Response to Reviewer David Egholm

We appreciate the constructive comments of both reviewers and have responded to each comment, copied verbatim, below. Our responses are italicized.

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.

We agree and are very keen to undertake a study that compares the classical sliding law and the friction law implemented in this model. We have already begun a follow-up study to the one reported in this paper that will include such a comparison, but using surface velocities calculated from sub-daily GPS solutions over three melt seasons, rather than just mean summer and annual velocities from stake surveys. This should allow us to examine the differences between the classical sliding law and the friction law for basal motion on a variety of time scales. Our suspicion is that the differences will be amplified on shorter timescales, and thus our sub-daily GPS data will provide a more useful *test of the two models than the data presented here. We would therefore like to postpone the implementation of this suggestion to include as part of our future work.*

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.

We have added text to address the first assumption in the model description: "In making the hydrostatic assumption, vertical resistive stresses and therefore bridging effects are neglected \citep[e.g.][]{Pattyn_2002}. Models that include this term are termed LTSML, rather than LMLa. Bridging is significant over short spatial scales and, for example, in icefalls \citep{vanderVeen_Whillans_1989}. Bridging may be non-negligible over several steep sections in the glacier surface profile (Figure~\ref{fig:geometry}b) and where basal slip conditions change over short distances."

We make the assumption dv/dx=0 in our lateral stress parameterization, where v is the transverse horizontal velocity. We think the notational confusion led the reviewer to believe that we are assuming dw/dx=0 instead. With the hydrostatic assumption (following Blatter), the vertical velocity (w) does not feature in the model equations because the vertical normal stress is hydrostatic. A model that employs the LTSML approximation (as opposed to LMLa used here) would include vertical resistive stresses but neglect horizontal gradients of the vertical velocity. In corrections and changes to our notation (see further below), we have made clear that we assume dv/dx = 0 rather than dw/dx = 0.

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.

We created many flow lines when we first set up the model, and found that any flowline that is a plausible centerline in the ablation area gets pinched through the narrow zone where you see the three flowlines in Figure 2 traverse the ridge. We have used the same automated flowline generating *algorithm to illustrate the continuity of the ridge, or some expression thereof, in additional profiles (please see figures at the end of this document). The first set of flowlines begins at nearly equal elevation in the upper basin; the bed profiles along these flowlines all exhibit a prominent bedrock ridge, in some cases more prominent than beneath the flowline we modelled. The second set of flowlines attempts to sample the ridge more broadly, with flowlines starting at a lower elevation than in the first set. Bedrock ridges can be identified in nearly all of these profiles, but the profiles illustrate the reduced amplitude of the ridge toward the glacier-left margin (see also Figure 2a). We have chosen not to include these additional profiles in Figure 2, as this would probably obscure what is already there.*

We responded to a similar comment, addressing the exaggerated effect of the ridge in a 2-D model, from the other reviewer as follows: "As for 2-D versus 3-D effects: we agree that the reservoir development is exaggerated in the 2-D case, however we do not believe that it is an artefact of the flowband model (and would thus be absent from a 3-D model) for the following reasons. First, although the "bump" is highest on one side of the glacier, it is part of a bedrock ridge that is continuous beneath the glacier from one side of the valley to the other; any longitudinal profile one would extract through this area would contain an overdeepening and a ridge. Second, the valley bends and narrows near the subglacial ridge, providing further resistance to flow through this cross-section. The combined basal drag (from the bedrock ridge that extends across the glacier) and lateral drag (from the narrow valley walls) would cause thickening of the ice in this region even in a 3-D model. The extent to which this thickening would persist under various mass balance conditions according to a 3-D model would have to be determined by further study. We have tried to address the points above by revising the abstract and the discussion (both sections 6.1: Model simplifications and limitations, and 6.2:Iinterpretation of model results)."

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.

We have changed Equation 4 as suggested: σ'_ij = σ _ij +1/3 δ_ij σ_ii, and have converted all of our coordinate axis labels to numbers from letters. Our original notation generally follows what we used in the original paper describing the model (Pimentel et al., JGR, 2010), but we hope that the changes here have made the model presentation more clear and consistent.

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)?

We have corrected this error in the definition of the velocity vector (formerly written u=(u,v,0)) that caused confusion. We are, in fact, using conventional ordering for the coordinate axes and have rewritten u as suggested: (u_1, u_2, u_3).

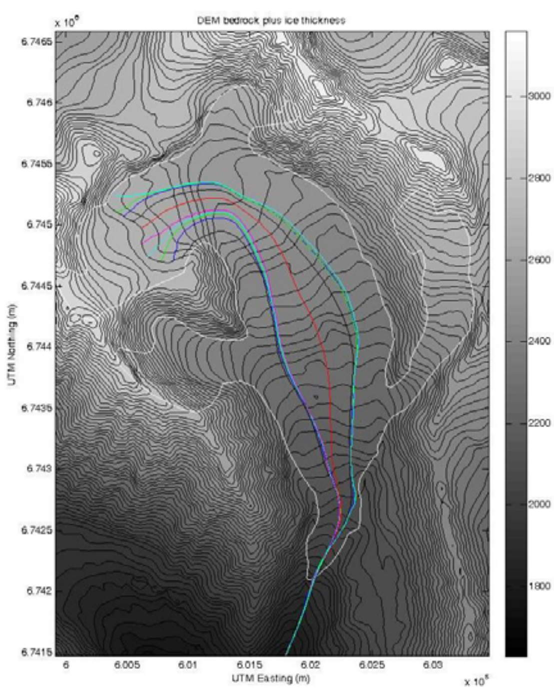
3. I may be wrong, but is there a sign error in eqn. 10? The sign does not seem to fit with the sign convention used in eqn. 9.

