Final response to the reviews of "Modeling surface response of the Greenland Ice Sheet to interglacial climate" by Rau and Rogozhina.

Our responses are given in brown

Response to short comment on initialization procedure by A. Aschwanden

We wish to thank Andy Aschwanden for his comments that allowed us to make our conclusions more robust.

I. I am not an official reviewer, but I have a comment that I'd like to share, hoping to improve the manuscript. I'm wondering if the authors could provide a justification for the initialization ("spinup") procedure. A fixed-surface initialization leads to an initialized ice sheet which is not in equilibrium with its climate and contains unphysical transients. As soon as the surface is allowed to evolve, the modeled ice sheet will quickly adjust towards a state that is in equilibrium with the forcing. The timescale of this adjustment is at least on the order of the length of the transient simulations made here (50 years). Consequentially, the response will be a mix of this adjustment and the applied reanalysis climate. Thus any interpretation of model results will be biased. This bias can be large, possibly dominating the signal over the modeled 50 years. Applying a surface relaxation may help to remove unphysical transients (e.g. Seddik et al., 2012; Gillet-Chaulet et al., 2012) or a different initialization procedure may be more suitable for this type of sensitivity study. For illustration, Figure 1 compares time series of mass change for two hindcasts, both forced with climatic mass balance and 2-m air temperature from RACMO for 1958–2011. One was obtained with a fixed surface (as in this manuscript) and the other with a free surface. While the differences are striking, this may be not used to make a case for one or the other initialization method. To detect whether a simulation is biased by unphysical transients, flux divergence or surface elevation changes are probably better metrics than the total mass change I've used in my illustration. In any case, validation with independent metrics is needed (c.f. Aschwanden et al., 2013). I thus recommend that the authors provide a strong case that their simulations are not strongly affected by unphysical transients.

Here we analyze the effects of fixed ice surface spin-ups on the modeled evolution of SMB of the Greenland Ice Sheet on decadal time scales. Following the suggestion of the second reviewer, Nicole Schlegel, we redesigned all experiments presented in our study by applying the present-day observed ice surface elevation and keeping it constant throughout the transient simulations. Taking into account that actual changes in ice surface elevation of around ±20 cm/year (Thomas et al., 2008) over the period in question (1958 - 2010) have negligibly small effects on the modeled surface responses of the Greenland Ice Sheet, especially on a coarse grid of 10 km × 10 km, the assumption about unvaried ice surface elevation is well justified.
To assess the effects of unphysical transients associated with the use of fixed ice surface spin-up procedures, we compare the modeled SMB time series from newly designed experiments (neglecting the evolution of ice surface over the period of 1958 - 2010) and previously used experiments with free surface evolution over 1958 – 2010 and initial conditions provided by the fixed-ice surface spin-up. In Figure 1R we exemplarily show the outputs of simulations using the SMB parameterization of Huybrechts [2002] combined with the retention model of Janssens and Huybrechts [2000]. Although the SMB values averaged over the reference period of 1958 to 2001 do not differ significantly, the evolution of the modeled SMB is indeed affected by the effects of the initialization procedure. This difference is especially pronounced over the years of SMB maxima and minima where free ice surface simulations tend to exaggerate the amplitudes of SMB variation and most importantly, over the satellite observation period (2003 - 2010) where these exaggerate the degreasing trend in SMB.

