Authors' response to reviewers report

Response to Reviewer 2

This is an interesting model study of an important process. The model represents an advance on previous treatments of hydrofracture, and has yielded some interesting new insights into the relative importance of thermal and deformational processes in controlling rates of hydrogfracture propagation and supraglacial lake drainage. The anonymous referee has raised a number of technical points regarding the paper, and I shall not repeat them here.

...

These comments aside, I like this paper, and commend the authors on some interesting work. With the addition of a bit more context (and subject to the technical issues raised by the other referee), I recommend publication.

We thank the reviewer for his comments and positive recommendation. Below, have addressed the points raised by the anonymous reviewer 1, in our the response above. Changes to the manuscript are in blue within the manuscript.

Comment # 1

1) With regard to terminology, I agree that the term 'visco-plastic' is not appropriate and should be changed to "viscous" or "non-linear viscous" throughout. It is also unnecessary to refer to the "so-called Glen's flow law" (line 126). Although some have used other names for this flow law from time to time, "Glen's Flow Law" is in very widespread use, so the "so-called" is superfluous.

Based on the comments of reviewer 1 (see reviewer 1, response #TC1) , we have updated our terminology throughout the manuscript to refer as the model describing linear-elastic and viscous deformation as visco-elastic model and have changed the term "visco-plastic" to "viscous", to avoid misunderstanding. We have also updated line 126 to read as simply *"Glen's flow law"*. We do note this terminology is different between computational solid mechanics and non-Newtonian fluid mechanics communities, and also experimental versus theoretical modelling communities.

Comment # 2

2) My main comments concern model formulation and how it may relate to reality. The 2D plane geometry seems to me to be perfectly adequate to explore the issues of interest, and there is perhaps no need to add experiments with an axisymmetric geometry (full 3D would of course be much better still).

Please see our response to reviewer 1, comments #1 and #3, where we justify the use of the 2D plane geometry. We have added the following to the manuscript:

"Using an axisymmetric representation would be appropriate for short crevasses and conduits with lengths much shorter compared to the length of the horizontal/radial basal crack, whereas the plane-strain representation used here is suitable for when the surface crevasse spans longer distances."

"Of course a 3D model able to capture both these phenomena would be ideal, but it would be computationally too expensive, so we utilize the 2D plane strain approximation, focusing on the propagation of fractures driven by stresses and only consider the lateral melting of the fracture faces."

Comment # 3

3) More serious for present purposes is the assumption of a frozen bed. In Greenland, the bed is temperate below most of the ablation zone, and water is certainly present at the ice-bed interface. This has three important implications for any attempts to compare the model results with reality. First, it is not necessary to form a fracture along the bed, simply to lift it.

We acknowledge that the ice-bed interface is not frozen over much of the Greenland Ice Sheet's ablation area, particularly in the locations where supraglacial lakes drain via hydrofracture (and this is recognised in the text on lines 95-97). We address the frozen-bed assumption in more detail below and in Section 2.1.2 in the text. We do recognise that this assumption does require more explanation as to not confuse the reader. We have therefore updated the text by adding a statement on line 95 directing the reader to the full discussion of this assumption at the first mention of the frozen-bed assumption. In this statement we also reiterate the fact that the ice-bed interface is this region is not frozen. We address the reviewer with the full reasoning for this model assumption below.

The crack propagates when the vertical stresses exceed the tensile strength of the cohesive interface, such that the difference in the stress required for horizontal cracking/uplifting to occur σ_{yy}^{crack} and the stress without any crack must exceed the tensile strength σ_{yy}^0 and the weight of the ice, that is:

$$\sigma_{yy}^{crack} - \sigma_{yy}^0 > f_t + \rho_{\mathrm{i}} g H. \tag{1}$$

The magnitude of these terms for the 980 m thick ice-sheet considered are $f_t = 0.14$ MPa and $\rho_{\mathrm{i}} g H = 8.74$ MPa, with the pressure within the fracture needing to be high enough to overcome the combination of these. If instead the base was not frozen, the tensile strength needed to be exceeded would be zero, but the majority of the stress that needs to be overcome from the gravity contribution would still exist, with comparable water pressure at the base being required for the uplifting. The effect of this would be that the crack starts propagating slightly sooner, with the pressure within the crevasse being slightly lower at the moment the horizontal propagation commences. However, from that point onwards, the rate of propagation is governed by the volume created due to vertical displacement of the ice-sheet, and the rate at which water flows down through the vertical crevasse; the lack of any tensile strength would not create a significant difference. To clarify that the majority of the resistance to horizontal crack propagation and uplifting at the base comes from the weight of the ice-sheet, we have added the following to the paper:

