Patrick Applegate
Department of Geosciences, Pennsylvania State University
-and- Physical Geography and Quaternary Geology (INK), Stockholm University
patrick.applegate@natgeo.su.se

22 March 2012

Dear Dr. Larour,

Thank you very much for considering our manuscript for publication in The Cryosphere. Both reviewers made very helpful comments on the paper, and we have addressed all of these comments in our revisions.

Major changes include
-- a new Section 2, describing how ice sheet models work and what the modeling community is doing to improve them
-- a new Figure 8, showing the effects of initial condition on the ice sheet model output
-- a new Figure 9, showing a comparison between observed and modeled total mass balances over the last half-century

We also corrected an inconsistency between the temperature curves covering 1840-3500 AD and the temperature and precipitation data sets. In the original draft of the manuscript, the baseline for the temperature curve was set such that the 1840-1869 temperature anomalies would average to zero. However, the temperature and precipitation grids used in the model are based on observations from the latter part of the 20th century and the earliest years of the 21st. To correct this problem, we adjusted the temperature curve so that the baseline is now 1976-2005 and reran the affected parts of the simulations. This change does not affect any of the conclusions of the manuscript, as can be verified by comparing the figures in the revised manuscript to the earlier figures.

We will be happy to make additional changes if necessary. Thank you again,

Patrick Applegate
and co-authors

Response to reviewer comments
Applegate, P. J., Kirchner, N. K., Stone, E. J., Keller, K, and Greve, R., in review 2011, The Cryosphere Discussions.

Reviewer #1

This paper asses the range of the contribution of the Greenland Ice Sheet to sea level rise by 3500 as predicted by the ice sheet model SICOPOLIS for various sets of model parameters. A 125ky time-period is used to spin-up the model starting from present day geometry. The performance of the model (i.e. of each set of model parameters) is assessed by comparing the modeled present-day ice volume to the observation, allowing the culling of the ensemble of model parameters. The model performance in reconstructing paleo-ice volume is discussed in section 3. The main conclusion of the paper is that the range of projected contributions to sea level rise in 2100 remains large even after culling the ensemble.

The quality of the presentation is good. The subject is within the scope of The Cryosphere and the methods and results presented are an important contribution to assess the uncertainty of ice-sheet model projections.

However I have few major comments that I think should be taken into account and I will advise major corrections for the moment:

1) Because of the topic related with sea-level rise projections, this paper is meant to be read by a wide audience not especially familiar with all the details of ice sheet modelling. Before assessing the effect of a parameter on the model results it is important to clearly address the assumptions in the model and its parametrizations and discuss their effect on the results. I found these discussions too short in sections 2.1 and 2.2. For clarity and to assess the applicability of the paper results the authors should try to answer the following questions:
-- What does affect the ice sheet volume in the model?
-- What physical process is the parameter meant to represent?
-- What are the assumptions?
-- How do they affect the model results?

Reviewer #1 raises several important questions, and we agree that extra material on this topic would help widen the paper's potential audience.

To address the reviewer's comments, we added Section 2, "Overview of ice sheet processes and models," which contains subsections on "Ice sheet processes" and "Model structure and ongoing improvements." This section provides a qualitative description of how ice sheet models work, as well as deficiencies in standard shallow-ice models and what the community is doing to address them. We feel this extra material directly addresses bullet points #1-3, above.

Point #4 is very difficult to answer at the present time, because model treatments of many relevant processes are simply not available yet. We address this point in Section 2.

2) For the projections, the paper focuses more on the results for 2100 and one conclusion of the paper is that the ice PDD factor is the dominant factor. There is statements in several sections that SICOPOLIS does not incorporate higher order physics to model the ice flow and thus ,"lack certain observed physical that may tend to enhance the real ice sheet response,"; but there is nearly no mentions (except ref to Straneo et al. in the conclusions) to these observations and the reader don,'t know exactly how important these processes could be. This comment is obviously related to the first comment.

See above.

3) I think that it could be interesting to add a section to discuss how ice sheet models could be improved. As the conclusion is that the range of projected values for sea level rise is large, the paper should try to give some clues about how we could reduce this uncertainty? Should we put efforts in trying to constrain the free parameters, should we change the parameterisations, implement higher order physics, couple with climate/ocean models?

All of the above. As we now explain in Section 2.2, the community is already working very hard to improve the models, and we wish to provide tools for evaluating new models as they become available.