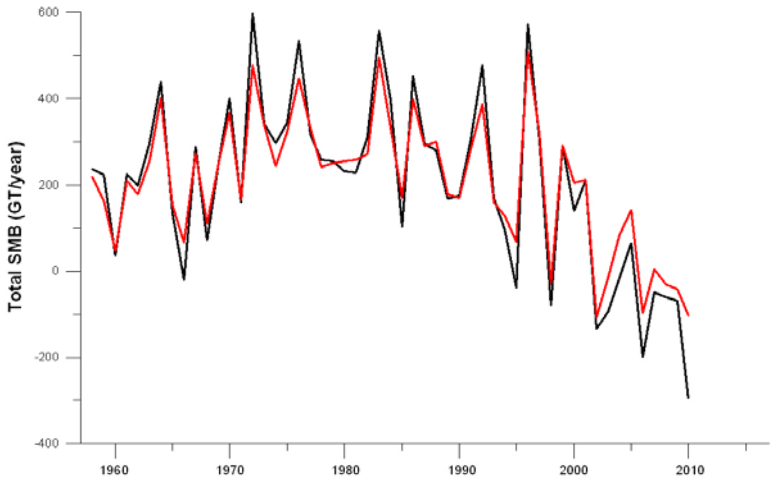

Figure 1R. Comparison of the modeled SMB time series of the Greenland Ice Sheet derived from simulations with the ice surface kept fixed at the observed elevation over the simulation time (red) and with the free-surface evolution enabled after the fixed-ice topography spin-up procedure (black).

II. Minor comment: The manuscript states: "... in order to validate a number of existing SMB parameterizations and our new approach against the results of the high-resolution model RACMO2/GR and recent satellite observations." I think this is an unlucky choice of language, and what I believe the authors meant to say is: "... in order to validate a number of existing SMB parameterizations and our new approach against recent satellite observations and compare to the results of the high-resolution model RACMO2/GR." Validation, even if interpreted somewhat loosely (as often the case in glaciology), means comparison to observations. Therefore, validation against another model is not permissible. One may weigh in that some modeling is required to obtain time-series of mass change from the L1 GRACE signal. However the major difference is that GRACE measures mass changes directly, and modeling is only needed remove contaminations in the signal (e.g. GIA).

We kept the difference between "validation against observations" and "comparison with regional models" in mind while rewriting the text.

References:

Thomas, R.H., K. Davis, E. Frederick, W. Krabill, Y.Li, S. Manizade, C. Chreston and C. Martin, 2008. A comparison of Greenland Ice Sheet volume changes derived from altimetry measurements. Journal of Glaciology, 54:203-212.

Response to the Reviewer 1 (S. Charbit)

We thank Silvie Charbit for thoughtful and detailed review of our manuscript. Many of her questions and suggestions enabled better understanding of the influence of different model parameters on the modeled regional SMB, this complemented by considerable improvements in the presentation of our results. In addition, it is always extremely helpful to learn that not keeping up with existing literature may result in extremely laborious revisions, which we will keep in mind for our upcoming submissions.

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 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 climatedetailed 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 spinup 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.

We have addressed the question of initialization procedure and discussed changes in our simulation setup in the response to Andy Aschwanden's short comment.

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

2. We have added the analysis of Fausto's SMB parameterization in our regional analysis and discussion of modeled SMB. We have also compared the parameterized SD values with the ones derived from ERA-40 (see Figure 2R) and discussed potential reasons of significant discrepancies in parameterized and ERA-40-derived SD values such as for example, short period of observation (2 months to maximum 3 years at low elevations, 10 years at higher elevations), poor coverage by weather stations (only 27 stations for entire Greenland), low resolution and simplifications associated with the use of reanalysis datasets, etc. To emphasize the novelty of this study, we have designed our simulations using seasonally and spatially variable SDs instead of mean summer SD as in our initial submission. This would not be possible using the parameterization of Fausto et al. [2009].

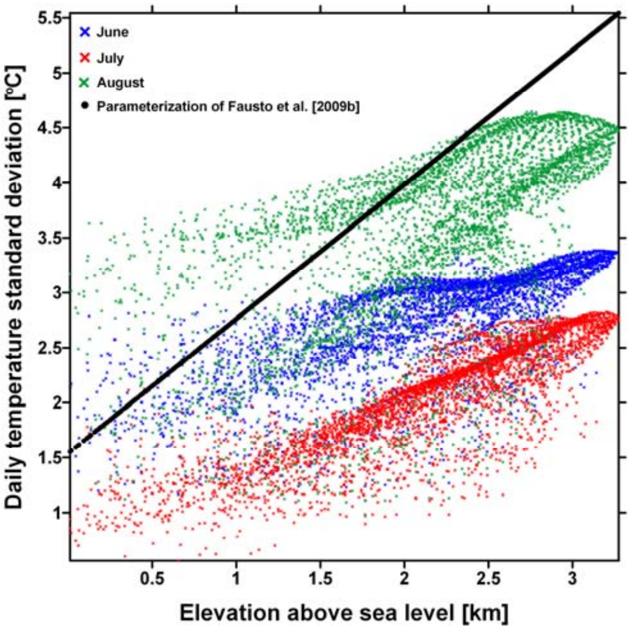

Figure 2R. Daily temperature standard deviation (SD) for June, July and August months obtained from the analysis of ERA-40 temperature time series (1958 - 2001) versus the parameterization of mean summer SD of Fausto et al. [2009].

