

We would like to very much thank the reviewer Alexander Robinson for reviewing our study and his constructive comments which helped to significantly improve our manuscript. Please find below the reviewer's comments in black font and the author's response in blue font.

Responses to Alexander Robinson (Referee # 1)

This study is focused on understanding the challenges and sources of uncertainty of simulating the surface mass balance (SMB) of the Eemian interglacial period. Steady-state time slice simulations are performed for the Eemian and the present day, with global and regionally downscaled climatic forcing applied to several combinations of SMB models. The manuscript does a good job of describing many aspects of Eemian smb modeling that are often overlooked (seasonal changes in climate, sea-ice extent, lapse rate validation). The review of past Eemian sea-level contribution estimates is also well done, even if it is only part of the motivation for the current work rather than the main focus. I think that the paper should be published after minor revisions, explained below.

We thank you for your overall positive evaluation of our study and hope that we address your comments in the following paragraphs to your satisfaction.

This is an excellent time-slice study with a good experimental design and thorough analysis. However, it is missing any insight into the role of feedbacks in transient coupled experiments where the ice-sheet topography could evolve. This could arguably be as important as the inherent bias that a particular smb model imposes, or even more so – see Robinson and Goelzer (2014), for example. I suggest adding some discussion of this point (note that this is a different point than that of the last paragraph on Page 18, and the first paragraph of Page 19 is more focused on whether a given time slice is realistic).

We agree with you and acknowledge that we failed to discuss this very important issue. The following paragraph was added to the discussion section:

“Furthermore, Ridley et al. (2005) find an additional surface warming in Greenland in transient coupled 4xCO₂ ice sheet-GCM simulations compared to uncoupled simulations caused by an albedo-temperature feedback. Similarly, Robinson and Goelzer (2014) show that 30% of the additional insolation-induced Eemian melt is caused by the albedo-melt feedback. Somewhat unexpectedly, given the higher temperatures, Ridley et al. (2005) find more melting in stand-alone ice sheet simulations than in the coupled simulations. The local climate change in the coupled runs results in a negative feedback that likely causes reduced melting and enhanced precipitation. They propose the formation of a convection cell over the newly ice-free margins in summer which causes air to rise at the margins and descent over the high-elevation ice sheet (too cold for increased ablation). This leads to stronger katabatic winds which cool the lower regions and prevent warm air from penetrating towards the ice sheet. An increased strength of katabatic winds can also be caused by steeper ice sheet slopes (Gallée and Pettré, 1998; Le clec'h et al., 2017).”

added references:

Gallée, H. and Pettré, P.: Dynamical Constraints on Katabatic Wind Cessation in Adélie Land, Antarctica, Journal of the Atmospheric Sciences, 55, 1755–1770, [https://doi.org/10.1175/1520-0469\(1998\)055<1755:DCOKWC>2.0.CO;2](https://doi.org/10.1175/1520-0469(1998)055<1755:DCOKWC>2.0.CO;2), 1998.

Ridley, J. K., Huybrechts, P., Gregory, J. M., and Lowe, J. A.: Elimination of the Greenland Ice Sheet in a High CO₂ Climate, *Journal of Climate*, 18, 3409–3427, <https://doi.org/10.1175/JCLI3482.1>, 2005.

Robinson, A. and Goelzer, H.: The importance of insolation changes for paleo ice sheet modeling, *The Cryosphere*, 8, 1419–1428, <http://doi.org/10.5194/tc-8-1419-2014>, 2014.

While I found the analysis very thorough, it was difficult to agree with the overall conclusions reached by the authors. For example, I disagree with this sentence from the abstract: “We suggest that future Eemian climate model inter-comparison studies are combined with different SMB models to quantify Eemian SMB uncertainty estimates.” To me this is a strange conclusion to make, or perhaps I don’t understand the phrasing clearly. Should we believe PDD is providing added information to an energy balance model? This also comes up in the last paragraph of the Discussion. The authors seem to conclude that all SMB models are needed, because emissivity of the atmosphere is uncertain. This is a strong conclusion, but here nothing was done with emissivity. Further, wouldn’t a more prudent conclusion be that deeply uncertain parameters in complex models should include sensitivity experiments (parameter perturbation) rather than simply reverting to simpler models known to lack important processes?

We agree that our conclusions were not well phrased in this regard. We wanted to make the point that it is important to also have a scheme in place to capture SMB uncertainty. We rephrased in the different parts of the paper as follows:

abstract:

~~“We suggest that future Eemian climate model inter-comparison studies are combined with different SMB models to quantify Eemian SMB uncertainty estimates~~ intercomparison studies should include SMB estimates and a scheme to capture SMB uncertainties.”

discussion section:

~~“Since it is not feasible to perform transient fully-coupled climate-ice sheet model runs with several regional climate models, it is desirable to perform Eemian ice sheet simulations within a model intercomparison covering a range of different (high-resolution) climate forcings and a range of SMB models to capture uncertainties in the best possible way~~ climate forcings (ideally finer than 1° to capture orographic precipitation and narrow ablation zones). However, it is also essential to capture SMB uncertainties in such a model intercomparison. This could for example be realized by employing several SMB models and/or by performing sensitivity experiments of highly uncertain SMB model parameters (e.g., emissivity or melt factors). For the early Eemian it appears to be essential that the used SMB models include shortwave radiation. Furthermore, if lower resolution global climate is used, it might be worth to investigate options for correcting not just the temperature, but also the precipitation/accumulation fields.”