Detailed remarks:
-- Title: Why using ,"preliminary,"?

We adjusted the title, since both reviewers asked us to remove the word "preliminary." This study is preliminary because the ensemble of model runs contains only 100 members, fails to reproduce important aspects of the ice sheet's history, and lacks any real objective function for comparing the model results to data.

-- Abstract: I think another conclusion of the paper is that the climate forcing(i.e the ice PDD factor) is the most influential and that the parameters governing the ice flow are rather non-influential. This could be put forward in the abstract. But it should be discussed more clearly in the paper that this could not be true for very short term projections as highlighted by the recent observations of ice dynamics, and this is why peoples are putting efforts in developing higher order models (see general comment 2).

We added an extra sentence about the importance of the surface mass balance parameters to the abstract. We are unsure how to answer the second half of the reviewer's comment. In our new Figure 9, both the observed and modeled changes in mass balance track very well with surface temperature changes over the last half-century. Thus, we do not need to invoke dynamic changes to explain the observed mass balance changes. But, we feel that making any statements about the relative importance of dynamic and surface mass balance changes on the present evolution of the ice sheet from these model results would place us on very shaky ground.

-- Section 2: you could make an explicit mention to the other methods to assess model sensitivity to parameters and initial condition, i.e. the work of Heimbach and Bugnon with SICOPOLIS.

Done. There is now a paragraph in the new Section 5.4 on these methods.

-- Section 2.1-2.2 : see general comment 1). ‚Äì Especially describe the source of mass gain (only through accumulation I assume) and mass loss (ablation, basal melting, and I assume that the icesheet is not allowed to develop floating extensions (it is how sea-level is used to force the model?) resulting in an ice flux to the ocean). ‚Äì Describe how the parameters affect these sources of loss and gain. ‚Äì Discuss the different assumptions behind the model/parameterisation (for exemple Heimbach and Bugnon show that the model is sensitive to reparti- tion of basal sliding and geothermal heat flux, so that using constant values could be too simple.).

All fixed. Much of this material appears in the new Section 2. We added a sentence on the lack of ice shelves in the model to Section 3.1. The individual parameters now have descriptions of how they affect the model behavior, as well as a note on whether they are spatially constant in the model vs. the real world.

-- l164- ‚"this initial condition is not ideal... The errors in the initial condition should average,"; This question could be addressed for example by taking 30 to 85% of the modern ice thickness and running the model with a set of parameter resulting in a good match of the present day volume. Will this set of parameters still lead to a good fit?

Fixed. We performed an additional model experiment and added the new Section 5.4 and Figure 8 to address this question. The extra model experiment starts from ice-free conditions, providing a maximum bound on the uncertainty introduced by initial conditions.

-- l-224 - the present rate of mass loss could be another metric to try to match

Done. We added Section 5.5 and Figure 9 to address this comment.

-- l-315 ‚"values of the ice flow," -> ‚"values of the parameters affecting the ice flow,"

We amended this sentence to read, "...Stone et al. (2010)... noted that high values of the ice flow enhancement factor and ice positive degree day factor yielded the best matches with observed total ice volume..."

-- l-345 ‚"...the large scale shape of the ice sheet is more strongly controlled by surface mass balance than ice-flow,"; this requires more justification or references.

Fixed. We expanded this sentence to read, "However, the large-scale shape of the ice sheet is more strongly controlled by surface mass balance than ice flow; near the ice margins, thin ice and low slopes lead to small ice fluxes that are easily overwhelmed by negative mass balances (Greve, 1997; Alley et al., 2010; Born and Nisancioglu, 2011)." We also added an extra paragraph to this section that should help address both reviewers' comments.

Reviewer #2

The paper under review investigates the sensitivity of projections of the Greenland ice sheet evolution to parameter uncertainties. Using the well-established ice sheet model SICOPOLIS, a 100-member ensemble with perturbed physics is obtained by spinning up the model for 125 ka using information about past climate. Present-day ice sheet volume serves as validation metric, taking into account uncertainties in the observed ice sheet volume. Even after culling the ensemble, the authors observe a large spread in simulated future mass loss between the different model realizations.

The manuscript addresses the important question of quantifying uncertainties in future sea-level projections. The manuscript is well written, reads fluently, and is clearly within the scope of The Cryosphere. Use of a Latin hypercube method is appropriate to sub-sample the parameter space. The methods used in the paper are explained in enough detail, and references to published literature is made whenever appropriate (SICOPOLIS has already been described in detail in the literature).

