Response to Anonymous Referee #1

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

Anonymous Referee #1 Comments

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 Pi 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.

This comment brought to our attention an important detail regarding the definition of effective pressure in higher-order models. In our implementation, we followed the extensive literature that defines effective pressure as $N = P_{i} - P_{w}$, where typically within the literature P_{i} is considered hydrostatic; however, within the Blatter approximation used here, $P_{i} = \sigma'_{xx} + \sigma'_{yy} - \rho g H$ and this is what we have used. The reviewer raises the point that actually the more correct way to view effective pressure is as the normal stress to the bedrock surface minus the water pressure. If we took this approach then within the Blatter approximation, normal stress at the bed is considered hydrostatic (this is an order epsilon^2 error, where epsilon is the aspect ratio). Therefore with hindsight, to remain entirely consistent with the Blatter model, we should have used $N = \rho g H - P_{w}$ as the definition of effective pressure rather than $N = P_{i} – P_{w}$. Whether our use of P_{i} as calculated within the Blatter model, as opposed to simply $\rho g H$ in the definition of effective stress, is sufficient grounds to warrant repeating the study is a question we leave to the editor. We certainly plan to revise the model so that $N = \rho g H – Pw$ for future work. One of our plans for future research is to apply a full Stokes model to our study glacier, in which case we will have an opportunity to evaluate the effects of the approximations made in a Blatter-Pattyn-type model, including the representation of the effective pressure.

For now we have we have added the following to the model description below Equation 15 (the friction law): "We define effective pressure in the conventional way as $N=P_{\rm i} - P_{\rm w}$,

with water pressure $P_{\rm w}$. Taking $N=\rho g h - P_{\rm w}$ would be strictly consistent with the approximations made in this Blatter-Pattyn-type flowband model \citep{Schoof_Hindmarsh_2010}, but would not be appropriate for higher-order models in which the leading-order bed normal stress differs from cryostatic."

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/Pi = 1 − μ. 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 ≈ 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).

Our use of the parameter μ made the presentation unintentionally confusing. Part of this confusion is due to μ having been defined improperly in the text as Pw/Pi, and to the inaccurate statement that we prescribed effective pressure. We did, in fact, prescribe basal water pressures directly, not values of Pw/Pi in the analysis. Our intention in using the parameter μ was simply to make the results more intuitive by expressing prescribed values of basal water pressure (Pw) as a coefficient multiplied by ρgh , i.e. μ = Pw/ρgh, where ρ is ice density and h is ice thickness. We refer to the coefficient μ as the "flotation fraction". We chose to prescribe uniform flotation fractions (i.e. Pw = $\mu\rho gh$ with constant μ), rather than uniform values of the dimensional water pressure Pw, as we deemed this more realistic for zones of variable ice thickness. The above means that Pi, and therefore N, were still solved for within the model. Thus, water pressure is variable along the flowline and does play a role in setting the modelled velocities. Our use of the phrase "prescribed effective pressure" actually would have been correct, had we formulated effective pressure consistently with the Blatter approximation as N = ρgh – Pw. In this case, prescribing Pw = $\mu\rho gh$ essentially fixes N.

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 simulations were not intended to represent the actual build-up to a surge or a surge itself, because, as the reviewer points out, they were conducted with water pressure set to zero and for a time sufficiently long so as to cover several surge cycles. We reported the length of the simulations (280 and 400 years) merely to indicate the time required for the glacier to attain a new steady state in response to the prescribed mass balance. The prognostic simulations were intended to be related to surges in the following two ways. First, they demonstrated that the glacier is currently in a transient state, being both thinner in its reservoir area and longer than it should be, given its recent mass balance; this corroborates the suggestion that the glacier has been subject to its currently unsustainable flow rates for some time (what we're calling the slow surge). The prognostic simulations also suggest that the bedrock ridge may facilitate the development of an ice reservoir, a pre-requisite to surging. This mechanism, as modelled here, is entirely a function of the glacier geometry, and independent of contrasts in thermal and/or hydrological conditions along the flowline which were not modelled. We suggested that topography (in addition to thermal and hydrological conditions) may contribute to the propensity of a glacier to surge, something that has not been widely discussed before in the literature. We think the ridge (or more generally, the glacier/valley geometry) influences the surge-type character of this glacier not only because the model predicts ice thickening above the ridge, but because the ridge seems to be the upper limit of surging as judging from our own recent measurements and from air/ground photographs of previous (fast) surges.

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: Interpretation of model results).

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.

The mass balance profile corresponding to zero net balance has now been included in Figure 3. The text explains that this curve is shifted up or down (as can now be seen by comparing the two

polynomial profiles in Figure 3) to obtain other mass balance profiles. The reviewer is correct that we have omitted the mass-balance elevation feedback by choosing to express mass balance as a function of the position along the flowline, rather than as a function of elevation.