"The crack propagates once the stress within the ice, normal to the prescribed crack direction, exceeds the tensile strength, $\sigma_{yy} > f_t$ for the horizontal crack and $\sigma_{xx} > f_t$ for propagation of the vertical crevasse. As the weight of the ice induces compressive (negative) stresses within the ice-sheet, the total change in stress needed to propagate the crack is therefore given by $(\boldsymbol{\sigma} - \boldsymbol{\sigma}_0) \cdot \mathbf{n} > f_t + \rho_i g(H - y)$. For the horizontal crack, this implies that even through a frozen rock-ice interface is assumed with a tensile strength, the majority of the stresses that need to be overcome are a result of the weight of the ice and not the tensile strength."

Of course, a second difference of the bed not being frozen is the possibility of sliding to occur pre-fracture, enhancing the vertical crevasse opening by the two fracture faces "sliding" apart, which is addressed as:

"It should be noted that assuming the bed to be frozen has implications for the downward crevasse propagation and crevasse opening width after the vertical crevasse reaches the base, which is only driven by the elastic and viscous deformations. In contrast, were frictional sliding be allowed at the glacier bed even before the onset of horizontal crack and uplift, an additional opening width would be created due to the two "sides" of the ice-sheet sliding apart. The effect of the glacier sliding induced crevasse widening would be significant if the basal friction is weak."

It should be noted that, if a non-frozen bed was assumed, this free-slip enhanced crevasse opening would be significantly impacted by the plane-strain assumption. In a three-dimensional setting, the ice ahead and behind the surface crevasse would still connect the ice, limiting how much the sides can slide apart. In contrast, under plane-strain, representing an infinitely long crevasse in the out of plane direction, the two sides of the ice-sheet would be completely disconnected by the crevasse, thus allowing for a much larger sliding. As such, assuming free-slip (or frictional) basal conditions would not produce realistic results for a temperate base either without the addition of an additional body force representing the three-dimensionality of the crevasse (or performing full 3D simulations).

Comment # 4

4) Second, interactions with the basal drainage system will have potentially large influence on the fate of water arriving via a hydrofracture. Third, enhanced slip at the bed will not necessarily be confined to local 'blisters' as implied by the model. Each of these three considerations mean that real-world drainage events can play out in ways not simulated in the model. The points about basal drainage and basal slip are especially important, as the authors invoke concern about sea level rise as a justification of their work. In the frozen-bed scenario assumed in the model, any slip will be local and bounded by the surrounding frozen bed; in the real world, slip is less constrained, and can either be enhanced (if the extra water increases areas of ice-bed separation) or reduced (if the extra water encourages development of an efficient conduit). Both of these effects have been described as consequences of surface to bed drainage in Greenland, and show that interactions between hydrofractures and basal drainage are of great importance.

I am not suggesting that the authors include a basal drainage component in their model. Science often proceeds incrementally, and it is unreasonable to expect that this study should solve the complete problem. I am suggesting though, that the authors more fully acknowledge the complexity of the full problem, and more carefully identify which issues their paper has explored and which ones remain unsolved.

We address the reviewer's comments about the frozen-bed assumption in Comment #3 above and here focus on the broader points raised in this comment regarding the basal drainage component and basal slip.

