The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-218-RC1, 2018
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Interactive comment on "A surface energy and mass balance model for simulations over multiple glacial cycles" *by* Andreas Born et al.

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Received and published: 10 December 2018

1 General comments

This manuscript presents the BErgen Snow SImulator (BESSI), a surface energy and mass balance model, which is designed to facilitate coupled ice sheet-climate simulations on multi-millennial time scales. In this context BESSI may serve as an interface between a coarse resolution and uncomprohensive forcing typically stemming from climate models of intermediate complexity and three-dimensional ice sheet models which require a detailed and accurate forcing consisting of mass and energy fluxes.
Other common surface mass balance (SMB) models aiming at paleo-climate questions only consider a single snow layer (Krapp et al., 2017; Robinson et al., 2010) or

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neglect processes in the snow pack and instead parameterize melting and refreezing empirically (Reeh, 1989). BESSI particularly sets itself apart from these schemes by representing snow and firn in 15 layers. Besides accumulation, the scheme uses insolation and near surface temperatures as a forcing and calculates the surface energy balance distinguishing the albedo of ice and dry and wet snow. Melting is deduced from the energy balance of the surface layer, while refreezing of liquid water is considered in all layers of the snow column. Furthermore, the heat diffusion equation and a firnification scheme yield temperature, snow mass, snow density and water content as prognostic variables in each model layer.
Born et al. provide a detailed model description, propose calibrations based on different data sets, and present a first application by investigating the sensitivity of Greenland's SMB to perturbed temperature and precipitation input.
The paper is generally well written, provides a good insight into snow pack modelling for a wider community, and the sophisticated snow pack representation is clearly a valuable contribution for the ice sheet modelling community. However, the paper also has some shortcomings which, in my view, would require major revisions.

2 Major comments

Abstract:
In its first half (lines 1-8) the abstract puts too much emphasis on motivation and background, while the second half is too short and unspecific (what is the calibration base, how was the model set-up evaluated, what is the time scale of the sensitivity experiments, what is the outcome of the sensitivity experiments...)

The benefits of a sophisticated representation of the snow pack could have been carved out better.

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This aspect is missing in the introduction (e.g. are there biases and limitations in other schemes, which can be related to insufficient representation of processes in the snow column?). For the same reason, the results and discussion could particularly focus on snow covered regions and those processes which are important for coupled Earth system models.

The model does not resolve the diurnal freeze-melt cycles.
In my view, this is a major weakness in this scheme, which should be discussed. In Krebs-Kanzow et al. (2018) (Fig. 4) we demonstrate, that the length of the daily melt period influences surface melt rates. If the shape of the diurnal freeze-melt cycle is included in a SMB scheme, the same forcing (i.e. short wave energy uptake and air temperature) will result in different melt rates for different latitudes and seasons. Likewise nocturnal refreezing depends on the diurnal cycle. If the water holding capacity of the snow layer is sufficient to store the melt water produced during day-time, the net melt water production (or runoff) of the whole day will correspond to the melt rate predicted by a scheme, which uses only daily means. In any other case with distinct nocturnal refreezing, however, I would expect that such a daily scheme would underestimate the runoff, particularly over bare ice. I think it would be helpful to show the spatial distribution of the SMB of the ERA-Interim period, ideally in comparison to a regional model with sub-daily timestep, such as MAR or RACMO. Also the seasonal evolution could be of interest.

While the scheme is carefully calibrated, the evaluation is too short, in my view. As mentioned above, I think it would be interesting to assess the model's ability to reproduce spatial and seasonal patterns, maybe focussing on the accumulation area. Additionally, is it possible to specifically compare the regions outside of Greenland to observations in greater detail (e.g. onset and end of melt period, representation of large glaciers)?

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3 Specific comments

Page 1 lines 13-14: Please provide a rough estimate of the amount of water stored in polar ice sheets. Also the reference in this sentence is wrong: it should be the locking of water which lowers the sea level, not the ice sheets.

Page 1 lines 19-21: Please specify: high-frequency variability is interannual here?

Page 2 lines 1-2: I assume that the acceleration of mass loss is related to positive feedbacks, so you might change the order of the first 2 sentences of this page and drop the “moreover”.

Page 2 lines 3-32: I would propose a slightly changed structure:
Page 2 lines 11-15: This paragraph could move to the end of this part while
Page 2 lines 16-28: could be positioned earlier, maybe around line 5.

Page 2 lines 7-10: I think, primarily the problem is, that these models have a too low spatial resolution to resolve the narrow ablation zone. Most SMB schemes actually simplify physics (or even replace physical parameterizations by empirical functions) for the benefit of a better spatial resolution. This typical approach should be highlighted here, since BESSI does provide better physics in terms of the snow pack and uses physical meaningful atmospheric parameterizations (I would expect that even EMICs will include a similar degree of complexity in the atmosphere, though).

Page 2 line 33: This sentence is hard to understand and phase transitions as part of the energy balance should be mentioned. Maybe: The energy balance of the snow column is calculated by considering the energy fluxes through the surface and diffusive heat fluxes to deeper layers, the latent heat of melting in the surface layer and refreezing in all layers.

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Page 3 line 7: What is the reason to choose 15 layers?

Page 3 line 9: To me it is not clear, how this follows from the previous sentences; maybe it is better without “thus”.

Page 8 line 15: Please replace surface temperature by near surface temperature

Page 12 line 15: The formulation “observed” could be misleading- maybe use something like “simulated” or “effective”

Fig. 4: Does the scheme only transfer the mass balance to the ice model or also heat flux/temperature?

Fig. 5 and 6: The model seems to be conserving mass and energy almost perfectly and I wonder if these figures could be reduced to fewer seasonal cycles, or even be replaced by some statistics, while the figures could be moved to the supplement.

Page 13 Model calibration: The choice of calibration data sets should be motivated. I guess that the calibration is deliberately limited to data which are direct and relatively precise measurements (with the exception of the surface mass balance time series deduced from GRACE). However, the ablation zone is not well represented in the calibration and consequently, an evaluation of the spatial pattern of the SMB is important (see major comments).

Page 17 lines 1-6 and Table 4: Here, the clarity could be improved. Maybe the parameter combination over the ten simulations with lowest RMSE could appear as $TOP10_x$ and together with $BEST_x$, could be introduced in the text before line 4.

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Page 17 lines 14-17: I don't understand, why higher wind speeds might reconcile relatively low optimal parameters of D_{SH}. Also, at least over melting ice, wind speeds are rather reduced. And finally, the last sentence of this paragraph is not very clear to me.

Page 18 line 17-19: I don't find this analysis very convincing. I assume that surface temperatures are closely related to air temperature and even a PDD scheme would predict melt, if forced with daily temperatures $> -5^oC$.

Fig. 11a: What exactly is runoff? It does not seem to be runoff=rain+melting-refreezing.
Generally, I don't seem to interpret Fig. 11 correctly. I don't see a good agreement with van den Broeke et al. (2016), who estimate SMB $\approx$ 300-400 Gt, accumulation is $\approx$ 600 Gt and refreezing is $\approx$ 200 GT.

Page 20 line 2: Indeed, considering short wave radiation anomalies might be interesting. Is it possible to discuss this option?

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