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The Cryosphere Discussions

Interactive comment on "Ice-stream response to ocean tides and the form of the basal sliding law" *by* G. H. Gudmundsson

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Dear Editor

I thank the reviewers for very carefull and helpful reviews. The new version of the manuscript has benifitted significantly from comments and suggestions made by the reviwers and I thank them for their work.

Answers to review by M Truffer

Question: Why use a visco-elastic model, and how important is the use of that model.

Answer: I use a visco-elastic rheology because it is the most appropriate rheological model to use for the loading periods of interest. I'm not sure if a purely viscous model will produce the long-term tidal variation observed. I guess it will because it is really the viscosity of the till that gives rise to this effect. However, the response will be significantly different. For example, the model would not produce the marked differences in response to the S2/M2 as compared to the O1/K1 forcing.

Question: How sensitive are the results to m

Answer: I realised that this important issue was not addressed sufficiently in the manuscript. I've now done a number of additional runs to answer the question and included an additional figure showing the response at a fixed location upstream from the grounding line for varies values of m (2, 4, 8, 8, and 10).

Question: Is the assumption of ignoring the inertial terms valid in a visco-elastic model?

Answer: In this particular case it is. The vertical acceleration of the floating part due to tidal action is on the order of a wˆ2 sin(wt) where a=2m and w=2pi/0.5 dˆ{-1} for the semi-diurnal tides, ie about 1.4e-8 m/sˆ2at the most. This is much smaller than the gravitational acceleration (9.8 m/sˆ2). So the inertia terms are very small compared to the body force term.

Question: More details about he flow model needed. What about a figure of the model domain?

Answer: I did not add a figure of the model domain as I think the Schematic in Fig. 3 shows the model setup in some detail. However, I've expanded the description of the model considerably. I've added a new sub-section where I give information about he type and size of the elements used in the FE discretisation as suggested.

-Minor points: Reference for Eqs. 5 and 7 added.

-one instance of 'shear modulus' replaced with 'bulk modulus'. I thank the reviewer for spotting this!

-added a reference to Schluson and Duval as suggested.

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-I agree with the reviewer that the term ‘floating condition’ could be misinterpreted. I’ve had discussions with the other modellers here at BAS as what the term ‘floating condition’ implies. And we could not agree! So I’ve decided to simply talk about the ‘stress condition along the ice-ocean interface’.

-A number of other minors points related to spelling corrected.

Answers to review by anonymous reviewer.

Question: Can an experiment be done to show how the net overall discharge of the ice stream varies with m and the amplitude of the tidal beat amplitude.

Answer: This experiment can be done, and I’ve include a new figure that goes somewhat towards addressing this issue. In the new figure flow velocity at a given site is shown as a function of m.

Question: How important are visco elastic effects for the model output.

Answer: The inclusion of visco elasticity affects the phase shifts, among other things. Without visco-elasticity the phase relation would not change with distance upstream from the grounding line. I’ve added one paragraph addressing this aspect of the model. I have not done a run using a purely viscous rheology, and it is possible that the long-period variation will also appear in such a model, provide m\=1.

Further issues raised by anonymous reviewer:

-I was not aware that ‘long period tide’ is defined as a tide that is generated by a potential lacking longitudinal variation. I thank the reviewer for point this out. I’ve added a definition of ‘long period’ tide as used in the paper, i.e. simply longer than one day.

-As suggested I’ve made it clear that the variations in flow refer to variations in the horizontal plane.

-I’ve added a sentence making it clear that significant variations are also observed

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downstream of the grounding lien, and I've added two references to such studies.

-rewording of a sentence (10-20% increase) as suggested.

-I found suggestions made by reviewer on how to better explain the importance of these variations very helpful and have shamelessly included some of the suggestion into the paper.

-Added a paragraph explaining that vertical motion of ice shelves reflects the displacement of the ocean surface to tides, and that here the main focus is on horizontal movements upstream from the grounding line.

-I agree with the points made by reviewer that it is in principle possible for the MSf tide on Rutford Ice Stream to results from a linear response to the (very small) MSf ocean tide. But in the numerical model the response if clearly non-linear because the MSf tide is absent from the forcing.

-The 'spring' used in applying the boundary condition along the floating interface does not cause the system to bob up and down with a buoyancy frequency because inertial effects are not included.

-The author suggest an extended discussion and some rearranging of the discussion material. I've added a paragraph on visco-elastic effects but possibly not as much as the reviewer is asking for. These effects are somewhat complicated and I feel discussing them at length is best left for another paper. The main focus of this paper is to establish beyond doubt that string response at the MSf frequency can be found despite that frequency being absent in the forcing. This is a point that I've have speculated about previously but have been unable to demonstrate fully until now.

Interactive comment on The Cryosphere Discuss., 4, 2523, 2010.