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The Cryosphere Discussions

Interactive comment on "Modelling historical and recent mass loss of McCall Glacier, Alaska, USA" *by* C. Delcourt et al.

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1. I do not understand why in this work the 3-dimensional geometry is not taken into account. No matter how the mechanics are treated, conservation of mass requires that the 3-d geometry is dealt with. This can easily be done in a flowline model by making the area of the cross section a function of x (distance along the flowline) and ice thickness. It would make the model much more realistic. For instance, it allows a model glacier to become narrower when it retreats, thereby reducing the area of the ablation zone. Numerous studies have shown that including a parameterisation of the geometry of the cross section works, and that one may even combine flowlines to describe the effect of tributary glaciers (basins). If the changing hypsometry is not taken into account, which seems to be the case in this study, conclusions about the

sensitivity of the glacier to climate change can simply not be made. At some point the authors state that 3-d effects are taken into account by using a shape factor in the force balance. This is an entirely different issue. As far as I know the concept of a shape factor was introduced long ago by Nye to account for the effect of side drag on the mean velocity in a cross section. The shape factor therefore depends first of all on the width-thickness ratio of a glacier. It does not interfere with the continuity equation.

2. In the beginning of the paper the authors state that they have also done the experiments with a model based on the shallow ice approximation (SIA). However, they never really come back to this. To me it seems very interesting to show a comparison. Some glaciologists claim that the SIA approximation should not be used for glacier modelling studies. However, there is very little evidence that higher-order models do a better job in simulating the observed thickness and velocity profiles of typical valley glaciers. Perhaps the authors could make an interesting contribution to this discussion based on their calculations.

3. To me it seems that the approach taken in this paper is not very balanced. A detailed calculation of the 2-d (vertical plane) stress, velocity and temperature fields, is combined with a simple sliding law in which water pressure is not considered, and with a representation of the geometry that is inadequate. Could the authors demonstrate what is gained here by using a higher-order model here?

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