfverström and Liakka, page 1, line 8) to address the question, whether the degradation of the atmospheric model resolution results in ice sheet collapses consistent with their current conclusions. However, their modeled ice sheets in the T85 simulation are 1.5 to 2 km too thick relative to the existing reconstructions of the LGM ice sheet geometries. Even without additional lapse rate corrections (introduced to reconcile the difference between the ice sheets prescribed in the atmospheric simulations and derived from the T85 simulation), removing such thick ice sheets would be a difficult task for the T42, T31 and even T21 data sets. At this point a question arises: Why are the modeled ice sheets so unrealistically thick? I envision several potential causes of such unrealistic model performance: (i) The spin-up of the ice sheet model: Running an ice sheet model to an equilibrium with the LGM climate over 150 thousand years is not in line with the existing evidence. Most of the former ice sheets were short-lived (tens of thousands of years from buildup to decay) (ii) Shallow ice approximation (excluding ice stream dynamics) in combination with excessively low resolution of the ice sheet model (80 km) fails to approximate the rapid ice flow and routing of ice masses towards the ocean. (iii) The isostatic adjustment scheme may cause an exaggerated bending of the bedrock surface under the weight of growing ice sheets (I have not seen the Local Lithosphere and Relaxing Asthenosphere method being used in years).

The overall quality of the study could improve if the authors address problems (1) and (2). It can also benefit from the use of SICOPOLIS v3.3 that includes options for higher resolution, more realistic treatment of ice streams and glacial isostatic adjustment. Finally I strongly suggest that the authors improve their figures. The adopted projection strongly distorts the Arctic region, which is the main focus of the present study.

References Fausto, R. S., Ahlstrøm, A. P., Van As, D., Bøggild, C. E., and Johnsen, S. J.: A new present-day temperature parameterization for Greenland, J. Glaciol., 55, 95–105, 2009.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-235, 2017.