The Cryosphere Discuss., 5, C2181–C2184, 2012
www.the-cryosphere-discuss.net/5/C2181/2012/

The Cryosphere Discussions

Interactive comment on "Melting of Northern Greenland during the last interglacial" *by* A. Born and K. H. Nisancioglu

A. Born and K. H. Nisancioglu

andreas.born@climate.unibe.ch

Received and published: 20 April 2012

We would like to thank all four reviewers for very constructive and helpful comments. We will be happy to address all suggestions in a revised manuscript in the coming weeks together with point-by-point answers to all the reviewer's comments. At this point, given a substantial overlap in the comments, we will summarize the major issues and report on progress that has been made so far in addressing these.

All reviewers require a more comprehensive literature review, and moving part of that review from the discussions section to the introduction. We will gladly follow this advice and extend the manuscript text to include the suggested publications from both modeling and ice core research. This issue is partly related to shortcomings in the motivation, also mentioned by some reviewers (R #2 & #4). We agree that the phrasing of the text

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

must be revised with attention to not rely on the comparison with just a few influential studies. However, we believe that these studies led the discussion and investigation towards a disproportionate focus on the stability of the southern part of the Greenland ice sheet. Leading to a wealth of observational and proxy studies in this region in recent years while a second region of concern (the northeast) did not receive much attention. We acknowledge that while parts of the glaciology community are aware of the vulnerability of the low-accumulation region in northeastern Greenland, and results consistent with this idea have been published before, the perception is different in other fields of paleoclimate research and an explicit discussion is needed. We will revise the text to clarify this point and include all the relevant literature.

Reviewer #1,#2 and #3 rightfully criticize our choice of initial ice temperature. It was suggested to either discuss the shortcoming (R #1) or repeat the experiments from a spin-up over an entire glacial cycle. Reviewers #1 and #3 suggests exploring the uncertainty related to initial ice temperature. We are very grateful for these comments and decided to repeat all experiments including the ensemble with three different initial ice temperatures. The ice was initialized with a constant temperature of -30/-40/-50 deg C from the surface to a distance of 1000m from the bedrock. Further down, the temperature increases linearly to -5 deg C at the lowest model level. This is motivated by borehole temperatures at GRIP (Johnsen et al, Tellus, 1995) and a reconstruction of surface temperatures from the last glacial period (Dahl-Jensen et al., Science, 1998). We use -50 deg C as the minimum temperature despite lower estimates for the last glacial maximum (LGM), because accumulation arguably was very low during the LGM and does not comprise a large part of the ice volume. The use of three different initial temperatures triples the number of experiments (to over 15,500) leading to a long waiting time for the computation to complete. We finalized all experiments with climate forcing from IPSL CM4, and about half this number for CCSM3. Our original findings are robust to the change in model set-up.

Reviewers #1,#2 and #3 suggest the description of the climate model data and coupling scheme to be extended. Reviewer #3 raises the issue of poor simulated seasonality in coupled climate models. Reviewer #4 asks for an improved description of the ensemble set-up. All these comments will be addressed in detail and with additional figures. We will extend the corresponding sections and include maps of the relevant seasonal forcing used to force the ice sheet model. The impact of climate model forcing deficiencies on the ice sheet model simulation will be discussed and any caveats will be taken into account. We will also extend the review of literature on the climate model experiments. The performance of these models has been assessed repeatedly in previous publications.

From comments by reviewers #2 & #4, we understand that our discussion of the stability criterion is too brief and that key assumptions are not sufficiently motivated. It was our aim to accompany the complex 3D ice sheet model (SICOPOLIS) with the most elementary model that can explain the modeled change qualitatively. One key assumption is that the ice topography at the beginning of the Eemian was comparable to today (although with a different ice temperature). This approach is debatable but not necessarily worse than a spin-up or other initialization techniques that introduce similar uncertainties. The second important assumption is that this initial ice topography was subject to a fast climate change (for an ice sheet) of large amplitude. Again, little data exists to unambiguously support this assumption for the Eemian, but the very secure reconstruction of seasonal solar forcing due to orbital changes is a good indicator. These two assumptions are reflected in the design of the ice sheet model experiments. Since the ice topography and therefore the ice flow do not differ in the experiments for present-day and Eemian climate forcing but some regions show strong ice loss with the latter, the difference must be in the mass balance. We attempt to quantify this imbalance and illustrate the fact that the regions of strongest melting are not necessarily the most vulnerable when calculating the stability criterion. For the intended purpose, we deliberately chose a very simple conceptual model, neglecting ice flow. We also attempt to estimate the ice volume transport necessary to compensate the mass imbalance. Note that the main arguments do not rely on the precise definition of this criterion, but on the extensive set of ice sheet model sensitivity experiments that do include a realistic representation of the ice flow. We will clarify this misunderstanding in a revised version of the manuscript and discuss the justification of the stability criterion in depth.

Reviewer #3 cautions against the comparison of Eemian and projected future climate change because of very different change in radiative forcing, and suggests expanding this discussion. We agree that this is an important point to be addressed and that the analogy should not be overstated (e.g. van de Berg, NGS, 2011). However, we will try to highlight the basic finding that the northeastern Greenland ice sheet is vulnerable to any warming due to the dry climate of the region.

Reviewer #1 suggests developing a higher-order positive degree-day model including changes in shortwave radiation during the Eemian. This could improve the realism of our simulations, but would also introduce more degrees of freedom and therefore more uncertainty. Note that changes in shortwave radiation, as simulated by our global climate models, critically depend on the representation of clouds and cloud-related feedbacks in these models. This continues to be one of the largest uncertainties in climate modeling in general, and especially over high topography. Our aim is to address uncertainties arising from the melt parametrization differently, by evaluating a large ensemble of different parameter settings. The design of these experiments, parametrizations, forcing and initialization, are not suited to provide a realistic representation of the Greenland ice sheet during the Eemian and we do not claim so. We want to highlight the sensitivity of the Northeastern Greenland ice sheet to climate change. As stated above, this motivation will be clarified in a revised version of the manuscript.

Interactive comment on The Cryosphere Discuss., 5, 3517, 2011.