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The Cryosphere Discussions

Interactive comment on "Greenland Ice Sheet contribution to sea-level rise from a new-generation ice-sheet model" *by* F. Gillet-Chaulet et al.

J.V. Johnson (Referee)

jesse.v.johnson@gmail.com

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In this paper a highly sophisticated ice-sheet model is used to assimilate available observations and then forced with 100 years of climate or basal processes perturbations. The perturbation experiments are used to provide insight into future changes in the mass balance of the ice-sheet, and potential contributions to sea level. The basic experimental setup follows from SeaRISE with most advances occurring in the modeling techniques, rather than experimental design.

In my mind, what is best about the paper is that it establishes a set of methods and procedures that are relatively well defined, and will serve as a template for future in-

vestigation. For example; the authors perform the data assimilation in two different ways and report that which is used is not that important, that's really good to know! They perform an "L-curve" analysis that is illuminating for all who use the Tikhonov regularization, and they identify optimal parameter values. They provide some useful guidelines on mesh production, use some novel methods of estimating the thermal regime of the ice-sheet, and offer a means of easing transients that result from errors in bed topography. These are all of a fairly technical nature, because modeling tends to require a relatively specialized set of skills. As such, the potential audience for the paper is limited, but enthusiastic. The techniques are mostly explained with detail sufficient to allow others to reproduce the effort.

I do not think the paper will attract a larger audience of readers interested in sea-level rise because the experiments, in spite of the model's high fidelity to material physics, are still too crude to be plausible. Specifically, I think that the perturbations to the basal traction are oversimplified, and have no confidence that the initial state plus 50 years of relaxation is a reasonable proxy for the modern GrIS. Nor do I see see significant advances over the SeaRISE experiments. This is a weakness of the paper, but not one that I think needs to be addressed before publication. I see this as more a paper for practitioners than planners.

As such, my recommendations for the authors have to do with the things that other modelers would be interested in reading, and more revelations of what I think the shortcomings of model are. This would make it a better paper for modelers, and help other readers understand the limitations inherit in the prognostic runs that are performed. These authors, in particular, have an obligation to discuss model shortcomings, as theirs is among the very best models available right now. Their honesty in discussing model deficiencies has the potential to set the agenda for the entire modeling community's efforts. What follows are some of the deficiencies I noted. In each case I would ask the authors to investigate the degree to which they are true, with supporting graphics, and comment on why they might be occurring. The resulting paper would provide a much better reference, and help identify areas where more model development in needed.

Rate of change of surface elevation: The authors are right to neglect these data (Prichard 2009) for assimilation purposes (page 2796, lines 14-17), because flux divergences are dominated by errors in bed topography (Seroussi et al. 2011). However, this data should not be neglected entirely. Especially after the relaxation process is done, the rates of change in surface elevation should be close to what the model is producing. I was surprised to see in figure 6 b that even after the relaxation, the rate of change is much different from Prichard 2009. Specifically the typical range near the outlets is 1-10, rather than 0-1 m/a, and the choice of absolute values prevents interpretation of the direction of surface elevation change. The direction is something that I'd be interested in knowing! If it's bad, let us know it is bad, and provide some commentary about what is happening, and why the modeled surface rate of change is so different from the observed. Also, the relaxed figure shows what looks like a classic fingerprint of numerical instability: a 'ringing' or high frequency oscillation of the ds/dt field in the upstream areas of Jakobshavns as well as several other outlet glaciers. This needs to be addressed. How is equation 5 being solved? Can you provide any evidence that it is being solved in a robust manner?

Basal boundary condition and an apparent failure to conserve mass: I credit the authors for being very honest about a loss of 10% of the mass through the lower boundary (see lines 6-12 on page 2806), but I don't understand why it is happening. It seems like it has to do with equation 7, impenetrability, and bed roughness (lines 3-6 page 2795). I guess I thought that equation 7 entered as a Lagrange multiplier and was something that could be satisfied with accuracies similar to those of the state variables. I would really like to know more about what is happening here. Of course, I'm worried that others, myself included, might have similar problems, but at this point I simply do not have enough information to know exactly what is happening.

Lateral boundary condition and the domain: In Figure 2, looking at the close ups, it

does not appear that the extent of the domain matches the margin of the ice sheet. I understand why this would happen; it isn't easy work to create a mesh like this. However, I'm curious about what is done in regions where there is not ice? Is a thin layer applied? What are the boundary conditions for terrestrially terminating glaciers? It looks like they should be stress free, is that correct? Ultimately, I think this becomes a big concern, as we see the flux emerging from the ice sheet sometimes increase, and other times decrease. What is happening during the relaxation period? Is the ice being transported outward, towards the edge of the domain? Is flux computed out the margin of the domain, or out the original ice margin? 50 years seems like an arbitrary value to make sure that the flux is transported from the edge of the ice to edge of the model domain, isn't it? Some more discussion of; 1) how the differences in the edge of the domain and the margin of the ice are reconciled, and 2) how the calculation of how the flux out is computed is needed.

Enhanced sliding: In the BF3 experiment, the authors continuously increase basal sliding by dropping the traction by a factor of 10. Maybe that is possible, none of us really knows the future, but this experiment is at odds with a large body of theory suggesting that basal sliding would tend to be reduced as the transmission of basal water becomes more efficient. They do acknowledge this, but carry on with the experiment anyway. I guess I'd rather see this particular experiment dropped. It's just a little too 'chicken little' for my tastes.

Finally, I read the transcript closely and have a few suggestions on wording or style.

Page 2792 line 3 Stokes equations no need to capitalize 'equations'

Page2794 line 16 horizontale should be horizontal

Page 2795 line 18 speed (scalar), not velocity (vector), right?

Page 2796 line 15 specify that these are bed uncertainties. Also, point out that the rate of change of surface elevation gives a volume change, but that is not necessarily a mass change, due to firn densification.

Page 2800 line 22-23 But this introduces large gradients at the boundary between balance velocity and INSAR velocities. Do you do anything about that?

Page 2802 line 11 guaranty should be guarantee in this context.

Page 2802 line 17 Unsufficient should be insufficient

Page 2802 line 23 Control does not need caps.

Page 2803 line 15 on should be changed to other

Page 2803 line 26 I think that closeness to equilibrium should be shown in table 2 by including an additional column having the integral of the SMB over the catchment.

Page 2804 line 10 How can it evolve, isn't it somewhat fixed by the extent of the mesh?

Page 2804 line 14 Gtaˆ{-1} not Gtaˆ{-2}

Page 2804 line 23 "Future work might consider to use…" very awkward sentence, reword.

Page 2803-2804 The word good is used 3 times. I don't really know what good means in this context. Try and be more quantitative.

Page 2806 line 24 'retro-actions' I'm not sure I know what you mean.

Page 2809 line 11 'processes' say which are most important.

Page 2810 line 1 'self-similar' what do you mean by this?

Interactive comment on The Cryosphere Discuss., 6, 2789, 2012.