3. We use trends in ice discharge from Sasgen et al. [2012] where authors employ regional estimates of ice discharge derived from the InSAR data by Rignot and Kanagaratnam [2006]. We apologize for having mistaken the source of these data in our initial version of the manuscript. Chapter 3.3 has been rewritten, accordingly. The biases introduced by this approach are very well described by Sasgen et al. [2012].

Minor points

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

Done

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

Done

III. p. 2706 : Which reconstruction of eustatic sea level has been used ? Add also the reference to Fox Maule et al. (2009) for geothermal heat flux before section 2.2.

Eustatic sea level is kept fixed at its observed present-day level. Following the suggestion of the reviewer 3, we have removed all details on the ice-sheet model aside from the SMB model description.

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

Rewritten

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

Done

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

Addressed – we included the parameterization of Fausto et al. [2009] in our analysis.

VII. 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 ?

The main source of discrepancies between the modeled SMB from the parameterizations of Huybrechts [2002] and Tarasov and Peltier [2002] is the difference in degree-day factors (DDFs) and SD. The largest effects on the modeled SMB come from different DDFs for ice, especially due to the large difference between DDFs for 'cold' ice (17.22 vs 8) in the north of Greenland distinguished by the parameterization of Tarasov and Peltier [2002] (Figure 3R). The difference between DDFs for snow is not as significant as well as its influence on the modeled SMB. Finally SD values are similar in both parameterizations, both very high (5 and 5.2), with relatively small effects on the modeled SMB, of the same order as are the effects of DDFs for snow. The effects of different thicknesses of the thermal layer in the retention model of Janssens and Huybrechts [2000] and different heat capacity formulations are found to be negligibly small.

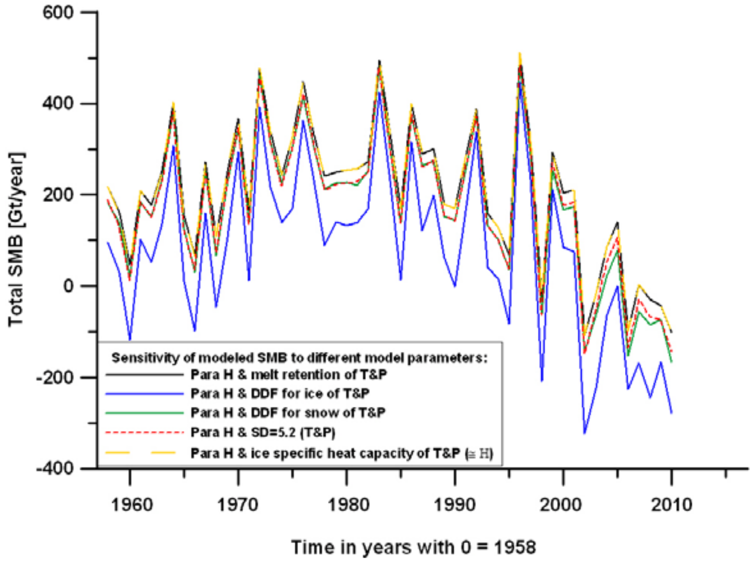

Figure 3R. Contribution of SD (5°C as in Huybrechts [2000] and 5.2°C as in Tarasov and Peltier [2002]), degree-day factors (3 mm/(°Cd) for snow and 8 mm/(°Cd) for ice as in Huybrechts [2002], and 2.65/4.3 mm/(°Cd) for 'cold/warm' snow and 17.22/8.3 mm/(°Cd) for 'cold/warm' ice as in Tarasov and Peltier [2002]), and thickness of the thermal layer in the retention model of Janssens and Huybrechts [2000] (2 m in Huybrechts [2002] and 1 m in Tarasov and Peltier [2002]).

