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The Cryosphere
Discussions

Interactive comment on "Hindcasting to measure ice sheet model sensitivity to initial states" *by* A. Aschwanden et al.

Anonymous Referee #3

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This paper advances the concept of hindcasting, or predicting prior observations, as it may be used to assess the skill of ice-sheet models. This is the first paper to do so, probably because others have avoided the issue due to a paucity of data and concerns about the differences between glaciological and observational time scales. Even as more data have become available, the questions remain, and prevent this paper from having quite the impact it is trying to achieve. Additionally, I have real concerns about what is being attributed to the ice-sheet model vs. what is attributed to the climate forcing. Nevertheless, in this paper I found a reasonably well-defined plan for hindcasting with ice-sheet models, which I am sure will be dissected and reassembled by future investigators. I also found several useful guidelines relating to model initialization, and real insight into the tradeoffs associated with different initialization methods. Finally, the work has been carried out with an attention to detail regarding the input data sets,

and the model runs are openly available to the public through the PISM repository. These facts do much to assure the work has a legacy, and is followed up on by other investigators. Hence, I recommend publication of the manuscript, but only after some of the significant issues having to do with time scales, and surface mass balance vs. dynamical thinning have been addressed in a substantive way.

First, the issue of time scales. There isn't much question about if hindcasting is useful. Plainly, it would be; any means of improving the quality and reliability of ice-sheet models highly desirable. The question is how hindcasting could be done with ice-sheets which are characterized by changes taking place on very long (decades to centuries) timescales, and a very short, incomplete observational record. So, in the language of the paper: what are the 'known inputs' for 'past events'. Presumably this is the 8 year GRACE record, the present day surface and its rates of change, and the present day surface speed. All based on a short, 20 some years of climate forcing. In some cases the model output matches observations well, and in others it doesn't. Given the complexity of the ice sheet models, and the vagaries of the model initialization processes, I'm not at all confident the time periods being compared are reasonable, and am not convinced that a good or poor match (however those are defined) to those observations reveals much.

The authors do attempt to address the short time periods; considering model 'drift', and discussing the potential for transients to be introduced into the system as the climate forcing is changed. In the end I still think more is needed. The ergodic hypothesis is that the time average is the same as the ensemble average. In this experiment we don't have a large ensemble, or a long time period. Why not continue the runs another century under or more (under the same climate forcing) and see how well the trends hold up? If they don't hold up, the authors should address the issue of why. The authors should dispel the notion that we are observing a transient, or a short term fluctuation in ice dynamics that is not representative of the systems true behavior.

Second, issues related to surface mass balance vs. dynamic thinning need to be addressed more clearly. PISM is an ice-dynamics model, giving estimates of dynamic thinning. GRACE is measuring surface mass balance and dynamic thinning. Before the comparisons can be made, GRACE should have the SMB signal removed. I get the impression, from figure 5, that the ice dynamics is very sluggish, and on the 20 year time scales, accounts for an insignificant part of the total dynamics. As such, I'm not even convinced that this is a paper about hindcasting with ice-sheet models. Rather, it may be more a paper about differences in the SMB emerging from different dynamic land models and climate parameterizations. The authors need to confront this more directly. The initializations are different, and the SMB model forcing the ice-sheet in each of the three cases is not quite the same. Lapse rates are applied, and in some cases (flux corrected) anomalies are used. These differences in how the SMB is applied may account for most or all of the differences reported. This might be treated discursively, and parts of the paper related to this should be re-written. It is possible that I've somehow missed the point, but the authors should concede that the presentation is confusing.

Lateral boundary conditions, and calving criteria in particular need to be addressed in the paper. The reader needs some assurance that the observed behavior is not arising from whatever is happening there.

A final point is that I think it's misleading to report the thickness and surface height for flux corrected models. Either it's going to be very close, or the flux correcting scheme is wrong. Reporting it with graphical weight equal to the other runs gives a casual reader the impression that you've got things working well, when in reality all that works is the flux conservation.

Particular Comments on the text:

p 5072:

15: -> equilibrium with modeled present-day climate 20: mass balance from interpolated surface temperature...and model constrained precipitation.

p 5073: 5: specify that the flux correction is the same between forward and spinup. Also, the flux correction changes, right? But it doesn't change in the forward run? Justify. How large is the flux correction? How does it compare to accumulation? Is it unreasonably large or small? 26: "Good match" please be more quantitative! I have no idea what good is. p 5074: 4: remove 'dynamical' p 5075: 18: No fair making a big deal of how well the flux corrected run works. 24: What does 'normalized' mean here? p 5076: This might be the place to outline how to differentiate between SMB and dynamical thinning, and how the analysis will be done. p: 5076 26: a too weak – reword this.

p 5077: first two paragraphs: rework this entirely. It's a critical component of the paper, but in its present form, very challenging to understand. Please consider the points made above, and attempt to clarify the partition between mass balance and ice dynamics.

17: Not sure what is being referred to here 'before ice discharge started to increase rapidly'...

p 5078 18: - 50 Gt "constant climate" OK, but then how do you reconcile the previous page's statement on lines 25-28? Is it ALL due to changes in climate forcing? Some appears to be due to drift, right? 25-28 move to conclusions, this is an interesting result.

p 5080:

7-10: I'm just not convinced the conclusions are as strong as you make them out to be. Spend some time in the conclusion discussing the shortcomings of the approach.

Interactive comment on The Cryosphere Discuss., 6, 5069, 2012.