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

S. Sun et al.

sainansun1985@sina.com

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We thank referee 3 for an insightful review, and think we are able to respond properly to the comments

"As already done by the two other reviewers, I questioned the ability of the L1L2 model to feel correctly the smallest (about 1 km) length scales undulations added to your bedrock for the highest frequency experiments. Even though all your experiments give you an indication on the sensitivity to errors I don't think that you can be too much affirmative in your conclusion saying that "low frequency noise on the bedrock plays a more important role on the ice sheet retreat than high frequency noise". It is true but only in the scope of your L1L2 model, which is also a nice result in itself. To give more weight to your study and be able to extrapolate to the reality, I think the proposition

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Interactive Discussion
Discussion Paper

of Dan Goldberg to perform full-Stokes experiments along a flowline for the highest frequency noise is to be done."

This comment obviously adds weight to Dr Goldberg's request – we have complied and for convenience repeat our response to him below. 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 suggested by referee 2. We added an appendix describing these supporting results.

"I would like to see more precisions about how you build your initial state, because there is a lack of details in the text. First of all, did you infer your basal friction for each modified bedrock. I don't think it is stated in the text. If you did not, then that would be an issue to me because it would change the variability of your results. Some uncertainties are rather large, it can modify your bedrock by hundreds of meters so it must be taken into account. If you did, you should make it clear in the text. Also, once you have added your noise to the bedrock, do you perform a relaxation or do you start your 200 years prognostics directly? Again, your bedrock may change substantially after adding the noise, and could make large differences already at the beginning of the prognostic simulation (if you didn't make any relaxation) that could also explain the final variability coming from the lowest frequency noise." It is correct to say that our

TCD
8, C715–C720, 2014

bedrock and ice thickness is quite different at that start of each run. We don't think a relaxation would be the correct approach, though. The experiments as they stand start from a constant surface and velocity (insofar as the inversion is successful) and a perturbed thickness, which reflect the idea that the velocity and surface are well known and the bedrock is less well known. A relaxation would lead us to states where all three would be different for each experiment. That is not to say that relaxation is never the correct approach, if we were looking at melt rate variability it would be better to start from a set of (nearly) steady states for example. We have described this more clearly in the revised text and we have switched to plots of the change in VAF, so the spread of results shown is due to dynamical response to the initial state, rather than differences in the initial state.

SPECIFIC COMMENTS "p479 l2: Influences instead of influence" Fixed.

"p481 l5: Using the Shepherd reference it should be -71 +/- 53 * 20 = -1420 +/- 1060 instead of 1350 +/- 1010 (20 years instead of 19)" Fixed.

"p481 p1: In that first paragraph you seems to focus already on very specific process such as the influence of calving and melting on ice sheet dynamics. I think that you gave too much space for those processes (also in p3 of the same page) compared to how much you study them." We shortened our description in response to this fair point.

"p481 l28: This sentence is not well written to me. The reverse sloping bedrock actually leads to positive feedback, which is modulated by the buttressing coming from the sides. This modulation can then be large enough to stop the retreat of the grounding line in case of a previous retreat. I don't feel like your sentence is reflecting that idea, could you rephrase?" We rephrased the paragraph.

"p482 p1: In this paragraph, I understand more or less that you want to discuss the influence of bedrock uncertainties that have been investigated previously using numerical models. However, it does not appear logical nor clear to me. Could you rewrite it?" We rewrote the paragraph.

TCD
8, C715–C720, 2014

“p484 l2: In the work made in Durand et al, they also introduced noise in their bedrock. In their second set of experiments they randomly modified an ideal bedrock at different length scales. Therefore I find that sentence not correct and would like it to be rewritten.” Fixed.

“p484 l6: I didn’t quite understand what is red noise. Is it related to the sentence in l7-l8? Could you clarify it?” Red noise (as distinct from white noise) has a power spectrum that decays with frequency / wavenumber. We added a note to the text.

“p486 l16: Does it mean that a grounded point remains grounded after adding the noise? Could you confirm?” Yes. We have clarified this.