VIII. 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 ?

Since the regional sensitivity of runoff/SMB also depends on the choice of DDFs and retention model, we decided to remove this figure and related discussion from the manuscript. In the initial Figure 3, high DDFs adopted by the parameterization of Greve [2005] in the north of Greenland enhanced the influence of higher SD. The low sensitivity of the regional SMB in the south-east of Greenland is also related to overall low runoff rates as shown in the former Figure 3. Please, also see our response to the comment of the reviewer 3 and Figure 4R.
7 drainage basins have been adopted from Sasgen et al. [2012] because our modeling study presents one-to-one comparison with their estimates.

IX. p.2710: the reference period should be clarified and it seems to me that there are a gew 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) ?

We have chosen the reference period of 1958 – 2001 because it is characterized by relatively stable total SMB of the GIS (according to RACMO2/GR), followed by abrupt changes in SMB in the period of 2002 - 2010 (e.g., see Figure 1R). It is therefore meaningful to operate with mean regional

SMB values over the chosen reference period in order to evaluate the performance of different parameterizations. Following the suggestion of the reviewer 2, we have also added comparison of SMB time series over the reference period to the former Figure 4.
We derive the spatial distribution of SD from the entire ERA-40 dataset spanning 1958 – 2001. This has been clarified in “Methods”. We have chosen to use the longest dataset covering 44 years, which is (i) sufficiently long to obtain robust estimates and (ii) covers the period of relatively stable SMB discussed in the previous paragraph. (iii) The ERA-Interim dataset has been processed using an essentially different assimilation technique, which could potentially introduce unwanted effects on the SD fields, and spans a period of 1989 – 2010 (22 of 53 years considered in this study) – twice as short as the ERA-40 dataset.

X. 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.

Done

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

XII. 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.

Figures have been changed accordingly

Charbit S., C. Dumas, M. Kageyama, D.M. roche and C; Ritz, Influence of ablationrelated processes in the build-up of simulated Northern hemisphere ice sheets during the last glacial cycle, The Cryopshere, 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 meltwater retention in large-scale ice-sheet models of Greenland, journal of Glaciology, 55, No. 193, 869-878, 2009b

Response to the Reviewer 2 (N.-J. Schlegel)

We are grateful to Nicole Schlegel for her positive and constructive review complemented by nice advice on how to deal with the points raised by the reviewer 1 and Andy Aschwanden. We have attempted to bring the novelty of the approach into focus and to improve the description of our methods.

The following is a review of “Modeling surface response of the Greenland Ice Sheet to interglacial climate” By D. Rau and I. Rogozhina.
This manuscript is a brief communication describing a new surface mass balance parameterization, implemented within the SICOPOLIS Greenland, based on daily standard deviation of temperature. Standard deviations vary spatially over the ice sheet, and the characterization of this variation changes throughout the yearly cycle. The authors implement the new parameterization and compare results with regional-climate model-derived SMB and with other parameterizations available within SICOPOLIS. The authors show that when taking daily standard deviations of temperature into account, their estimates of SMB are greatly improved, especially spatially. The new parameterization yields total estimates of SMB from 1958-2001 that agree regionally with the RACMO2 SMB product. The investigating of SMB parameterization is an important piece of the current advancements in ice sheet modeling, and the authors present interesting results that highlight the need for methods better than the traditional PDD. However, there are a number of things that remain unclear about the methods used in this study, and the authors should address them before being recommended for publication in TC.

I. The biggest problem of this manuscript is the vagueness of its subject. For instance, this paper is about an exciting, new parameterization of SMB for Greenland using SD, and the title should reflect this. Similarly in the methods, the “Modeling Approach” section should be titled something that indicates that it is the key section that describes the new parameterization being introduced. Perhaps there should be a short section on the ice sheet model and then a section on the SMB parameterization. There should be a clear indication of which section is the one explaining the new method being introduced.

We have changed the title of the manuscript to “Vital role of daily temperature variability in surface mass balance parameterizations of the Greenland Ice Sheet”.

