The Cryosphere Discuss., 7, C741–C744, 2013
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The Cryosphere Discussions
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Interactive comment on "Influence of high-order mechanics on simulation of glacier response to climate change: insights from Haig Glacier, Canadian Rocky Mountains" *by* S. Adhikari and S. J. Marshall

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Received and published: 5 June 2013

This paper is well written and concise; detailing the results from three different numerical models of a glacier in the Canadian Rockies. Models are forced by temperature and mass balance anomalies in order to see how strongly model physics influences glacier retreat and advance. The model results are presented with a high degree of skill, as the authors manage a partitioning of the resistive forces into longitudinal, transverse, and basal components. From these results, we see basal drag to is the most significant, accounting for 70% of the force budget. I found this particularly interesting because it

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would suggest that higher order stresses are unimportant in the retreat of such alpine glaciers with similar aspect ratios. This is borne out in the paper's conclusions. The paper also reaches interesting conclusions relating to the importance of higher order stresses in glacial advance, and the life expectancy of the Haig glacier. Overall, I found it worthwhile reading with well supported and interesting conclusions. I recommend that the paper be published, with only minor revisions. Some things that are worth serious consideration follow.

1. The use of the SD model is equivalent to a shallow ice (SIA) model. I don't think that it is fair to run SIA on a 25 m mesh, which violates the basic assumptions that lead to the shallow ice equations; ie the thickness to length ratio is not small. Of course SIA performs poorly, it's not at all the right model to be using. We didn't need a run to see this. I would propose that to be fair to poor, old SIA, some averaging over the surface be done. To achieve an thickness to length ratio of about 1:10, you should average surface elevations over about 850 m. On so short a glacier, this could lead to problems with averaging near the boundaries, but I think it's possible with an asymmetric kernel. Were this done, I suspect that SIA will do nearly as well as the other models.

2. The authors are modelers, and everyone likes a little job security. That said, in this paper there is an important case that is missing from the analysis, and that is 'no model'. Why not simply assume that velocities will continue to be similar to what they are now, and see how the time until the glacier disintegrates is changed. I suspect it is not appreciably different to the modeled cases, and that the 'model' used for wasting and retreat is irrelevant, because all of the changes are driven by surface mass balance. This is quite alright in my mind, a paper that shows the community the easiest way to correctly model alpine glacier demise is likely to be much more important than a paper that teases out subtle difference in the model output from different momentum balance approximations. This also takes us back to point 1, above. If we can get away with just SIA on alpine glaciers, we should.

3. The assumption of a no slip basal boundary may not be entirely justified. While

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observed velocities are small, I don't think that we can know whether it is deformation flow, or sliding taking place during a short period during the spring when subglacial water pressures are high due to the lack of an efficient sub-glacial drainage system. This isn't something that the authors can do much about, other than to acknowledge the possibility, and that if true, the stress balance would be significantly different, depending on the time of year. This would also tend to decrease A, and that will influence the stress balance as well.

Some minor comments that might improve the readability follow:

p1708 line 26 "tremendous" seems to overstate things.

p1709 I'm not sure if we'll ever have "full understanding..." in the sense that a predictive framework will be developed. There are too many non-linear couplings and stochastic variables. But that's pessimistic. What I am pretty sure of is that continuing to add more and more components to Earth system's models is just giving us a greater ability to overfit what precious little data we do have, and likely to contribute to less understanding. These are philosophical matters, I don't really care what you say, but you should know that some of us disagree rather stridently.

p1715: The manuscript would benefit from error estimates in the measurements of velocity.

p1716 Some readers might benefit from having the ,j subscript defined for them as derivative wrt j.

p1719 I like the idea of calling the SD model the SIA model, because that's what it is, and that's what most of us are used to seeing it called.

p 1719 I don't know what is meant by 'semi-structured' mesh.

p 1720 a 5m layer of fictitious ice is pretty thick for a glacier with average thicknesses of 85 m. Justify this decision...

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p 1722 line 22: I don't know what 'realistic' means here.

Interactive comment on The Cryosphere Discuss., 7, 1707, 2013.