conclusion section:

~~“To improve the Eemian SMB estimate, further effort needs to be put~~ enhanced efforts are needed in developing fully-coupled regional climate-ice sheet models ~~and making them efficient enough to be run over whole glacial-interglacial cycles. We deem Eemian climate model inter-comparisons combining with various SMB models to be the best way to evaluate~~

~~and ultimately lower Eemian SMB uncertainties.~~ glacial timescales (~100 kyr), capturing the evolution of the interglacial as well as the preceding glacial ice sheets and the corresponding surface and topography changes (both are essential for estimating the Eemian sea level rise contribution). These coupled climate model runs could be downscaled at key time steps covering the Eemian period with a regional climate model, providing more accurate SMB estimates. In a next step, intermediate models like BESSI, could be used to provide SMB uncertainty estimates of this best guess SMB via model parameter sensitivity tests. To capture the uncertainty in the simulated global climate from GCMs, it would be an advantage to include dedicated experiments in a climate model intercomparison project.”

Along those lines, I think it would have been quite interesting to see if using different parameter values (for example changing the emissivity of the atmosphere), it would be possible to bracket the MAR-SEB results on both sides with MAR-BESSI (SMB at 130ka showing negative and positive anomalies). That would go a long way towards showing that lower complexity smb models can be useful, but several simulations may be necessary to sample the uncertainty. [This is only a suggestion, not a requirement for publication.]

We thank you for this interesting suggestion and acknowledge that it would be very promising to use BESSI to estimate SMB uncertainties. Furthermore, BESSI could also be forced with transient climate simulations instead of steady-state simulations in the future. However, BESSI is in active development and once the identified shortcomings, i.e., the simple albedo scheme, are improved, BESSI will be a valuable tool to be tested in more paleo applications. We therefore keep your suggestion in mind for future studies.

Generally, the manuscript could use a revision for English usage as well. Some mistakes are highlighted below. Particularly, I noticed the article “the” missing in many instances.

We have reviewed the manuscript again for English usage, and rephrased and simplified many formulations.

== Minor comments =====

Page 1, line 2: Eemian interglacial => Eemian interglacial period

This formulation has been changed throughout the whole manuscript.

Page 1, line 5: “introduces uncertainties” sounds a bit strange, consider rephrasing.

This was changed accordingly.

Page 1, line 10: the calculation of insolation should be straightforward – do you mean shortwave radiation at the surface?

We wanted to say that it is important whether insolation is included in the SMB model or not and we acknowledge that it was not formulated well. It has been rephrased as follows:

“For the relatively warm early Eemian, the differences between SMB models are large which is associated with ~~the representation of insolation~~ whether insolation is included in the respective models.”

Page 1, line 12: simulated climate => simulated climate,

This was changed accordingly.

Page 2, line 1: Past interglacials => Past interglacial periods [Generally this should be changed throughout, as “interglacial” is only an adjective.]

Page 2, line 7: pre-industrial => pre-industrial period

This was changed throughout the manuscript.

Page 4, line 30: surface air temperature => near-surface air temperature [?]

Yes, we mean near-surface air temperature. It was changed accordingly.

Page 4, line 34: “The only process it neglects” <= This is a strong statement, consider rephrasing.

We agree this was formulated too strongly. It was rephrased as follows:

~~“The only process it neglects is~~ However, it neglects sublimation which is of low importance for the mass balance of Greenland.”

Page 5, table 1: Units of PDD factors should be “mm/K/day”

Page 6, line 15: linearly => bilinearly [?]

Page 6, line 7: This 30 years => These 30 years

This was changed accordingly.

Page 8, Fig. 1: Lighter colors in the lower boxes would make this figure easier to read.

The figure was revised with lighter colors.

Page 10, line 13: “with an adapted PDD scheme” <= the ITM equation used by Robinson et al. (2011) and Calov et al. (2015) is not a PDD scheme, it is a “linearized energy-balance” scheme (originally published by Pollard, 1980).

We apologize for this mistake and rephrased as follows:

~~“The exceptions are Robinson et al. (2011) and Calov et al. (2015) who use an intermediate complexity statistical downscaling with an adapted PDD~~ a linearized energy-balance scheme to also include shortwave radiation.”

Page 13, line 35: are we using => we use

This was changed accordingly.

Page 14, line 20: refreeze => refreezing [Change everywhere it appears as a noun.]

“Refreeze” was changed to “refreezing” throughout the manuscript.

Page 15, line 6: “warmer/cooler at 125/130 ka” <= Consider reversing the time order here for consistency with elsewhere.

We agree, reversing the time order here makes more sense and we changed it accordingly.

Page 15, line 7: I think Arctic warming and amplification are not synonymous, consider revising here somewhat for clarity.

We agree that these two phrases are not synonymous and we skipped the phrase amplification.

Page 15, line 13: During early Eemian => During the early Eemian

Page 15, line 18: Sea ice are => Sea ice is
This was changed accordingly.

Page 18, first paragraph: This seems more like Discussion than Results.
You are right, this paragraph was moved to the discussion section.

== References =====

Robinson, A. and Goelzer, H.: The importance of insolation changes for paleo ice sheet modeling, The Cryosphere, 8, 1419-1428, <https://doi.org/10.5194/tc-8-1419-2014>, 2014.

We thank A. Robinson again for the overall positive evaluation of our manuscript and his comments which improved our manuscript significantly!