Response to both reviewers

We thank both reviewers for their mostly positive and very constructive reviews. Many of their questions raised very interesting points which motivated new analyses and opened new perspectives. We however did not include any new figures in the revised paper as the paper is already quite long, but we are happy to include these in the supplement. We plan to revise our manuscript according to the reviewers' detailed comments as outlined below. We also corrected all typos. Proposed modifications of the manuscript are given in italics.

1 Response to Reviewer 2

The authors propose a new SMB model to quickly simulate SMB of the Greenland Ice Sheet for a long time (hundreds to millennium). The manuscript is well written, many tables and figures are of good quality. I appreciate the careful preparation of the manuscript. The model performance of dEBM compares favorably with that of the regional climate model. This study will bring new knowledge on the past reconstruction and future projection of SMB of the Greenland Ice Sheet and therefore fall within the scope of The Cryosphere. However, I would like to suggest authors do some modifica- tions before acceptance for publication. Major and specific comments are as below. I hope that my comment is very useful for the improvement of the manuscript.

Major comment:

JJA albedo simulated with dEBM was significant greater in south-western Greenland than that simulated with MAR (Figure 10). This is the reason that dEBM does not consider the effect of dark region (Wientjes et al., 2011) on SMB, which frequently appears on south-western Greenland during summer I guess. Previous studies suggest that the dark region significantly affects the SMB of the GrIS (e.g. Cook et al., 2020). The effect cannot be ignored to evaluate the SMB of the GrIS. dEBM uses the same albedo values (0.55) for ice and wet snow, but it's not realistic to assume an ice albedo of 0.55 in the coastal region. Fig. 13 showed negative SMB simulated with dEBM appeared in the late 21st century, whereas it showed SMB simulated with MAR appeared in the early 21st century. I guess this is due to the overestimation of SMB in the ablation area of the GrIS in the case of dEBM. Because the generation of the dark region is related to microbial activity, the incorporation of the albedo reduction caused by the dark region into dEBM may be still difficult. However, at least, authors should more discuss a factor affecting JJA albedo in Greenland. In addition to that, I suggest authors to more describe future challenges to improve dEBM.
You are actually raising two very important points here: (i) indeed I don't see how we could explicitly include processes such as microbial activity or dust deposition on the ice sheet in such a simple model. However, also with respect to the important role of dust in the course of the termination of the last ice age, we actually currently consider to prescribe a background bare ice albedo wherever the multi-year SMB is negative and the monthly snow height vanishes. In case of present-day Greenland one could then also prescribe observed melt season albedo in the ablation zone. We added this thought to section 6: *Furthermore one might prescribe a background bare ice albedo to account for regional darkening due to dust deposition or microbial activity (Wientjes et al., 2011; Cook et al., 2020).*
(ii) The fact that we do not see a negative SMB in the first half of this century points to a central problem in global climate modeling which cannot be attributed to or solved by a surface mass balance model. We address this at the end of section 5.2: *"The climate model however does not reproduce the extreme Greenland blocking in the 2005–2015 period, which is a common problem in global climate models (Hanna et al., 2018). Accordingly the interannual variations in SMB of recent decades is underestimated and the simulated negative trend in SMB may be delayed."*
Specific comments:
- 1 Introduction
P. 2 Line 4: Replace "cemtury" with "century".
OK
P. 3 Line 4-5: I suggest adding NHM-SMAP (Niwano et al., 2018), which is a 5km resolution regional climate model, to the list of regional climate models to evaluate SMB of the GrIS.

OK

- 2 Model Description

P. 6 Line 14-15: Please more explain why does this study neglect the effect of sublimation, evaporation, and hoar on SMB of the GrIS. Also, to calculate these properties by dEBM, what atmospheric forcing does dEBM require?

We have here added the following sentences: *"Other contributions to the SMB such as sublimation, evaporation, and hoar are so far neglected by the dEBM as it is not expected that our downscaling approach can improve the respective mass fluxes from climate models, which simulate these processes on larger spatial scales but shorter time steps. With minor technical modifications, these fluxes can be individually added to snow fall SF and rain fall RF as an additional forcing (negative snow fall does not pose a problem)."* We will test this strategy in upcoming studies.

P. 7 Line 5: Why is the albedo differentiated between fair and cloudy sky conditions? In my understanding, albedo is used as a constant value for each surface type in dEBM. Please explain clearly more.

