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Climate of the Past Discussions

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

Anonymous Referee #2

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The paper presents a fair attempt to revisit a matter of large importance, namely the possible size of the Greenland ice sheet size during the Eemian interglacial, a period warmer than today and possibly the best analogue to better understand the sensitivity of the ice sheet to future warming. By itself, the results are not much more conclusive than previous modeling studies published over the last 20 years. Ever since ice cores demonstrated that the central dome must have survived the last interglacial period with relatively minor elevation changes, this implied an upper bound to sea-level rise of around 5 m. The lower bound has however proven much more difficult to constrain and this study is no exception. The major problem, of course, is that there is no strong proxy for the magnitude of summer melting around the ice sheet margin during the Eemian, likely by far the main control on the ice sheet size. Although one can criticise the 3 main constraints used in this study for being rather weakly correlated to Eemian marginal

melting, or for being dependent on the poorly known amount of summer warming, I still believe the present study has potentially enough merit to be considered for publication in Climate of the Past. Especially the new treatment of the mass balance (REMBO) is an original approach to modeling the Greenland ice sheet. However, a revised paper should convincingly address the main concerns raised below, requiring a sharpening of the reasoning and the inclusion of additional material to better make their point (or not).

Comments in sequential order

p. 1554, line 5: mention that summer temperatures 'at the margin' are what is required.

p. 1556/1557: it is mentioned that SICOPOLIS v. 2.9 includes 'a physically-based treatment of the temperate layer at the base of the ice sheets via explicit calculation of the water content of the temperate basal ice'. Is this at all relevant for Greenland, is such a layer actually occurring?

p. 1557, line 27: REMBO does not assume changes in relative humidity at the Greenland borders. What is the implication of that assumption for precipitation changes over Greenland during the last glacial cycle and during the Eemian? That is an important issue because the central ice thickness responds quite strongly to accumulation changes in addition to the changes in extent driven by marginal melting. Fig. 1 should include a curve on the precipitation evolution (e.g. as a ratio wrt present) as those can also be compared to constraints from ice cores.

p. 1558: first paragraph. More details are required on how the ice sheet extent during glacial times depends on the sea-level forcing. How far out on the continental shelf can the ice sheet expand?

p. 1559: it is puzzling why the first constraint on mass balance partition is diagnosed for a fixed topography. Ice-sheet modellers are well aware that a modelled topography differs from an observed topography, especially concerning steepness of the margin

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and consequently the ratio of ablation area to accumulation area. Since a fixed topography is used to constrain behaviour obtained afterwards for a modelled topography an important systematic bias is introduced. A set of mass balance parameters that gives a reasonable partition for a fixed topography of the Greenland ice sheet will not do so for a model run, and this bears directly on the amount of ablation during the Eemian period. This problem should be carefully and convincingly addressed (see also further).

p. 1559, line 16: is it really 'precipitation' that is meant here, or do the authors in fact mean 'accumulation'?

p. 1559, line 18: what is the difference between 'calving' and 'ice discharged into the ocean'?

p. 1559, section 3.2: the authors ought to provide stronger arguments why they consider the present-day modelled absolute elevation to be a useful constraint for the Eemian? The present-day elevation has virtually no memory of the Eemian. Can we really constrain the Eemian climate well enough to use a set of parameters constrained for the present day as a good model validation for the Eemian?

p. 1560: total gas content has been contested as a good proxy for elevation changes. Referring to Raynaud et al. (1997) it is written that the gas content of the GRIP ice core indicates 'isotopically' warmer conditions. How is gas content related to isotopic composition? On what grounds do the authors interpret this as a surface lowering of maximally 400 m as the oxygen isotope record has equally recorded climate change? Assuming a large Eemian warming the central ice sheet could in fact also have been thicker. This argument needs sharpening.

p. 1562: why is so much importance attached to the geothermal heat flux as a perturbed model parameter? The geothermal heat flux controls the area of basal sliding and the temperature in the basal deformational layers, but many different values of the geothermal heat flux can give similar ice thicknesses with another choice of sliding coefficient and ice hardness (enhancement factor in the flow law). Besides, the

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geothermal heat flux has a high spatial variability and using a constant value may just be too simple to use this parameter as an influential parameter. This point needs more discussion.

p. 1564-1565, section 4.5: apparently, a spatially uniform temperature anomaly is applied all over the model grid, identical to what was done in most previous studies. That is a major simplification that should be discussed more fully. For instance, GCMs usually indicate that temperature anomalies over the central ice sheet are larger than those at the margin. Secondly, changes in precipitation follow from a constant relative humidity. More information (a graph and/or discussion) should be provided on what this implies for precipitation changes. For instance, what is the precipitation change for a 20°C cooling over central Greenland, and what is it for a 5° warming? Is it much different from the usual treatment based on the Clausius-Clapeyron relation?

p. 1566-1567: Figure 5: The colour scale does not allow to distinguish many details apart from the fact that elevation change is (not surprisingly) the main contributor to surface temperature change. This should be revised.

p. 1567, section 5. A figure comparable to Fig. 5 should be added for precipitation (or accumulation) anomalies/ ratios. Arguably, accumulation changes may be at least as important for ice thickness of the central dome than marginal melting during the Eemian.

p. 1567, lines 16-19: I disagree on the role of fast processes and/or model resolution to produce a steeper modelled than observed ice margin (and therefore requiring higher surface melt for the same extent). Papers by Saito et al. (2007, AnnGlac 42) and Van den Berg et al. (2006, JGlac.) have clearly demonstrated that this is a numerical artefact in ice-sheet models due to the flux calculation at the margin. This is an important issue the authors need to clarify in terms of using a fixed topography to constrain mass balance model parameters.

References: the first paper on modeling the behaviour of the Greenland ice sheet

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during the Eemian with essentially similar methods was published by Letreguilly A., N. Reeh and P. Huybrechts (1991) in Global and Planetary Change (The Greenland ice sheet through the last glacial-interglacial cycle), and deserves to be referenced and discussed.

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