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The Cryosphere Discussions

Interactive comment on "Applicability of the Shallow Ice Approximation inferred from model inter-comparison using various glacier geometries" *by* M. Schäfer et al.

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At first sight I was enthousiastic about this paper because model intercomparisons are an essential ingredient of our science. However, after reading it more carefully a lot of issues came up that I do not understand or that I find misleading. My main points are the following.

* INITIALISATION An approach in which an ice body of an arbitrarily shape is taken to calculate a velocity field is not logical. Existing glaciers are the result of an equilibration process imposed by physical laws, and therefore only a very small number of all possible shapes is meaningful. Because in all models of viscous flow velocities depend

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on high powers of slopes and thicknesses, the shape of a body should not be chosen freely. Meteorologists and oceanographers are very much aware of this. A proper initialisation procedure of a particular state is crucial for a succesful integration with a model. One cannot just put a bump on the ocean, do some calculations, and conclude that current ocean models are inadequate. Here glacialogists cannot escape, in spite of the fact that we can neglect inertial terms. If we take a map of a real glacier and use that, without any smoothing/further preparation, to calculate a velocity field, we will not get a good match with an observed velocity field whatever model is used. Against this background, it is meaningless to calculate ice velocities with the SIA for an arbitrarily shaped body and then state that the values are wrong. What is wrong? The selected shape of the glacier is wrong!

* SLIDING There is a simple statement in the paper that sliding is not considered. The authors state that they have done a few calculations with sliding, but they do not explain how and they do not show any results. They merely state briefly that it seems that the SIA performs better when sliding is included. For me this nonchalant attitude is very hard to understand. 99% of all glaciers slide, and for perhaps half of them sliding makes a larger contribution to the ice discharge than deformation. This simple fact makes the conclusions of this paper rather irrelevant.

* LONGITUDINAL COUPLING The authors state correctly that the SIA implies that the local ice velocity is determined by the local slope and thickness, and not by the stress field up- and downstream. But they also write:...."without including any interactions from the neighbouring grid points". This is not correct. In numerical models based on the SIA, the interaction operates through the divergence of the mass flux which effects the ice thickness up- and downstream and thereby the local slope (I guess this is what the authors call "negative feedback" on page 582).

* HORIZONTAL SHEAR STRESS In the Conclusions the authors state that neglecting horizontal shear stresses is another problem with the SIA. Well, as far as I know, in the glacier models (flow-line models) in which the SIA has been applied in the past

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the basic assumption is that the cross-sectional mean velocity can be related to the cross-sectional mean slope and thickness. The importance of side drag has been discussed a long time ago by Nye, and he suggested that it can be accommodated by introducing a shape factor in the force balance for the cross-sectional slab. Actually, that is what many people working with the SIA for glaciers have done. By the way, the bending of Forbes bands indeed implies that the horizontal velocity towards the glacier margin decreases, but it does not prove that this is due to horizontal shear stresses (although they are likely to play a role). A decreasing ice thickness towards the margin would have the same effect even with the SIA! This may for instance be the case for Svinafellsjokull: http://www.swisseduc.ch/glaciers/glossary/ogives-svinafellsjoekull-en.html The real issue, and difficulty, is to determine the side drag as a boundary condition for either a simple or a more sophisticated model.

* WHEN WILL REAL GLACIERS BE CONSIDERED? The final sentence of this paper reads: "As a next step, it would be interesting to make some more comparisons on prognostic simulations, not only by comparing final velocities and surface, but by comparing the dynamics over the entire simulation time". Fine, but when will real glaciers be considered? I think THAT should be the next step, and it should in fact be done in this paper! A real model test would be to see if the observed fluctuations of a couple of glaciers over the last 150 years or so can be simulated better with a higher-order/full Stokes model than with the SIA. Why not take e.g. Hintereisferner and Kesselwandferner, having very different geometric characteristics, well-known length histories, a set of measured surface profiles at different times, and almost identical climate forcing? Or even Saint-Sorlin, although one may wonder if this is a really suitable glacier for this purpose. To put it a bit nasty, the paper shows that a simplified set of equations may give results that differ significantly from a more complete set of equations, depending on the chosen configuration. So what?

* GENERAL COMMENT I suppose that the goal of glacier modelling is to be able to simulate the past behaviour of glaciers and to make projections into the future for

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certain scenarios of climate change. Here a lot of steps are involved, all introducing uncertainty. A major problem is relating the glacier mass balance to the large scale meteorological/ climatological conditions. Glaciers are highly damped systems driven by continuous mass throughput. To determine the size of a glacier for given mass-balance conditions is first of all a geometric exercise, not an ice-mechanical one. The feedback of the glacier shape on the mass budget operates through the relation between mean surface elevation and mass budget, and that is where ice mechanics comes in. However, this feedback is only important when bed slopes are small, making the SIA adequate to estimate the relation between mean glacier thickness and glacier size. What I want to say with this is that "scaling" of the climate-glacier issue is much, much more than scaling the equations governing the mechanics of ice deformation.

* Finally, I am not sure that referencing in this paper does do justice to the historical development of the subject.

Interactive comment on The Cryosphere Discuss., 2, 557, 2008.