This was not very clear and inconsistent at several places. We have made several modifications: (i) we introduce albedo in 2.3 as *"albedo ($A(SurfaceType)$ which is chosen according to the given surface types (i.e., NewSnow, DrySnow, or WetSnow) and further differentiate these for cloudy and fair conditions following Willeit and Ganopolski (2018)"* (ii) To section 2.6 we have added *"Each surface type is assigned a pair of albedo values for fair and cloudy conditions. Following Willeit and Ganopolski (2018) we assume that the albedo for cloudy conditions exceeds by 0.05 the respective albedo for fair conditions of the same surface type."* and (iii) in section 3.1 it is now *"For fair conditions we vary $A_{NewSnow}$ within $[0.84, 0.845, 0.85]$, $A_{DrySnow}$ within $[0.68, 0.69, 0.70, ..., 0.78]$, $A_{WetSnow}$ within $[0.53, 0.54, 0.55, 0.56, 0.57]$, the albedo values for cloudy conditions are varied with accordingly larger base values, and R varies within $[-2, -1, 0, 1, 2]\mathrm{W\,m^{-2}}$."*

P. 8 Line 5: Could you show me a map of Hice and Hint? Also, how did you get such elevation information? Because spatial interpolation is an important part of this study, the authors should describe the elevation data clearly.

We now specify the resolution of the atmospheric forcing in the manuscript in section 5 *"Both simulations have been conducted with the AWI Earth System Model, AWI-ESM (Sidorenko et al., 2015) at an atmospheric resolution of approximately 1.85X1.85 degree horizontal resolution with 47 vertical levels (T63L47) and both experiments are using an invariant present day ice sheet geometry as boundary conditions."*, also please see Fig. AC2-1

P. 8 Line 16: P. 8 Line 16: Please replace ". respectively." with ", respectively

OK

P. 8 Line 20: Is CC in eq. (10) interpolated? If not, please describe the reason not to interpolate CC. If interpolated, please describe the method. LW seems highly dependent on CC according to eq. (10).

CC is interpolated (section 2.1 first paragraph)

P. 9 Line 1-2: How did you classify sky conditions (cloudy and fair) in the other season such as MAM (March, April and May)?

We did not analyse the other seasons here because we wanted to find parameters which represent the main melt season.

P. 9 Line 7: Isn't "CC > 0.9"?

Thank you, we corrected this.

P. 11 Sub-section 2.7: Can dEBM output the volume of the transformed ice? I think that such spatio-temporal information would be useful to evaluate SMB from the past to the future.

The model does not simulate snow or ice density. The output of the model would only allow to diagnose the mass of the transformed ice.

P. 11 Sub-section 2.7: Replace "m" with "mth" because "mth" is used in eq. (4).

We replaced mth with m instead.

P. 11 Sub-section 2.7: Add "(15)" to the later equation.

OK.

- 3 Parameter selection and evaluation based on observations

P. 12 Line 6: Replace "(Fettweis et al., 2020)" with "Fettweis et al. (2020)".

OK.

P. 12 Line 6-8: It's better to add information on original spatial resolution (before interpolation).

We did so.

