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Climate of the Past Discussions

Interactive comment on "Quantification of the Greenland ice sheet contribution to Last Interglacial sea-level rise" *by* E. J. Stone et al.

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This manuscript describes simulations of the Eemian evolution of the Greenland ice sheet and a probabilistic analysis of the results. The authors use an innovative approach to apply more realistic boundary conditions from the GCM fields. Additionally, they perform an ensemble of simulations and apply probabilistic methods to weight the results. The methods applied (particularly the statistical analysis, the climatic interpolation and the ensemble generation) are quite rigorous and serve to move paleo ice sheet modeling in the right direction. However, I have several concerns about some of the initial assumptions and some of the conclusions that are derived from this study, which I list below. I would recommend publication of this manuscript if the authors address these concerns.

1. GCM Climatic forcing. Overall, by interpolating between different time slices and topographies, the approach for producing more realistic transient climates for the ice sheet is clever. However, I am not so convinced by the use of the "cl_pice" (partially melted) state. In the discussion, the authors mention also running simulations that only interpolate between ice-free and ice-covered states, and producing similar results. To me, this seems like a better and cleaner approach, since it could be argued that the method as it is now would impose a deglaciation pattern similar to that of the future warming scenarios of Stone et al. (2010). Although not a requirement, I would suggest using the alternative simulations instead (only interpolating between ice-free and ice-covered states).

2. PDD. The authors, while aware of the issues of using PDD forcing during the Eemian, justify its use here with the following sentence: "However, although the mass balance scheme used in this study does not take into account directly the radiative forcing, it does indirectly because the GCM sees the full insolation change, which then modifies the seasonality of the surface temperature which drives the PDD scheme." Unfortunately, this justification is incorrect. For example, as stated by van de Berg et al. (2011), "Our sensitivity experiments show that only about 55% of this change in surface mass balance can be attributed to higher ambient temperatures, with the remaining 45% caused by higher insolation and associated nonlinear feedbacks". Through sensitivity analyses, they show that "the PDD method significantly underestimates melt for the experiments with Eemian insolation conditions". Thus, although the GCM climate shows higher temperatures or different seasonality, the PDD melt scheme as applied still cannot account for a fundamentally different ratio between the absorption of shortwave and longwave radiation. In principle, this could perhaps still be done using the PDD method by increasing the PDD factors as a function of the insolation anomaly. At a minimum, I think this point needs to be discussed in more detail. However I would also strongly recommend that the authors consider if their method could be improved somewhat to account for the insolation change. In principle, the suggestion above would only require rerunning the rather short Eemian simulations, which

should certainly be computationally feasible.

3. Model skill score. The following statement is very problematic for me and highlights a larger issue with the manuscript: "Our estimate is more reliable because it derives from a full probabilistic analysis, taking into account ice sheet model and data uncertainties." The probabilistic methods applied here certainly improve the interpretation of the modeling results and provide more information than only performing one simulation, for example. However, the skill score that has been applied is based only on consideration of how well the model reproduces the present-day distribution of ice. This is a rather dubious criterion, however, especially given that the shallow-ice approximation ice sheet model is unable to account for fast flow and consistently produces too large ice sheets for the present day. Figure 9 should already serve as a warning in this respect, since the entire range of simulated ice volumes are well above that of the data. Using ice volume as the sole evaluative criterion thus gives higher skill to model versions that likely compensate for this lack of physics with higher melt rates, which doesn't necessarily make these model versions more realistic and could dramatically impact the pattern of ice loss simulated during the Eemian. I would therefore strongly urge the authors to consider additional criteria in their assessment of the performance of the different model versions.

- Minor comments ————-

Abstract: Please consider rephrasing the term "coupled climate – ice sheet model simulations", as I think this a bit misleading. The simulations performed here do attempt to account for potential coupling effects between the climate (GCM) and the ice sheet and this should be acknowledged. But to me, the term "coupled" implies something more interactive.

Please move the description of the probabilistic method (section 5.1.1) to an Appendix. It is important to show the statistical methodology used, but such a detailed description of arguably well-documented statistical methods in the paper itself distracts from the

main message of the paper.

Page 2740, line 29: "The monthly average climate...". This wording is a bit unclear. By climate, I suppose the authors mean temperature and precipitation fields? This might be more clearly reformulated as "The monthly average variables of temperature and precipitation, here denoted as CL(t),...".

Climate interpolation equations: The equations were somewhat hard to follow because of the symbols used and the mixed sub/superscripts. For example, "CL" could be represented more concisely by one letter, like "C", and "vol" could be "V". Furthermore, I don't see the need to switch between lower and uppercase letters CL or cl, as they are the same variable. Also note that right now, "ice" and "pice" appear as subscripts for "vol", but superscripts for "cl", which seems inconsistent. Finally, instead of putting the time in subscripts for the GCM climate fields, this could be in parentheses, like (t) is elsewhere. This would leave the only subscripts to be "ice", "pice" and "0", which are enough to indicate that these are the GCM derived fields. These are only suggestions, but I think the equations would be much easier to follow.

Page 2751, line 25: In this context, the proper reference concerning temperature-melt relationships and insolation is Robinson et al., The Cryosphere, 2010.

Table 1: The units given for the PDD factors are mm / d / degC. Is this mm water equivalent or mm ice? Please clarify.

Table 2: Perhaps it would be useful to add a column with the 65 N maximum summer insolation anomalies as well.

Table 3, caption: please change "brackets" to "parentheses"

Figures: It seems that some of the figures concerning statistical uncertainty could be condensed. Perhaps Fig. 10 and Fig. 14 could go together as four panels, which would also facilitate their comparison. Fig. 12 and Fig. 13 could also be combined. It would also be very interesting to see the 2D climatic forcing fields that were applied to the ice sheet during the Eemian. For example, for the highest skill score simulation, it would be informative to see the temperature (summer) and precipitation anomalies applied at the time of the maximum temperature anomaly and at the time of minimum ice volume.

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