Below, I offer some more specific comments:

II. Page 2707: Line 1-9: This section should make it clear how the new approach was derived and why the assumptions (if any) are justified. I think what the authors are saying here, is that the SD is usually assumed constant over the ice sheet, but their method actually varies the SD with values derived from their analysis of ERA-40 temperatures. But the fact that I am not sure suggests that there should be clearer step-by-step explanation.
For instance, in section 3.2, line 25-27, the authors should be able to reference the section that describe the methods for the new approach. These three lines in section 3.2 are not sufficient enough explanation.

The manuscript has been restructured following the above suggestions and the new parameterization has been described in a greater detail.

III. Page 2707-2708 Section 2.2: There has been much discussion about the simulation setup and what biases might exist because of the spinup. In all honesty, this paper is not about what the ice sheet model does at all. It is about comparing the different SMB parameterizations. In my opinion, the authors could solve this by holding the surface constant throughout the transient simulation. This is the only way to ensure that the SMB models are all being compared equally. Since the models are eventually evaluated against RACMO, it makes sense to just keep the ice sheet surface at the same elevation, since that is what the RACMO simulations do. RACMO and ECMWF both assume a fixed ice sheet surface, so allowing the surface to evolve only complicates the comparison between these products and all of the ice sheet model's SMB parameterizations. Perhaps comparing how the ice sheet surface evolves (dynamically?) due to the different forcings is another paper. But, for now, it is important to establish the differences in SMB only, in order to introduce the new method and establish its validity.

We have followed this suggestion and rerun all experiments with the ice surface kept at its observed elevation. Thank you for this comment! Some details on this topic are also provided in our response to the short comment of Andy Aschwanden.

IV. Page 2708 Line 9: Section 3.2 does not give details about the parameterization, though it does describe the spatially varying SD. There should be more details about the varying SD method in the methods section, so that it can be referenced in instances like this.

Done

V. Page 2709 Line 25: As mentioned above, the description of the SMB parameterization based on SD should be first introduced in the methods, not in the results or discussion, and more detail is needed. The results of the analysis of SD, however, should be presented and then discussed here.

Done

VI. Page 2711 Section 3.3: This section is very confusing. RACMO is a regional climate model that provided SMB and its components, not ice flow or discharge. What is the product providing discharge? InSAR? Please make sure to properly cite the remote sensing and satellite data being used. It is not enough to cite Sasgen et al. 2012 because they use this data for analysis, the authors must make sure to give credit to the source of the data.

Please, see our response to the comment 3 of the reviewer 1

VII. Page 2722: Fig. 4: A time series of the might be interesting to include as well, instead of just a comparison over the reference period. Regional climate model simulations of SMB suggest that there is significant interannual variability from 1958-2001, so it would be interesting to see how the parameterizations compare (E.g. Is the skill of the new method better in large accumulation/melt years? Or average years?). Also, it is not clear why this time period is considered as the reference period and why it is justifiable to assess the skill of the new method with averages over this period.

We have added comparison of modeled time series derived from different parameterizations and RACMO2/GR. The resulting total SMB time series from our new parameterization and the parameterization of Fausto et al. [2009] compare well with the results of RACMO2/GR. Our choice of the reference period is discussed in the response to the reviewer 1's comment IX.

Response to the Reviewer 3

We would like to thank the anonymous reviewer 3 and acknowledge that his comments helped us to improve the manuscript considerably, especially by showing that the use of realistic SD distribution leads to reduced uncertainties in modeled surface mass balance associated with the choice of degree day factors. This enabled the conclusion that the realistic SD distribution is key to modeling surface responses of continental-scale ice sheets using PDD models.

Rau and Rogozhina modify a classical positive degree day (PDD) surface mass balance (SMB) modelling approach in order to improve simulated SMB for the ERA period (1958-2009 AD). By using a spatially variable temperature standard deviation (SD) in the PDD formulation, the match to results from a regional climate model with a physically based energy balance approach is improved compared to simulations with constant SD values.

I. My main concern with the manuscript is the limited scientific significance of the presented work. The important value of the PDD approach still is (after the advent of regional climate models like RACMO and MAR) its simplicity and applicability for longterm paleo studies. Selecting spatially variable SD values for the present day climatic and geometric configuration is equivalent to tuning the model for the present day and thus limiting it severely. Furthermore, I am not convinced the improvement compared to other PDD approaches is as clear as it appears in the manuscript.

