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The Cryosphere
Discussions

Interactive comment on "Present dynamics and future prognosis of a slowly surging glacier" *by* G. E. Flowers et al.

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General comments

I enjoyed reading this manuscript. It is well written, and it presents new valuable insights on the glacial surging phenomenon. By modelling observed flow velocities, the authors find that high basal melt water pressure under the central regions of the study glacier is a likely reason for its present 'slow surge' mode. The model results presented also demonstrate convincingly that the glacier is presently in a transient mode, and that steady-state situations are likely to have thicker ice in a reservoir bounded by a bedrock ridge – even under warmer climate conditions. I think the latter is particularly interesting, as it provides new insights into the influence of bed topography on the surge phenomenon.

I have some specific comments and suggestions that I think could make the manuscript even better. However, they are mostly minor comments, and they should not hamper the publication of this manuscript in any serious way.

Specific comments

1. The use of a regularized Coulomb friction basal criterion is part of the novelty of this paper. I think that this new and physically sound approach is one of the papers strengths. The results of the regularized Coulomb criterion are used for suggesting variations on basal water pressure. However, much is still unknown about sub-glacial dynamics, and there is room for suggesting various models. When reading this, one cannot help wonder how big a difference the regularized Coulomb criterion makes when comparing, for example, with the 'standard' empirically derived sliding law (e.g. u_b=k*tbˆn/N). Can the velocity observations more easily be explained with the regularized Coulomb criterion? Or does it make it more difficult? Does the choice of sliding relation have a high impact on the patterns of water pressure needed for explaining the observations? To this end it would be good to see calculations of basal slip rates when adopting also the 'usual' empirical relation u_b=A*t_bˆn/N. At least I think the authors should plot the values of basal shear and normal stress, which then would allow the reader to evaluate the influence of different sliding models qualitatively.

2. A higher order flow band model is used in this study. It means, for example, that longitudinal and to some extent transverse stress gradients are considered. However, by making two assumptions the model presented ignores other higher-order terms that could potentially be important for alpine glaciers. I think one or two sentences on the validity of these two specific assumptions would strengthen the model. The first assumption is that sigma_zz is hydrostatic (eqn. 3), and the second assumption is that dv/dx=0. Regarding the first assumption, the authors could perhaps estimate the magnitude of bridging effects ignored by this assumption (see e.g. Pattyn 2002). And for the second assumption, one would expect the vertical velocity and its horizontal gradient to be non-zero in this alpine setting with apparently high slip rates. I do not suggest

that the authors redo the model study. I would just like to see estimates of 'order of magnitudes'. I suspect that these assumptions do not interfere with the conclusions of this paper – but I think including some reflections on this matter would strengthen the paper.

3. Almost all of the prognostic simulations show thickening of ice above the bed ridge. The question is, however, to which degree the two-dimensional flow band model overestimates the effect of the three-dimensional bedrock ridge. Will three-dimensional models show the effect to the same extend? The authors demonstrate with three automatically generated flowlines, that much of the ice flux passes through the overdeepening above the ridge. I think the authors could extend this line of argumentation in favour of their model approach, perhaps by generating even more flowlines.

Technical comments

1. I think the mixing of two different notations regarding vector and tensor indices complicates the model description in section 4 somewhat. In most equations the authors use xx, yy, xy, xz... etc. for the tensor components referring to the coordinate axis labels. In other relations they use i and j, which usually refers to the numbers of the coordinate axis. It is a bit confusing and not particularly elegant. It is perhaps a small thing, but eqn. 4, for example, could also simply be written sigma'_ij=sigma_ij-sigma_ii/3 when adopting standard index notation.

2. Mostly I think the issue above confused me because the coordinate axis numbering is also a bit unusual. From u=(u,v,0) I gather that the x axis is number 1, the z axis number 2, and the y axis number 3 ? Usually, the ordering is (x,y,z), but this would zero the vertical velocity, which cannot be true. I think some clarifying sentences on this would improve the reading. Also, the authors should consider not using the same letter u for the velocity vector and for its x-component. Why not adhere to the index notation and say u=(u_1,u_2,u_3)? Or if labels are preferred u=(u_x,u_y,u_z)?

3. I may be wrong, but is there a sign error in eqn. 10? The sign does not seem to fit

with the sign convention used in eqn. 9.

4. I think it is best to consistently refer to the basal boundary condition as a regularized Coulomb condition as the authors do in the first part of the paper but stop doing in the latter part. In my mind, a strict Coulomb condition only applies to contact surfaces, and it does not depend on sliding rates.

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