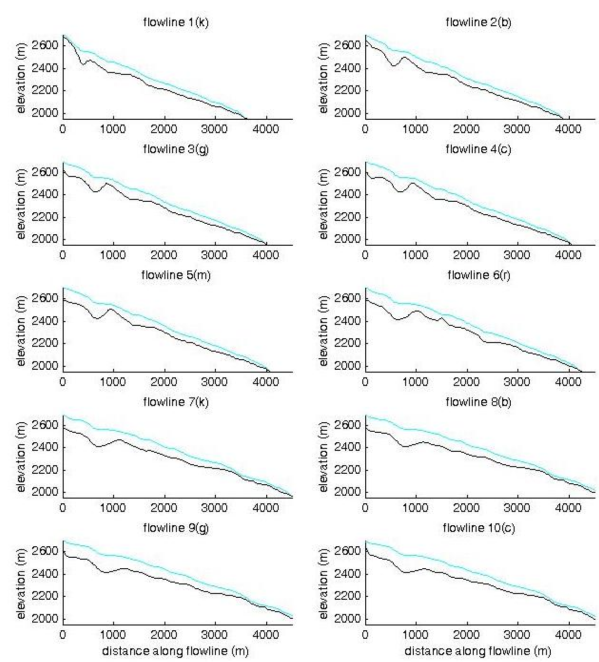
Thank you for pointing this out. In Equation (9) we are really approximating ∂u/∂y as +/-u/W (longitudinal velocity diminishes away from the centerline), where the sign depends on what side of the flowline is being considered. Our parameterization of the lateral shear stress should therefore be written +/–νu/W for consistency, and we have changed this in Equation (9). We had written this approximation with only a negative sign before. The way we have written the momentum balance (Equation 5) requires that F_lat be negative (with contributions from both valley walls), so Equation 10 retains its negative sign.

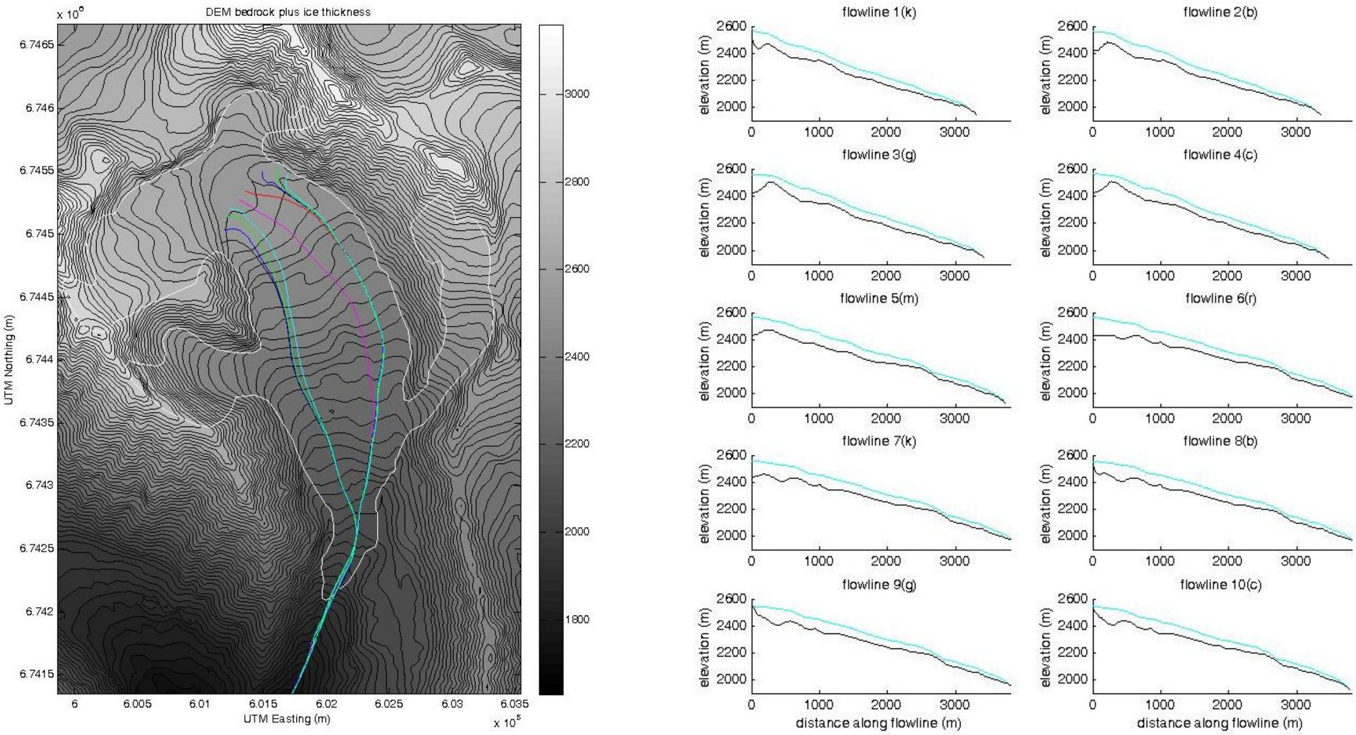
4. I think it is best to consistently refer to the basal boundary condition as a regularized Coulomb condition as the authors do in the first part of the paper but stop doing in the latter part. In my mind, a strict Coulomb condition only applies to contact surfaces, and it does not depend on sliding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

We have edited the text to consistently refer to a "regularized Coulomb friction law" or sometimes simply a "friction law", and have omitted occurrences of "Coulomb friction law" without the "regularized" modifier.

Flowline figures:







Flowline profiles plotted at right in both sets of figures are numbered consecutively 1-10. Profile 1 begins near the glacier-right margin and is black.