We would like to first thank the reviewer for their clear explanation of the complexity of the interactions between surface meltwater induced hydrofracture and basal drainage interaction. We have addressed the reviewers concerns regarding the distinction of our model's results versus future model applications by revising Section 4.4 entitled

"Future of the Greenland Ice Sheet" in the main text. In this revision we make clear the scope of this model's application (i.e., the first hour of the lake drainage event and exploring local conditions only) and how these results can be coupled with other models or observations to investigate lake drainage induced changes to the subglacial drainage system and overall impacts on ice dynamics. Specifically, we do not currently include interactions with the subglacial drainage system because rapid lake drainage via hydrofracture should quickly overwhelm any existing subglacial drainage system. Regarding interactions with the subglacial drainage system after the formation of the local blister, this can be investigated by coupling our model with a subglacial hydrology model to include a drainage term to remove water from the blister. However, because this current paper focuses on the first 90 minutes of lake drainage when the blister is forming we do not include that here and instead discuss how to incorporate basal drainage system interactions in Section 4.4.

The added text to Section 4.4 reads: *"While the presented model is restricted in scope to only solve for a single hydro-fracture and the resulting uplift, results can be coupled with data sets and other process based models to fully investigate ice dynamic response to rapid supraglacial lake drainages. For example, crevasse formation models (e.g., Hoffman et al., 2018) or remote sensing data can constrain the timing of supraglacial lake drainages with the later also defining pre-drainage lake conditions. Results from our subsequent model runs can be fed back into large-scale low-resolution glaciological models, or smaller floodwave propagation models (e.g., Lai et al., 2021) to inform changes in basal conditions both locally and downstream along the resulting subglacial floodwave." … "While our model does not incorporate a basal drainage component nor simulate the subglacial floodwave produced by the lake drainage event, these complex processes can be investigated in the future by coupling our model with a subglacial hydrology model to comprehensively assess the ice-dynamic and hydrological consequences of rapid lake drainages. "*

[1] Dow, C. F. et al. (2015). Modeling of subglacial hydrological development following rapid supraglacial lake drainage. J. Geophys. Res. Earth Surf., 120, 1127–1147. `https://doi.org/10.1002/2014JF003333`.

Comment # 5

5) It is also worth highlighting that the model also requires the location of initial crack to be specified, a limitation that will need to be overcome before exploring the kind of "flood wave" scenario the authors invoke in lines 495 and forward.

Indeed, the location of initial cracks and the expected crack path need to be known and defined at the onset of the simulation, which is a weakness of the described model, and more broadly, a weakness of interface element-type representations of fracture processes. While this pre-determined crack path is already shown in Fig. 2, we have added the text below to the start of the methods section to make clear this limitation. We also discuss the use of satellite imagery or another process-scale model to define the locations and timing of supraglacial lake drainages to inform the application of our model.

"It is noteworthy that the path along which new interface elements are inserted is pre-determined, allowing the crevasse to only propagate straight down, and then splitting into two basal cracks that can only propagate sideways in a straight line. While the pre-determination of crack path and insertion of cohesive interface elements only between continuum finite elements are limitations of our numerical approach, it is reasonably realistic given the 2D idealisation of the rectangular glacier domain. The requirement of the crack path to be known a priori *restricts the nucleation of crevasses elsewhere in the domain. Also, we do not model the surface hydrology associated with the formation of supra-glacial lakes, but rather assume a pre-existing lake with known depth that intersects with this initial crack."*

Due to this limitation, the model indeed can not be used to study the nucleation of water-driven crevasses throughout an ice-sheet, and capture how the water transport to the glacial bed impacts the glacial movement. However, by informing the model with the data provided from large-scale glaciological models (providing strain rates, geometry, and surface water-levels), we may be able to produce high resolution predictions of the rate at which a single crevasse will transfer large amounts of melt-water to the bed; this meltwater flux can then be fed back into large-scale models to predict more long-term behaviour. We have clarified this potential usage of this model, indicating that it does not capture the full problem but only the part requiring high-fidelity simulations, in the discussion: *"While the presented model is restricted in scope to only solve for a single hydro-fracture and the resulting uplift, results can be coupled with data sets and other process based models to fully investigate ice dynamic response to rapid supraglacial lake drainages. For example, crevasse formation models (e.g., Hoffman et al., 2018) or remote sensing data can constrain the timing of supraglacial lake drainages with the later also defining pre-drainage lake conditions. Results from our subsequent model runs can be fed back into large-scale low-resolution glaciological models, or smaller floodwave propagation models (e.g., Lai et al., 2021) to inform changes in basal conditions both locally and downstream along the resulting subglacial floodwave."*