P. 12 Line 9-10: Modify italics

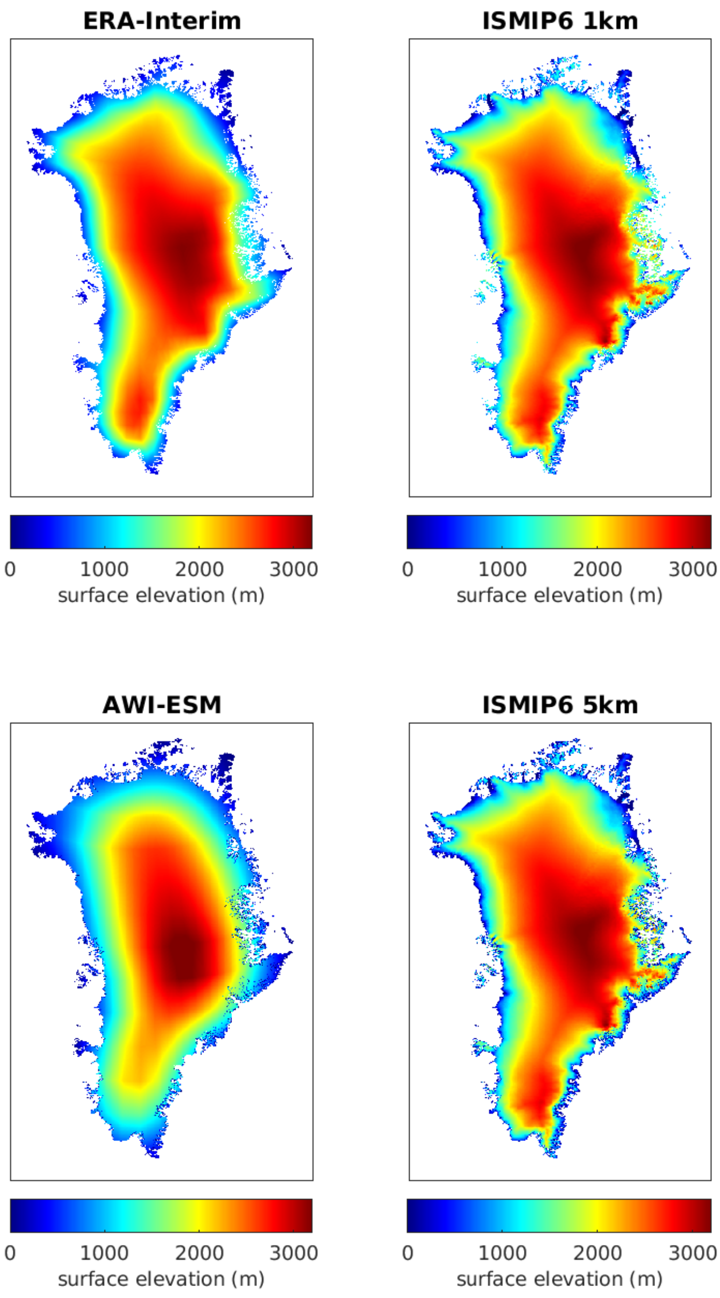

Figure AC2-1. Orographies as used in the manuscript: ERA-Interim linearly interpolated to the 1km ISMIP6 coordinates (upper left), ISMIP6 topography (Nowicki et al., 2016) (upper right), AWI-ESM linearly interpolated to an equidistant 5km grid (lower left) and ISMIP6 topography linearly interpolated to the same 5km grid as used for AWI-ESM.

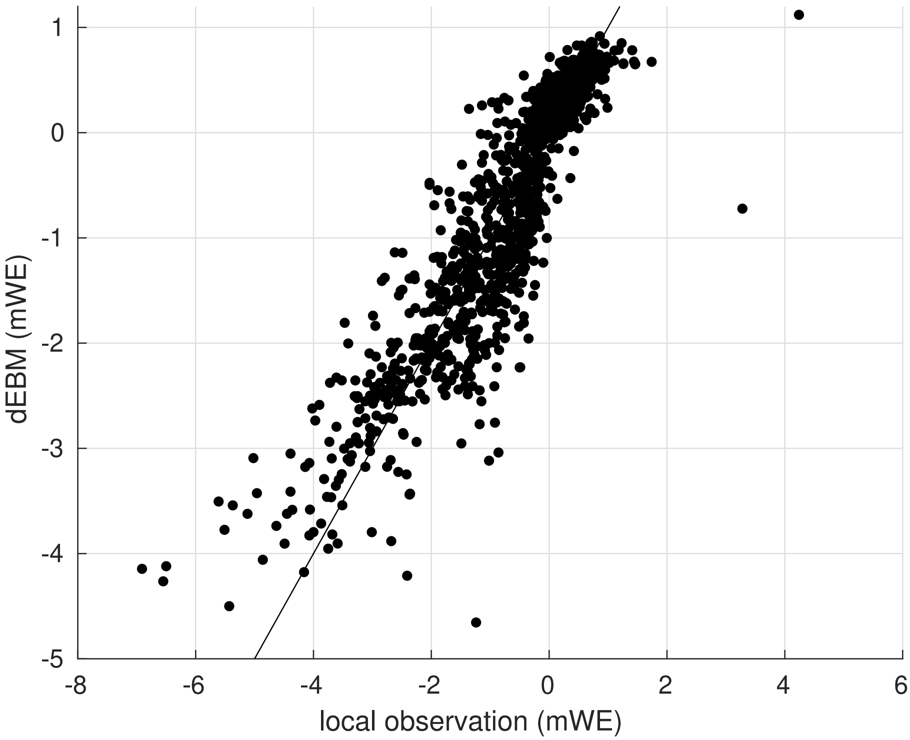

Figure AC2-2. Local SMB as simulated by experiment $dEBM_{MAR,ERA}$ as a function of the SMB observations.

