

Interactive comment on “Greenland Ice Sheet model parameters constrained using simulations of the Eemian Interglacial” by A. Robinson et al.

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This is a thorough, well-conceived study with significant new ideas, methods, and results. The authors apply a coupled regional climate and ice sheet model to address the controversial and uncertain question of how much the Greenland Ice Sheet retreated during the last glacial period. The sensitivity tests are carefully considered and the presentation is unusually clear, concise, and well-illustrated. This is one of the best-written papers I have ever reviewed.

The authors are objective and balanced and reach substantial conclusions. Although their results do not provide much greater understanding or certainty regarding the main question - how much did Greenland retreat in the Eem - they do provide what are likely the best-constrained estimates of this that are available to date. The methodology

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is solid, transparent, and well-rationalized. Thanks to the authors for this clear study design and presentation; this is a great contribution that should be published.

I attach an annotated manuscript with numerous minor concerns and suggestions. Included in these comments are two more general concerns that I would like the authors and Editor to consider.

1. I can think of a couple of areas where the sensitivity tests/bounds do not bracket the full realm of possibility. In particular, Eem summer warming in the Greenland region in the large-scale model (with two grid cells over Greenland) is tested for 1.7 to 3.4 degC, but it is possible that peak Eem warming exceeded this (e.g. CAPE reconstructions of up to 5 degC). While warming of that extent may be unlikely, or may implicitly include internal feedbacks from albedo and elevation that are included in the modelling of this paper, it cannot be ruled out. Other parameters in the modelling, such as sliding and the melt model, have similar questions attached to them. There is a wide range of phase space explored here, but within the parameters of the models/parameterizations; other approaches to modelling basal flow and melt (e.g. a full energy balance within an RCM) might give sensitivities outside what is explored here. This warrants a comment I think. The main conclusions on GIS retreat during the Eem are thoroughly supported by the tests presented here, but they are not a complete sampling of what is possible and they are probably not the final word.

2. I do question one of the fundamental premises and constraints that the authors employ, the amount of modern surface melt/runoff predicted by the model. (Or more precisely, the fraction of accumulation that this makes up). By taking a fraction, the authors may mask large biases in the modelled melt, e.g. it could be biased to be both too melty and too wet. This would lend uncertainty to predictive skill going forward or backward in time, as there is no reason to expect such offsetting errors, if they exist, to balance out in the same way under a different climate regime.

Also, as noted in the attachment, it is not clear to me that the ice extent, which the

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authors argue we should ignore, is a less robust predictor of model skill than the fraction of runoff/accumulation. The authors argue that missing fast-flow physics and poor resolution and representation of the ice margins make for a poor prediction of ice extent unless melt rates are turned artificially high. (Sidenote: the authors should add to this list the poor representation of ice-ocean interactions and ice sheet losses at marine margins; these are processes that are not well-represented in all ice sheet models, but also compromise model skill at predicting ice sheet extent and mass balance).

I agree with this but it is hard to isolate how the missing model physics affects ice extent vs. mass loss via calving. Both are compromised from the lack of model skill at ice-marginal and ice-ocean processes. Some areas that experience heavy melting in the model presented here would likely lose their mass 'first' through ocean melting and calving, in reality, if these processes were better captured in the model. This means that constraint that is applied, on surface melt totals, is not totally robust. Melt may be overestimated because some ice does not reach the ocean when it should, or does not melt from below or calve. If this occurs, it leaves 'room' for higher melt rates in other parts of the ice sheet, e.g. in some of the terrestrially-terminating regions of Figure 4 where too much ice is predicted, while still falling within reasonable amounts of total modelled melt.

I think the constraint as applied by the authors is interesting and has some merits, but I don't fully trust it and would argue that ice extent should also be considered. I won't insist on this but like love to see a bit more discussion on this.

– Thanks again for a super contribution.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/6/C1055/2010/cpd-6-C1055-2010-supplement.pdf>

Interactive comment on Clim. Past Discuss., 6, 1551, 2010.

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