For the simulations in Figure 8 (steady state profiles in response to prescribed negative balances), neglecting the mass-balance—elevation feedback results in a less pronounced response to the imposed balances. All of the simulations are initiated with the present glacier geometry, hence there would be little difference initially between a mass balance profile prescribed as a function of flowline position versus surface elevation. As the glacier evolves, the mass balance should be reduced in areas of ice thinning and increased in areas of ice thickening, were this feedback included. This means that the amount of thinning in the upper accumulation area and the terminus thinning and retreat would be exaggerated. The bulge above the bedrock ridge would become larger, were the feedback included, as it attains a higher elevation than the initial profile in most cases. Omitting the feedback in these cases makes our simulation results conservative in terms of the geometrical reconfiguration that would accompany a long-term change in mass balance.

For the simulation with zero net balance (Figure 9), the consequences of including the mass-balance—elevation feedback should again be more dramatic. Most of the glacier above the imposed ELA (~2500 m distance along the flowline) thickens without the feedback and would thicken even more with it. Below 2500m along the flowline, the initial phase of terminus retreat would be more pronounced, hence the surface slopes between accumulation and ablation areas would steepen. This would drive faster ice flow, partially offsetting the increased thickening upstream relative to the no-feedback case. Given the extent to which the modelled profiles are higher than the initial profile even below the fixed ELA, it is possible that the mass-balance—elevation feedback would prevent a steady state from being achieved (the zero net balance relationship being defined by the balance-elevation relationship applicable to the present-day glacier profile). We have inserted some discussion of the consequences of omitting the mass-balance—elevation feedback in both subsections of Section 5.2 (Prognostic simulations).

Minor Remarks:

page 1842, line 5: could you quantify the differences in term of surface velocity of a normal surge and the current surge?

This comment referred to the description of the slow surge of Trapridge Glacier, rather than the glacier studied in this paper. However, flow speeds during the slow surges are similar on both glaciers. Text added: "Flow speeds during a typical surge are expected to be 10--100 times those during quiescence \citep{Meier_Post_1969}, whereas the peak annual flow speeds measured during the slow surge of Trapridge Glacier only reached 42\,m\,a^{-1} compared to speeds measured after the surge of less than 10\,m\,a^{-1}."

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

Thank you. This has been corrected.

page 1849, line 3: In the reduction of the model to two dimensions, following Nye

(1959), we have taken

Change made.

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 $ub/(C^n N^n)$ whereas the expression proposed by Schoof (2005) is a function of ub/Nn (it was extended heuristically from the linear case to the non-linear one). The adopted friction law should write:

$\tau b = C \quad (ub/(ub + C^n N^n \Lambda))^{1/n}$

Since Cn 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.

The reviewer is correct here. We should have written the friction law for the non-linear case according to Gagliardini and others (2007), rather than Schoof (2005). For now, we have not repeated all of the simulations with the additional factor of C^n in the denominator, because as the reviewer points out, the net effect is only to alter the value of $\Lambda = A\,\lambda\_max/m\_max$, where A, λ_max and m_max are prescribed values. We have revised the text so that the friction law is written correctly for the non-linear case following Gagliardini et al (2007), and have recalculated the values of λ_max that correspond to our prescribed values of A and m_max for the simulations conducted. Our reference value of λ_max was formerly 1m and now is 13.5m. We have also revised Figure 5 (retaining only former panels c and d), labelling the simulations according to the value of Λ (instead of λ_max and m_max individually) as per the correct formulation of the friction law. We removed the results in former panels a and b, as each simulation in these panels represented combined changed in the values of m_max and λ_max when the friction law is written correctly. We have revised the text in section 5.1.2 (Sensitivity to the Coulomb friction-law parameters) and the related section of the discussion in section 6.1 (Model simplifications and limitations) to reflect these changes for now.

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

Text added: "For real bedrock geometries, $C \leq m_{\rm max}$; here we take $C = 0.84 \, m_{\rm max}$ as derived for a sinusoidal bedrock geometry \citep{Schoof05,Gagliardini07}."

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

Changed to "of the flowline can be interpreted 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?

This is a misunderstanding stemming from some confusing text. The model was run for 280 years in order to allow steady states to be achieved. The text has been changed from "We simulate glacier evolution over 280 years in response to four prescribed values of the global net balance." to "We simulate glacier evolution to a new steady state in response to four prescribed values of the global net balance".

Figure 1a and b should be larger.

Figure 1 (a and b) has been enlarged to its maximum width for the TCD format (14.5 cm). It should be possible to enlarge it to 17.3 cm width in the TC format (two columns). We have also enlarged Figures 2 and 5 to their maximum widths.

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

Done. Legend added and caption adjusted accordingly.

Figure 5: the curves for m_max = 0.5 and λ_max = 1 should be emphasized (continuous bold).

Line attributes have been changed in Figure 5 so that bold continuous lines indicate the reference simulation as suggested.