We suppose that the significance of scientific work should not be evaluated according to the number of ready solutions delivered - potential impact, which a particular study may have on subsequent studies, is sometimes of greater importance. Here we only show that spatial and seasonal variability in daily temperature is crucial to the results of modeling experiments applied to large-scale ice covers and present one method for deriving this parameter for Greenland/Antarctica and other regions where in-situ observations are limited to a few locations. The study of Fausto et al. [2009] presented a parameterization with spatially variable SD based on measurements from 27 stations across Greenland over the period of 10 years at maximum. We suggest using datasets that cover the entire region and significantly longer period and validate this method in application to recent history of the GIS – the modeling exercise that can be compared with the results of other models and existing observations.

Mayor points:

II. Introducing spatially variable SD values is ultimately equivalent to tuning the model to the present day climatic and geometric configuration. Since information of variable SD is limited to the recent past, the application of the model is reduced to that time period as well. The given SMB comparison strictly applies for the period 1958-2009. There is no reason to believe that the derived spatially variable SD values should apply in any other geometric or climatic configuration of the ice sheet and surrounding. The reference to "interglacial climate" in the title and other parts in the MS is therefore misleading and should be removed.

We have removed “interglacial climate” from the title. On the contrary, we believe that our method for estimating the SD parameter can potentially apply to glacial climate as well, by obtaining improved empirical relations between SD, surface elevation and other important factors.

III. It is not clear to me why the authors do not at least mention the use of spatially dependent PDD factors (PDDFs). Arguably, PDDFs are the most important tuning factors of the PDD approach.

Similar to SD there is no reason to assume PDDFs to be spatially or temporally constant. I think the authors should address how simulated SMB values would look like if spatially variable PDDFs and SD would be used together to make the analysis more complete. This would also limit the possibility of a coincidental good match with RACMO using variable SD values.

In the updated version of our manuscript, we discuss DDFs in a great detail. It appears that the use of realistic (low) SD values leads to low sensitivity of the modeled regional SMB to DDFs (Figure 4R). As such, it solves the problem of significant uncertainties in DDFs.
In Figure 4R, we present two series of simulations, all with the retention model of Janssens and Huybrechts [2000] and the specific heat capacity formulation of Huybrechts [2002]. In Figure 4Rb, we depict modeled values of regional and total SMB from simulations with the uniform SD of 5°C and DDFs of Greve [2005], Huybrechts [2002] and Tarasov and Peltier [2002]. For comparison, we provide analogous simulations with spatially and seasonally variable SD (12 monthly fields of SD derived from the ERA-40 dataset, 1958 – 2001, that are now adopted in our study instead of previously used summer SDs) in Figure 4Ra.