OK.

P. 13-14 Figures 4 and 5: Could you show me the relationship between the simulated mWE (Gt/yr) and observed mWE (Gt/yr)? I did not understand the messages of Figures 4 and 5 due to much information. Authors should show a model bias for local and GRACE observation, respectively, first.

Figures 4 and 5 illustrate statistics of all calibration experiments (830 experiments). The purpose of these figures is to demonstrate that (i) the systematic bias in the two observational data sets appears to be small (p.13, l.5), (ii) good matches in variability and mean are mutually exclusive and we provide some justification why we chose parameters which provide a good match with the variability. (iii) that using higher resolution precipitation substantially improves the match to the mean and to the variability of the local observations which supports our hypothesis that the precipitation forcing is systematically biased.

Fig. AC2-2 illustrates the relationship between simulated and observed local SMB measurement for the experiment $dEBM_{MAR,ERA}$ which was used in section 4.2., Fig. 6 in the manuscript shows the comparison between experiment $dEBM_{MAR,ERA}$ (red), integral observations (black) and MAR(blue).

P. 15 Experimental design: Authors mentioned that dEBM showed good performance in the simulated SMB using atmospheric forcing derived from MAR simulation. In my understanding, dEBM has an advantage of computational time for the SMB simulation compared with MAR. However, Isn't the calculation time of dEBMMAR, ERA more than that of MARERA? If so, is there any advantage to using dEBMMAR, ERA? MARERA has already shown reasonable performance in SMB in my sense. Please describe this section more carefully and emphasize the advantage of dEBM compared with MAR.

The sole purpose here was to evaluate the model independent of the suspected bias in precipitation forcing. Regional models can be expected to be superior to dEBM in all respects (apart from their computational cost) and experiment $dEBM_{MAR,ERA}$ does not represent a typical use case; typically one would use coarse resolution climate model output as in section 5.

P. 21 Line 16: Please describe the original spatial resolution of AWI-ESM forcing. Also, how did you get the forcing dataset?

Please describe the information on the dataset clearly.

We now specify that the resolution of the atmospheric component has a*"horizontal resolution of approximately 1.85X1.85 degree with 47 vertical levels (T63L47)."* With respect to their forcing, the global climate simulations for Mid-Holocene and Preindustrial only differ in orbital parameters and greenhouse gas concentrations, following PMIP protocols (Otto-Bliesner et al., 2017) and CMIP5 protocols (Taylor et al., 2012) respectively.

P. 23 Figure. 12: Ice sheet area gradually would change from past (Mid Holocene) to future (2099) I think. Could dEBM simulate the ice sheet area in Greenland? The ice sheet is being retreated under climate warming, so the ice sheet dynamics would significantly affect the SMB of the GrIS. I suggest adding a brief discussion about inter- annual changes in the ice sheet area.

The simulation of changes in the ice sheet area would require to couple dEBM to an ice sheet model. This is of cause an important next step but not the focus of this study. We indicate this in the last part of section 6: *"dEBM can be ... coupled to an ice sheet model using forcing derived from climate models and observation as in Niu et al. (2019)."*

P. 26 Line 5-7: I'm curious about the computational time of dEBM. Authors should describe the specific time in the manuscript. For example, how long did H6K and Industrial simulation take, respectively?

We have added the following lines: *"In its Fortran version the computational cost of the actual dEBM is similar to the cost of the necessary interpolations with existing interpolation weights. It takes about 5 seconds to compute the SMB of one year for a configuration with 360000 gridpoints on a CRAY CS400. A matlab version of the model simulates the 1979-2016 SMB of the GrIS at 1km resolution (approximately 4.8 million grid points) in approximately 30 minutes on a Linux desktop PC. Requiring only monthly forcing also provides for an uncomplicated interface, as monthly forcing usually is more accessible in case of completed transient climate simulations such as simulations of the CMIP5 project (Taylor et al., 2012)."*

P. 26 Line 8-9: As I mentioned in the major comment, further study is necessary to accurately evaluate SMB in GrIS, especially the south-western region. Please describe future challenges briefly.

We added : *"Furthermore one might prescribe a background bare ice albedo to account for regional darkening due to dust deposition or microbial activity (Wientjes et al., 2011; Cook et al., 2020)."*

Table A1: Please add CC as forcing into the table.

OK.

P. 33 Line 9-15: The paper has been published on TC. Please replace.

OK.

References

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