

Interactive comment on “Modeling surface response of the Greenland Ice Sheet to interglacial climate” by D. Rau and I. Rogozhina

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The authors make use of the ice-sheet model SICOPOLIS to explore the evolution of Greenland during interglacial climate and its sensitivity to different PDD schemes. The PDD approach is commonly used in ice-sheet modelling studies to compute ablation. It is based on an empirical formulation that relates snow and ice melt rates (through degree-day factors) to the sum of the excess of temperatures above 0°C. In such formulations, daily temperatures are assumed to be normally distributed, and the daily temperature variability is parameterized through the standard deviation (SD) of the normal distribution. The authors show that the usual assumption of a spatially uniform value of the SD parameter does not provide surface mass balance estimations that fit with estimations from available datasets. By reconstructing a spatial distribution of SD

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values they largely improve the SMB simulations which, thereby, favorably compare with satellite observations and outputs from a high resolution model. In a recent paper, Charbit et al. (2013) investigated the extent to which the evolution of past northern hemisphere ice sheets through the last glacial cycle was sensitive to the choice of the PDD scheme, and highlighted the great impact of the daily temperature variability in this evolution. The best agreement between their simulated ice sheets and available LGM reconstructions was obtained for an altitudinal dependency of the SD parameter. They also concluded on the importance of refining the PDD parameters (and especially SD) by carrying out inter-comparison studies with the use of high-resolution climate-detailed snow models. Although, the approach presented in this paper slightly differs from the previously suggested one, I am fully convinced of the importance of such studies. Nevertheless, I have a number of remarks and questions that should be addressed before the final publication.

Specific comments :

1. Model spin-up: I fully agree with the detailed comments of Andy Aschwanden (see “Short Comment”) concerning the spin-up procedure. Therefore, I am a bit doubtful about the robustness of the comparison between the SMB from the transient simulations and the SMB coming from observations or outputs from high resolution models. A new spin-up procedure based on an inverse method of velocity fields has recently been used in different ice-sheet model studies (e.g. Gillet-Chaulet et al., 2012). This method seems to become “the standard way” in the ice sheet modeling community to initialize ice-sheet models under present-day conditions. Although I am aware of the fact that implementing this new method may represent a huge amount of work, I think that the authors should at least address how their results are biased by their own spin-up procedure. If the bias (as suggested by Andy Aschwanden) dominates the signal or is of the same order of magnitude, I recommend performing new simulations with a more appropriate initialization of the model.

2. Following the approach they had used to derive a temperature parameterization

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(Fausto et al., 2009a), Fausto et al. (2009b) assumed that the SD values can be expressed as a sum of linear functions depending on altitude, latitude and longitude; they then applied a least-mean square fit to the observed SD values from automatic weather stations. This implies a non-spatially uniform distribution of SD. To my knowledge, Fausto et al. (2009b) were the first to propose a spatially dependent formulation of SD that can be implemented in a PDD scheme. Although the approach presented in this paper is a bit different (here the spatial SD distribution is derived from the ERA-40 temperature time series), the authors come to a similar qualitative conclusion (i.e. a strong dependency on the altitude) but with different numerical values. Therefore, a comparison with the Fausto et al. formulation is crudely lacking in the manuscript be addressed (and tested) in the revised version. As a result, it is difficult to have a clear idea of the novelty of this study.

3. Validation against satellite observations. At the bottom of page 2711, the authors explain they calculated trends in SMB by subtracting the contribution of ice discharge provided by RACMO2 from the total mass trends derived from satellite observations. I guess they intended to explain the opposite because since RACMO2 does not simulate ice discharge. In other words, I assume they mean they subtracted the SMB provided by RACMO to the total mass trend provided by satellite observations. Could they confirm ? If I am right, this supposes that the SMB simulated by RACMO is in full agreement with observations, which represents a huge assumption. In any case, I think that the approach should be better explained and the biases it may introduce should be discussed.

Minor points

Abstract line 1: use "parameterize" instead of "parameterizing"

p 2705, line 1-2 : references with fully coupled climate-ice sheet models could also be added.

p. 2706 : Which reconstruction of eustatic sea level has been used ? Add also the

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reference to Fox Maule et al. (2009) for geothermal heat flux before section 2.2.

p. 2706, lines 18-19 : The definition of the PDD is misleading : PDD is the integral of temperatures above 0°C over one year.

In section 2.1, a few words about the degree-day factors (Cice, Csnow) and the refreezing process should be added.

p. 2708 : lines 6-9 : see specific comment 2.

p.2708 and table 1 : Note that the refreezing scheme used in Tarasov and Peltier (2002) is basically the same as the one proposed by Janssens and Huybrechts (2000) except for the thickness of the thermally active layer. Tarasov and Peltier used a fixed value of 1 meter whereas Janssens and Huybrechts (2000) considered a variable thickness equivalent to the annual snow accumulation. Another difference lies in the dependency of the ice specific heat capacity (note it is also given in Jkg-1K-1 in Tarasov and Peltier). Anyway, we carried out some numerical experiments to test the sensitivity of our ice-sheet model (GRISLI) to different thicknesses of the thermally active layer and to different formulations of heat capacities and found that the simulated amounts of ablation (for the whole Greenland ice sheet) only differed by a few per cents. At the opposite, the right panel "Total" in Figure 4 exhibits huge difference between SMB from Huybrechts (2002) and from Tarasov and Peltier (2002). Could the reasons at the origin of these differences (I suspect there is no matter with the refreezing scheme) be discussed ?

p. 2709: Why some drainage basins are more sensitive than others to a doubling of SD values ? According to Figure 3, it does not seem to be only related to the elevation (basin C in the eastern part is almost insensitive to the SD doubling). This could be commented. Moreover, could the authors briefly explain on which basis the drainage basins have been defined ?

p.2710: the reference period should be clarified and it seems to me that there are a few

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inconsistencies in the text (unless I missed something). Why does the reference period span from 1958 to 2001 since the ERA-interim dataset goes until 2009 (see section 2.2, p.2707) ? In addition, the spatial distribution of SD is derived from the ERA-40 dataset (1958-1988), although Figure 2 caption mentions that the “monthly values of SD are derived from the ERA-40 temperatures time series (1958-2001) ? Why the ERA-Interim temperatures have not been used to derive the new SD parameterization ? How the results would have been affected by the use of longer time series (1958-2009) ?

p. 2710, lines 17-18 “and falls within the range of other independent estimates close to the upper bound of the estimated range” : these independent estimates (those found in the paper of Vernon et al; 2012 ?) should be quantitatively mentioned. The comparison with other high-resolution models should be further commented, although the drainage basins are not exactly the same in Vernon et al (as an example) and in the present study.

P 2711, line 1 : Add “are” between “results” and “in”

Figure 1 and Figure 4 : The different panels should be removed from the maps and put on the right (or left) side of the main figures. Moreover, the frontiers of the different drainage basins should be superimposed on the maps.

Charbit S., C. Dumas, M. Kageyama, D.M. Roche and C. Ritz, Influence of ablation-related processes in the build-up of simulated Northern hemisphere ice sheets during the last glacial cycle, *The Cryosphere*, 7, 681-698, doi: 10.5194/tc-7-681-2013, 2013.

Fausto, R.S., A.P. Ahlstrom, D. Van As, S.J. Johnsen, P.L. Langen, K. Steffen, Improving surface boundary conditions with focus on coupling snow densification and melt-water retention in large-scale ice-sheet models of Greenland, *Journal of Glaciology*, 55, No. 193, 869-878, 2009b

Interactive comment on *The Cryosphere Discuss.*, 7, 2703, 2013.

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