I have a few minor comments.

General Comments

As reported here (and previously elsewhere, e.g. Stone et al. (2010)) model results are very sensitive to the choice of the PDD factors. I agree with the author's conclusion ,"We attribute the bulk of the remaining errors in geographically distributed ice thickness values to problems with the modeled mass balance" (lines 347-349). I suggest to expand on this statement, and point out potential consequences. Even members of the culled ensemble, i.e., model realizations with reasonable ice sheet volumes, contain large errors in local ice thickness. Due to the temparature-altitude feedback (lapse rate), this will have an impact on the model sensitivity. Figure 7 is very illustrative as it reveals the spatial pattern of differences between modeled and observed ice thickness. From this manuscript it becomes evident that ice sheet volume is a way too weak metric to assess the skill of an ice sheet model to reproduce the present state of the ice sheet. This could be stated explicitly, e.g. at the end of the manuscript, as a recommendation for future work.

Both issues are now fixed. We added some additional discussion of how errors in the ice thickness grid affect the future evolution of individual model runs in Section 5.2. Section 5.5 includes discussion of how additional data and better objective functions could be used to better assess ice sheet model skill.

Model simulations are run until the year 3000 AD, however, the discussion almost exclusively focuses on the short term response (2100 AD). Everything past 2100 AD doesn't seem to add much to the manuscript. I am thus wondering if it would make more sense to remove those parts of the manuscript (incl. figures) that deal with the time past 2100 AD.

The reviewer raises a good point. We would prefer to leave in these parts of the figures. To help support their inclusion, we added a brief new section, 4.5, that describes the model runs' long term response. In particular, we note that the runs do not equilibrate to the new temperature within 1000 years of the final temperature being achieved. All runs have negative mass balances at the end of the simulations.

Detailed Comments

-- Title I agree with the other reviewer, ,"Preliminary" should be dropped from the title.

Done.

-- 80 ,". . . is agreed upon by the ice sheet modeling community." From reading this sen- tence it is not clear to me what is agreed upon. You could either explain or slightly rephrase: ,"Our approach builds on existing work by Stone et al. (2010) by using a spinup procedure that takes past climate variability into account (see, e.g., SeaRISE partners, 2008)". Or something along these lines.

Done. The sentence now reads, "The model setup that we use is specifically intended for the problem of projecting future sea level change (seaRISE partners, 2008; Greve et al., 2011)."

-- 132 change ,"0{20myr1/Pa" to ,"0{20myr1 Pa1"

Done.

-- 205 Awkward sentence: ,"... , the science of sea level rise is evolving rapidly,..."

Fixed. We removed this sentence and expanded the last paragraph of Section 3.3 to better explain our meaning.

-- 259 ,"...simulated ice volumes... are consistent among runs". The range is actually quite large, around 3m SLE, during that period. I assume you're trying to say that ice volume changes are consistent among runs.

Fixed. The new sentence reads, "Between -75 and -10 ka, simulated ice volumes are remarkably stable and the spread among runs, while substantial, is much smaller than during the Eemian (Fig. 3)."

-- 315 ,"High values of the ice flow". I assume you mean ,"large values of the enhancement factor". Throughout the manuscript (incl. Fig. 1), you may want to use ,"Flow enhancement factor", or simply ,"enhancement factor" instead of ,"Flow factor".

Done.

-- 375 change ,"surfacetemperature" to ,"surface temperature"

Done.

-- 415 While some longitudinal stress coupling may be needed to propagate thinning inwards, this will only occur if appropriate forcing at the ocean boundaries is applied. That is, using a higher-order model does not guarantee to accurately reproduce observed rapid thinning.

Fixed. The text now reads, "Additionally, ocean warming may contribute to mass loss where the ice is in contact with the water (Straneo et al., 2010; Yin et al., 2011), and the resulting rapid thinning of marine ice margins could then propagate up ice streams to the central parts of the ice sheet. This scenario cannot be captured by shallow-ice models like SICOPOLIS, but is expected to appear in higher-order models, provided that the ocean boundary conditions are correctly represented."

-- Fig. 7 . ,"Simulated ice volumes are. . . " Don't you mean ,"simulated ice thicknesses"?

Yes, this was a mistake. Fixed.