The Cryosphere Discuss., 4, C1064–C1068, 2010
www.the-cryosphere-discuss.net/4/C1064/2010/

The Cryosphere Discussions

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

Anonymous Referee #1

Received and published: 15 November 2010

TCD-2010-068 *"Present dynamics and future prognosis of a slowly surging glacier"* - **Flowers, Roux and Pimentel**

This paper presents diagnostic and prognostic simulations of a small surging valley glacier in the Yukon Territory, using a flowband higher-order model. The novelty in this paper is the use of a complex friction law which is function of the basal water pressure. From diagnostic simulations, the distribution of the basal effective pressure is inferred by a trial and error method on the modeled and measured surface velocities. Then, prognostic simulations for various mass balance scenario are performed, and allow the

authors to investigate the link between mass balance and the propensity of the glacier to surge. This paper is well written and contains sufficient material to be published. I have few main major comments that should be answer before publication and few minor comments (see below).

Major Remarks:

The definition of the effective pressure is not given (should be given page 1850, line 4). In the literature, the effective pressure is often defined using the isotropic ice pressure (noted P_i here), but using a higher-order it should be defined using the Cauchy stress normal to the bedrock surface ($\sigma_{nn} = n\sigma \cdot n$). So, define how you evaluate the effective pressure and, if you are using the isotropic pressure instead of the normal stress, justify your choice.

page 1863, paragraph 5.1.3: Why did you choose to use a new parameter μ to define how important is the water pressure relative to the ice normal stress? You should notice that N and μ are trivially linked as $N/P_i = 1 - \mu$. Then, the analysis is conducted using this parameter μ which takes four different values along the flowline. I think the analysis should be more pertinent by adding a plot of the evolution of the basal normal stress along the flowline. Due to the bedrock topography, I expect that σ_{nn} will be larger just upstream the prominent bed ridge (located between zone 1 and zone 2, in $x \approx 1550$ m), and will be smaller just downstream the ridge. The effect, is that for a uniform water pressure, the effective pressure N will decrease just downstream the ridge (or as you obtained, μ increases just after the prominent ridge). In other words, the variations in μ might be only the result of variations of the ice normal stress, and not the water pressure. This should be interesting to separate what is due to changes in ice normal stress (induced by topography changes) and what is due to changes in water pressure. From the plot of μ alone, one cannot deduce the variation of basal water pressure and all the discussion is done as if only the water pressure was evolving along the flowline. I would then suggest to plot in Figure 6 the ice normal stress (given by the model) and the range of water pressures consistent with the observations (instead of μ).

5.2 prognostic simulations: I have to admit that I didn't see clearly how the simulations that are performed in this section can be linked to the study of the glacier surge. The main point is that these prognostic simulations are performed assuming a basal water pressure set to zero and for very long duration (280 years) in comparison to the observed quiescent phase duration. Moreover, a large part of the modeled thickening upstream the bedrock bump certainly results from the flowline model assumption. Using a 3D model would certainly reduce this effect as the bedrock bump (as can be seen in Figure 2) is clearly a 3D feature, and is not elongated in the transverse direction (even if one can see the bump in the three different flow lines, which are very close to each other in this area). The various mass balance tested should be plotted in Figure 3. For example, how the zero net mass balance compares with the 2007 surface mass balance? In the model, the mass balance is assumed as a function of the distance along a flow line (what is obtained from the measurements) whereas the surface mass balance is function of the surface elevation, introducing feedback that are not accounted for here. This point should be discussed.

Minor Remarks:

page 1842, line 5: could you quantify the differences in term of surface velocity

of a normal surge and the current surge?

page 1848, line 7: $P_i = \sigma'_{xx} + \sigma'_{yy} - \rho g(z_s - z)$ instead of $P_i = \sigma'_{xx} + \sigma'_{yy} - \rho g(z_s)$

page 1849, line 3: In the reduction of the model to two dimensions, following Nye (1959), we have taken

Equation (15): I think the equation is not correct. Gagliardini and others (2007) showed that, in the non-linear case, $\tau_b/(CN)$ is a function of $u_b/(C^n N^n)$ whereas the expression proposed by Schoof (2005) is a function of u_b/N^n (it was extended heuristically from the linear case to the non-linear one). The adopted friction law should write:

$$\tau_b = C \left(\frac{u_b}{u_b + C^n N^n \Lambda} \right)^{1/n}$$

Since C^n is a constant in this application, this will just change the numerical value inferred for Λ by a factor $1/(0.5 \times 0.84)^3$. In the case of a non-uniform bedrock roughness, this would have more effect.

page 1850, line 5: It should be mentioned that the adopted relation $C = 0.84 m_{\max}$ has been obtained in the particular case of a sinusoidal bedrock. What is known for sure is that for a real bedrock $C \leq m_{\max}$.

page 1852, line 20: of the flow line can be seen as minimum estimates (?)

page 1855, line 16: why this value of 280 years whereas line 2 of page 1856, it is said that the profile are steady state profiles?

Figure 1a and b should be larger.

Figure 3: the zero-net mass balance profile should be plotted in this graph.

Figure 5: the curves for $m_{\max} = 0.5$ and $\lambda_{\max} = 1$ should be emphasized (continuous bold).