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Climate of the Past Discussions

Interactive comment on "Eemian Greenland Surface Mass Balance strongly sensitive to SMB model choice" *by* Andreas Plach et al.

Anonymous Referee #2

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General Comments

This paper considers sources of uncertainty in simulating Greenland ice sheet surface mass balance (SMB) during the Eemian interglacial. The authors use a global Earth System model (NorESM), a regional climate model (MAR), and three kinds of SMB model (a positive-degree-day scheme, a model of intermediate complexity, and a full surface-energy-balance model) to assess the sensitivity of Eemian SMB to climate model resolution and SMB model complexity. The authors find that for earlier Eemian time slices (130 and 125 ka, with high summertime Northern Hemisphere insolation), results are sensitive to model choices, with regionally-forced SMB models giving a more negative SMB than globally-forced models, and with the PDD model underestimating melting compared to the more complex models. For later Eemian time

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Discussion paper

slices (120 and 115 ka, with lower insolation), the SMB model is less critical, but SMB remains sensitive to the resolution of the forcing climate model.

The study is well designed, using a novel combination of models to draw useful inferences about SMB sensitivity for the Eemian. The authors give a broad review of earlier work and clearly describe their experimental methods. The analysis is clear and detailed, and the conclusions (with exceptions noted below) are generally well supported by the text and figures.

My main concern is that some of the conclusions are not well supported by the simulation results. I would suggest rewriting or removing some of these statements, as described below. Also, the text would benefit from some editing for English grammar; see Technical Corrections. Otherwise, the authors provide a solid and useful analysis of Eemian SMB sensitivity, and I suggest publication with minor revisions.

Specific Comments

I suggest a modified title. The current title emphasizes the sensitivity of the Eemian SMB to SMB model choice, whereas the text suggests an equally important role for the kind of climate forcing (high-resolution RCM v. lower-resolution GCM).

p. 1, l. 14: "We suggest that future Eemian climate model inter-comparison studies are combined with different SMB models to quantify Eemian SMB uncertainty estimates." Unless I misunderstand how "SMB model" is defined, this statement is not well supported. The text identifies three kinds of SMB model: PDD, intermediate complexity (BESSI), and full surface-energy-balance (as in MAR). The results suggest that PDD schemes are inappropriate for the early Eemian, when insolation differed markedly from present-day. While BESSI results are closer to MAR, I don't see an argument that BESSI results are in any way more accurate or credible than MAR results. I would infer that future studies should use MAR-SEB or a comparable scheme, in order to minimize uncertainties. More generally, one should always use the most realistic, best validated model that is computationally practical, unless it can be shown that running a simpler,

cheaper model yields closely similar results.

There are many other sources of uncertainty for simulated Eemian SMB, notably the absence in this study (as the authors point out) of time-varying topography. It seems more fruitful for future studies to explore other sources of uncertainty rather than revisit simple SMB models.

p. 2 l. 35: “the amplification of summer warming over Greenland has been found to be effective”. I’m not sure what is meant; effective for what?

p. 3, l. 2: Overall, I found Section 2 to be a very clear and helpful description of the models and methods.

p. 4, l. 33: “The only process it neglects. . .” I suggest “It neglects. . .”, since there are bound to be other neglected processes.

p. 6, ll. 24ff: When I read this the first time, I wondered whether the study used the same static surface topography for each time slice. It does, as stated later, but I suggest stating it here.

p. 9, ll. 1ff: I liked the comprehensive description of earlier studies and their limitations. However, this section might fit better into the overall structure if swapped with Section 2.

p. 9, l. 4: A broad range of 0.4 to 5.6 m is given, but the more recent studies have an upper bound of $\sim$3 m. Does this narrowing of the range (combined with the more recent ice core evidence) suggest that the high-end estimates likely are too high?

p. 10, l. 33: Please say what is meant by “model consistent”, or otherwise give a bit more detail about how the 3D lapse rate is computed.

p. 13, l. 15: “the ablation in the SW reaches much lower values”. Please clarify whether ablation is lower, or the SMB is lower (i.e., more negative).

p. 15, l. 2: Can you say why the annual warming signal is less pronounced in NorESM?

Do you suspect a winter cold bias, a summer cold bias, or both? (I don't think this is critical to explain, just helpful if you can make an educated guess. Similarly for the next question.)

p. 15, l. 7: Are you able to explain why Arctic amplification is mostly absent in the early Eemian?

p. 18, l. 2: I think "challenging" is not the right word here; not including SW for the Eemian seems like a more fundamental flaw. Maybe "highly problematic"? Similarly, "challenges" in l. 9 below could be replaced with something like "complicates".

p. 18, l. 4: I suggest removing "or other deficiencies", since deficiencies apart from coarse resolution haven't been discussed.

p. 18, l. 11: I'm not sure NorESM should be described as "relatively high resolution". Its resolution is low compared to MAR, and is not high compared to other IPCC-class ESMs. Some global ESMs, for example, run with a 1 degree rather than 2 degree atmosphere.

Section 6: The first part of the discussion appropriately focuses on big issues such as variable topography and climate forcing resolution. Later, e.g. the second full paragraph on p. 20, it gets into finer details such as refreezing and temporal resolution in BESSI, which might fit better in Section 5.

p. 18, l. 27: A discussion of evolving Greenland topography should refer to the study of Ridley et al. 2005 (in the context of future warming and deglaciation), and possibly some more recent coupled ESM-ISM studies.

p. 19, l. 5: "neglecting the meltwater influx to the ocean from the retreating glacial ice gives warmer simulated air temperatures". Can you say briefly why this is the case?

p. 21, l. 1: "it is hard to argue why a energy balance model which needs poorly constrained information (e.g., net radiation) would produce more reliable results for paleo ablation than a simple PDD model". I don't think this statement is well supported.