“p487 l14: Here you talk about numerics, but is there a relationship between the rate of retreat and the computation time for each iteration. That is just about curiosity, you don’t need to discuss it in the text.” Faster retreat rates are associated with faster flow as a rule, so we need to take more time steps to satisfy the CFL condition, but the relationship is not especially strong.

“p487 l18: Do you perform an inversion of your basal friction for each modified bedrock? If not I think that is an issue that would affect the relevance of your results, if yes, I think that should at least be mentioned.” We have performed an inversion of basal friction for each modified bedrock. Now we clarify this in the text.

“p489 l3: That would be worth explaining in the text why the retreat accelerates from 0.1 mm/yr to 0.25 mm/yr equivalent. That would help the reader to understand the processes that are involved in the ice sheet retreat even if it is not the main point of the paper.” We agree and have note that the retreat at accelerates as the grounding line retreats into a deeper bed – as seen in Favier et al., 2014 and anticipated by Schoof 2007.

“p489 l11: Is there any reason here to use the simple future while you have been using present tenses all over the paper?” This was a grammar error and has been corrected.

TCD
8, C715–C720, 2014

“P489 l26: Here, you must talk about the fastest part of LA but it is not written, could you write it down?” Fixed.

“P489 l27: "since after we added the noise, some floating areas become grounded": here you talk about the initial step. Is that the only reason of having a higher VAF compared to the noiseless control experiment? For example for the highest frequency experiments, it seems to me that the initial difference in the VAF between noiseless and noisy experiments increases along time. Could you make a comment on that? There is another thing here: it looks like the highest frequency noise has more influence than the lowest frequency compared to the noiseless result, which was the opposite for PIG. Could you comment on that on the text? In a general way I think that section on the LA system poorly commented in the paper.” Having plotted differences in VAF, it is even more clear that LA does not have the same kind of sensitivity to frequency as the other two basins. It does not retreat, and the changes in VAF are much smaller. We have rewritten the discussion of the LA results.

“p489 l28: Once you have defined VAF as the Volume Above Flotation, you should use it for every occurrence.” Fixed.

“Fig 1: - "topg" is not really a common notation, could you use something like "Bed elevation" instead. - Could you modify your boxes to make them consistent with the areas shown in Fig 2?” Fixed.

“Fig 4: last line: "across".” Fixed.

“Fig 5: Could you increase the quality of your plots and also the legend?” The plots have been re-done using better quality vector graphics.

“Fig 6: - Could you be consistent with you notation of mm/y, it is mm y-1 in the rest of the text - Here there are three accelerations in the increase of the SLR, at 50, 80 and 170 years or so. I think we can figure out the first one in Fig 5, but I hardly see the two others in Fig 5. Do you have an explanation for this? I don’t think you should put it

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Interactive Discussion
Discussion Paper

in the text but I would like to know the reason. Maybe it is too slow acceleration to be seen in the plot...” We have corrected the plot labels. It is hard to make out the second or third accelerations in the plots of VAF(t), but they can be made out on examination – they are just much smaller than the first.

“Fig 7 : - I don’t understand why the ice velocity is so low in the main ice stream once you have passed the initial grounding line. If I read well the scale, it is around 600 m/a there whereas upstream the ice velocity reaches 2400 m/a for an ice that must be way thicker than at the front. Could you add some precisions about it in the text?” The downstream ice is thin – it does not affect the fast upstream ice strongly, and it not affected strongly by it, but is more strongly affected by its contact with the (closer) slow flow at the channel boundaries and the locally low gravitational driving stress. We added a note in the test.

“- Is your velocity maps coming from an average of all your simulations in the same type of frequency? For the highest frequency experiments, the ice velocity variability between experiments should be very little, but is that also the case for the lowest frequency experiments? Could you add some precisions about it?” The speed in the original maps were from the noiseless experiment – and so the the same in each figure. We have changed these for plots of the difference in speed between the fastest and slowest retreating simulations for each case.

“Fig 10 : Could you increase the quality of the plots and the legend?” Yes, better quality vector graphics are used.

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

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Discussion Paper