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Interactive comment on "Dynamic response of Antarctic ice shelves to bedrock uncertainty" *by* S. Sun et al.

D. Goldberg (Referee)

dngoldberg@gmail.com

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this study makes use of a cutting edge glaciological model in realistic large-scale settings to address a very important question, one of bed elevation uncertainty and its impact on ice sheet projections using representations of ice flow physics. the authors take realistic models of fast-flowing drainages and perturb bed elevation in a sensible manner, and then forward-integrate the model to assess the envelope of transient behaviors determined by the envelope of bed uncertainty. the bed is randomly perturbed within the bounds of uncertainty; and autocorrelation length scale is used as a parameter. at the lowest length scale ($\sim$10 km) there is significant impact on the behavior; for smaller length scales (1-2 km) it is negligible.

i find the idea of this study very novel. it seems to be the norm in glaciological stud-

ies that use observational data to assume all uncertainty rests in the bed friction parameter; and very little attention is given to the fact that bed uncertainties may affect inversions of said parameter, and that the interplay between the two may impact time-dependent behavior. a grounding line retreating past a topographic rise should behave quite differently from a grounding line retreating past a "sticky spot" of locally rough bed or strong sediment. therefore i commend the vision of the authors.

I have a few issues with the results of the study, though, mostly minor but one major – and i would like to see these addressed, otherwise the conclusions of the study will remain questionable.

1) My foremost concern is that the ice flow physics in the model are not sufficient to carry out some of the investigations shown. Specifically, I question the balance of the L1L2 stress balance to represent the interaction of the ice and bedrock at small (1-2 km) length scales. For the lowest frequency experiments ($\sim$10 km), i think this is not an issue and i think the results should be published (subject to comment (2) below). However, at O(1 km) wavelengths, the perturbations would present through such effects as form drag, which is not representable even by Blatter-Pattyn, which is a closer approximation to the Stokes momentum balance. A priori the aspect ratios implied should predict breakdown of the approximation. Furthermore, from my own experience I know that at such short scales L1L2 is a poor representation even of B-P. Granted this is in cases where the bed is somewhat strong, if not frozen – but still I maintain that the low sensitivities seen are because the L1L2 model cannot "feel" the high frequency perturbation. And note that this is an issue of *equation* truncation, not one of numerical truncation, and therefore is not an issue that can be addressed by higher resolution. I would ask that a test be devised to compare these (high-freq) results with a full-stokes model. It could even be in a simple flowline setting, that could be enough – but the idea that the ice sheet is insensitive to high-frequency bed error should be tested by a model that can implement form drag. (note: this is my most serious concern, and the reason i selected "major revisions".)

2) It is not clear whether for each realization of topography a new inversion was carried out. it is highly unlikely that the inversion would find the same traction field for each realization, at least in the low-freq case. This should be done; if it was done, it should be made clear.

3) Figs 9 (top) and 10 are a little confusing w.r.t. the VAF trajectories for the noisy runs. For the PIG runs the variation over the runs presents itself over time; for the T-D and L-A runs it is present right away. Is this degree of spread due entirely to the bed perturbation?

4) While some reviewers might say it is too lengthy, I appreciate section 2.1. As someone who does not rise and sleep in fourier space, it is helpful to understand quantitatlvely how you have generated the noise fields used in your simulations. A few comments on this though:

– 4(a) in eqns 1 and 3, the divisor of “ux” should be M, not m – 4(b) in eqns 1 and 3, upper bound of first summation is M-1, not N-1 – 4(c) eqn 3 looks like an inverse DFT? Shouldn’t the sign on the exponent be positive? – 4(d) surely with initial white noise, there is no reason that the expression of eqn 3 should be real? I was only able to generate figures as in Fig 3 by taking the modulus of this expression, i.e. |f|.

minor comments:

abstract, l2: influence

p481, last full paragraph: should you be bringing up calving, when you do not represent it?

P481, l29: confusing/awkward wording

P482, l5: dimensional

P487, l28: we imposed melt as a piecewise linear function

P487, l29, reference: was this in there? I know it was in the recent Nature Climate

Change paper. . .

P490, l10: decreases

P491, l12: as high as

P492, l22-26: sentence is awkward, and furthermore not entirely justified by your study (you don’t show the section becoming ice-free) so you would need a reference

P493 l3: Law dome *might/could* become an island..

Interactive comment on The Cryosphere Discuss., 8, 479, 2014.