For example, incoming solar insolation is very well constrained by orbital calculations, and this alone is a good reason that an energy-balance model might produce more reliable paleo ablation than a PDD model.

p. 21, l. 15: “different SMB models should be included in Eemian ice sheet simulations to capture uncertainties”. I disagree with this statement. It is true that there will always be some uncertainties in atmospheric variables (such as cloud cover) that influence the surface energy balance. But it does not follow that “the uncertainty of Eemian global climate simulations cannot be narrowed down further.” (For instance, one could build a better cloud model.). Also, I see no reason not to use the best computationally affordable SMB model (either MAR’s SEB model or something comparable). See comments above for p. 1, l. 14.

p. 21, l. 18: “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”. Please define what is meant by high resolution. E.g., finer than 1 degree? Fine enough to capture orographic precipitation and narrow ablation zones?

I’m again unclear on the value of a range of SMB models for UQ, unless the range includes other models with SEB schemes comparable to MAR (e.g., RACMO). Also, it could be valuable to explore a range of parameter settings within MAR, to the extent that certain parameters are uncertain and tunable.

p. 22, l. 1: “we recognize that a further improved intermediate complexity SMB model (i.e. albedo parameterization) would be very useful for forcing ice sheet models on paleo time scales.” I agree that models like BESSI could be improved for paleo simulations, but I don’t see why an improved intermediate model would be preferable to SMB forcing from a detailed RCM. Assuming that you’re already using MAR or another RCM for dynamical downscaling, why not just use the RCM’s SMB?

p. 22, l. 7: “further effort needs to be put in developing fully-coupled regional climate-ice sheet models and making them efficient enough to be run over whole glacial-

interglacial cycles". I'm unclear on the role of global models here. Is the idea that the RCM would be run interactively with a global climate model, or just the ice sheet model? Also, what is meant by a whole glacial-interglacial cycle? Do you mean an interglacial time scale ($\sim$10 kyr) or a full glacial time scale ($\sim$100 kyr)?

I think that coupled GCM-ISMs have a role to play, which is not acknowledged here. Other GCMs/ESMs could prove to be more accurate than NorESM for Eemian SMB studies, using some combination of higher (or spatially variable) resolution, improved cloud and snow physics, and SEB schemes with subgrid elevation classes. Even if the SMB from a global ESM is less accurate than the dynamically downscaled SMB from MAR, this disadvantage could be offset by the benefits of simulating topographic feedbacks in a global model.

p. 22, l. 8: I disagree with the last sentence of the conclusions (in particular, "combining with various SMB models"), for the reasons stated above.

Technical Corrections

p.2 l. 10: "While" -> "However"

p. 2 l. 17: "Global Circulation Models" -> "Global Climate Models"

p. 2, l. 19: No caps in Surface Mass Balance. Likewise Surface Energy Balance, l. 24

p. 2, l. 27: Delete "due to"

p.2, l. 28: "which is the reason for" -> "which are primarily responsible for"

p. 4, l. 27: typo, "Ber/ge/n"

p. 5, l. 2: "Firn densification is realized with models. . .". Awkward phrasing; please reword.

p. 7, l. 6: "This 30 years" -> "These 30 years of output. . ."

p. 7, l. 8: "downwards "-> "downward"

p. 8, l. 14: Add comma after “topography”

p. 9, l. 14: Delete “a” before “Eemian”

p. 10, l. 4: “we are not discussing the ice dynamics used further.” Suggest “we do not further discuss the ice dynamics.”

p. 11, Fig. 3: “Nisancioglu” is misspelled. Suggest adding “Simulated” before “sea level rise” in the title.

p. 12, l. 9: Add units after “5”

p. 13, l. 6: No commas needed in this sentence.

p. 13, l. 22: No comma after “Both”

p 13, l. 29: “lower-resolution” (with a hyphen)

p. 13, l. 35: “are we using” -> “we are using”

p. 14, l. 9: can not -> cannot

p. 15, l. 14: “with ice thickness thinner” -> “with ice thinner”

p. 15, l. 18: “is thicker” -> “are thicker”. Also, do you mean an ice thickness increase?

p. 17, l. 11: Delete “the” before “their”

p. 18, l. 4: Suggest “Both the climate and the type of SMB are important”

p. 18, l. 30: Misplaced parentheses for Merz citation

p. 19, l. 6: “assumed” is not the right word, since you’ve given an argument. Suggest “. . .130 ka temperatures are likely warmer than the actual temperatures, resulting in. . .”

p. 19. l. 24: No quotes needed for “cooler climate states”. Likewise below for “warmer climate states”.

p. 21, l. 7: “assumption” -> “inference”

p. 21, l. 32: No comma needed after “Despite”

p. 29, Fig. 5: Use the same symbol for, e.g., ice cores in both temperature and precipitation plots. Should l. 2 of the caption read “temporally and spatially varying 3d lapse rate”?

p. 32, Fig. 8: The panels are small and hard to read. One way to make them larger would be to switch row and columns, thus having three panels across for Ann, DJF and JJA, and time running downward. Similarly for Fig. 9.

p. 37, Fig. 13: It’s hard to read PI values beneath the other lines. Maybe these could be shown on a vertical axis to the right of the timeline.

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