The Cryosphere Discuss., 8, C558–C562, 2014
www.the-cryosphere-discuss.net/8/C558/2014/

The Cryosphere Discussions

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

S. Sun et al.

sainansun1985@sina.com

Received and published: 30 April 2014

We thank Dr Goldberg for his incisive comments. We have added some comparisons between the L1L2 and full Stokes models that we think should answer his major concern (but may still be superseded by a future study), and addressed the others fully.

"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 (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

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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".)"

We carried out some simple tests with the L1L2 and a full Stokes model where we find that both models are decreasingly sensitive to the phase of oscillations in the bed rock topography as their wavelength decreases (and something similar is evident in the ISMIP-HOM tests). So we expect that the responses to a set of short wavelength perturbations (which differ in phase) would be clustered more closely than the responses to long wavelength perturbations in the Stokes case, just as they are in the L1L2 case. There is one notable difference in the Stokes results: the mean value of the velocity decreases as wavelength increases for a given friction coefficient. That would mean that for a given friction coefficient, the cluster of responses to short wavelength noise would not necessarily lie close to the unperturbed calculations. That said, since the friction coefficient it determined to fit the velocity field, we would simply expect to recover a lower friction coefficient for the short wavelength noise, as indicated by referee 2. We added an appendix describing these supporting results.

"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."

Yes, for each realization of topography a new inversion was carried out. We have modified the text to make this 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?"

There is some variation in the initial VAF for PIG too (in some parts of the domain we create more depressions than crests, or vice-versa), but the amplitude of fluctuations is lower (because the observation error is lower). We switched to presenting the change in VAF, which makes better sense because we are considering a scenario where the initial surface is well known but the initial thickness (and hence volume) is less well known.

"4) While some reviewers might say it is too lengthy, I appreciate section 2.1. As some-one who does not rise and sleep in fourier space, it is helpful to understand quan-titatlvely 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|."

These errors have been corrected.

"abstract, l2: influence"

Fixed.

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

We removed the calving part.

"P481, l29: confusing/awkward wording"

Modified.

"P482, l5: dimensional"

Fixed.

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

Fixed.

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

Fixed.

"P490, l10: decreases"

Fixed.

"P491, l12: as high as"

Fixed.

"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"

The sentence has been simplified and supporting references given - we simply say that the strait may be filled by an ice shelf or open water in future.

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

Fixed.

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

TCD
8, C558–C562, 2014