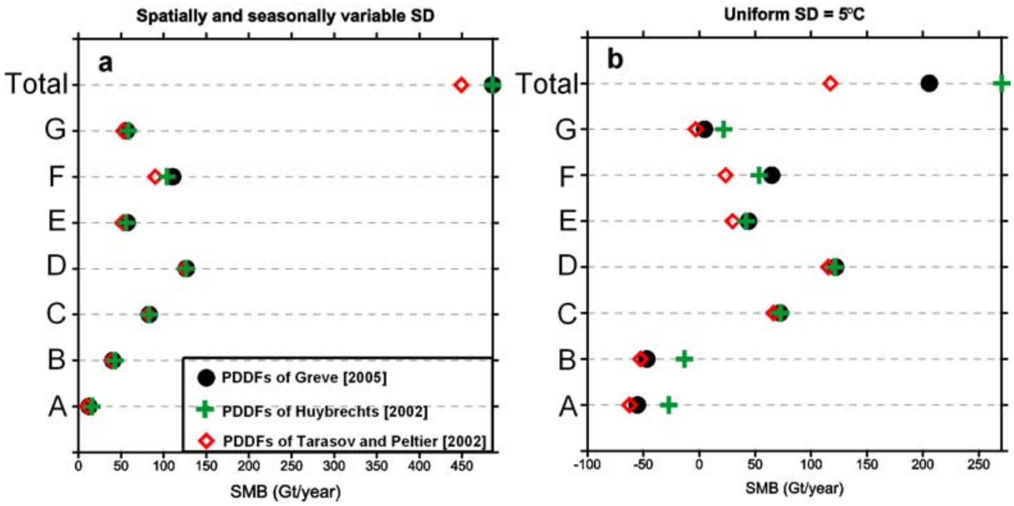

Figure 4R. Modeled regional (drainage basins as in Figure 1) and total SMB of the GIS from simulations with spatially and seasonally variable SD (a) and uniform SD = 5°C (b) and three sets of DDFs (3 mm/(°Cd) for snow and 8 mm/(°Cd) for ice as in Huybrechts [2002], 2.65/4.3 mm/(°Cd) for 'cold/warm' snow and 17.22/8.3 mm/(°Cd) for 'cold/warm' ice as in Tarasov and Peltier [2002], 3 mm/(°Cd) for snow and 15/7 mm/(°Cd) for 'cold/warm' ice as in Greve [2005]).

IV. For the evaluation of the SMB model discussed in the MS all aspects of ice dynamics, geothermal heat flux, rheology etc. are completely irrelevant.

Done

V. So is the question on how to initialize the ice sheet model. For the comparison with RACMO or any other SMB model, the PDD models should be run on the same fixed present day ice sheet geometry (and same mask; see comment below). Any details about the ice sheet model aside from the SMB model proper should be removed from the MS. An important conclusion of Vernon et al. (2013) is that using the same ice sheet mask is crucial for quantitative comparison between different model studies of SMB. This aspect has not been explicitly discussed in the presented MS, which leads me to believe that identical masking has not been guaranteed in the comparisons. The authors should make sure masking issues are addressed and solved for the presented comparisons.

Done. Please, see our response to the comment of Andy Aschwanden for detail.

VI. The new PDD approach is presented to better reproduce observations when in fact it is better matching another model, in this case RACMO.

We have added comparison of the regional SMB rates derived from satellite observations with those from parameterizations with uniform SD to the former figure 5. For example the use of the parameterization of Tarasov and Peltier [2002] results in total SMB trends of –238 Gt/year versus 142 Gt/year estimated from satellite observations (after removing the contribution of ice discharge from the InSAR data).

VII. I have strong doubts whether the experimental setup (Fig 4) allows for a "fair" comparison between the different PDD models. In other words, it is likely the improvement of including variable SD is over-estimated. Taking the given models (and most importantly their specific parameter settings) out of context of their paleo application and applying them for the present day may not be the best way of comparing them. For example, the model parameters may have been originally chosen as compromise to optimise both present day and LGM constraints and thus be sub-optimal for the present application.
One could e.g. envision a retune to match present day total SMB and then compare the spatial patterns.

Here we can provide a couple of examples of the use of similar settings for the present-day/future simulations (e.g., Aschwanden et al. [2013], Greve et al. [2011]) but there are hundreds of such examples. We would like to avoid any tuning in our study because this often results in unrealistic model parameters.

Minor points:
In many cases the order of references in brackets seems arbitrary. To my knowledge older citations should precede the newer ones unless there is a good reason for another choice.

Corrected

I find the distinction between SEB and SMB models in 2705.20ff confusing since in my understanding both PDD based and SEB models are SMB models. This should be clarified.

Rewritten

In my understanding a PDD model (2706.18) "parameterizes surface melt rates of snow and ice" not only "as a function of the number of days a year when mean daily air temperatures rise above 0 C", but also *by how much* temperatures rise above 0 C. This should be clarified.

Rewritten

Authors should describe how the conversion to snowfall and rain (2707.8ff) is actually done.

Done

References:

Aschwanden, A., G. Aðalgeirsdóttir, and C. Khroulev (2013). Hindcasting to measure ice sheet model sensitivity to initial states. The Cryopshere, 7, 1083–1093. doi:10.5194/tc-7-1083-2013.

Greve, R., Saito, F., and Abe-Ouchi, A.: Initial results of the SeaRISE numerical experiments with the models SICOPOLIS and IcIES for the Greenland Ice Sheet, Ann. Glaciol., 52, 1–12, 2011.