# Author's response to referee comments on "Buoyant forces promote tidewater glacier iceberg calving through large basal stress concentrations"

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We would like to thank both reviewers for their constructive and insightful criticisms and comments on our manuscript. These will be taken account of in our revised manuscript. Here we reply to the more important criticisms.

As both reviewers correctly point out, there are numerous instances where we have made use of jargon phrases which have been poorly explained and are therefore confusing to the reader. These will be more clearly defined to improve clarity for the reader. Typos will also be addressed, and appropriate references for Weertman sliding, Stokes equations and Glen's Flow Law will also be included where necessary.

Below, we deal with substantive comments from both reviews. Reviewer comments, numbered chronologically, are in italics, and the author response in plain text. Details of changes to the manuscript to address comments will be highlighted in bold text.

## Reviewer #1

1. *I wonder if the title really does justice. . . the paper is about bending moments (viscous and plastic bodies have bending moments too!) generated by geometry changes at the ice front due to ice/ocean and ice/atmosphere and ice/wave interactions. . . the present title could be misunderstood to represent "same old basal shear stress" stuff. . ..*

The paper is not specifically about bending moments but rather about forces generated at the bed instantaneously at the onset of bending in response to the perturbation. Some further explanation is required on this for clarity (see the response to the next comment), but as such, we do not intend to change the title.

2. *line 13 - would it be more accurate to say "viscous bending moment" (remember you can bend a beam viscously and elastically and viscoelastically) leading to high tensile stress concentration at the bed. . . instead of stresses at the ice-bed interface? Who cares what the stresses are at the interface if the ice is actually in a state of bending induced fracture?*

As above, we don't attribute these stresses to a bending moment. We attribute these longitudinal stresses as those required to balance the abrupt decrease in basal shear across the grounding zone. Arguments to support this are as follows.

Firstly, the region of large stress is very sharply focused at the bed. In the bending moment hypothesis, they would be further distributed into the ice body, with a more gradual increase towards the bed. Secondly, the location of the maximum stress corresponds with the precise point of ungrounding. In the bending hypothesis, it would be slightly downstream of this point due to a finite radius of curvature. Thirdly, the downstream longitudinal deviatoric stress associated with regrounding of glacier ice is negative (corresponding to the abrupt increase in basal shear stress at the point, the inverse of the effect occurring upstream), and not positive as it would be under the bending hypothesis. Finally, we show that the form and magnitude of stress is dependent upon the choice of sliding law and application of basal water pressure, which would be largely irrelevant under the bending hypothesis.

**We will include these arguments in the discussion to make our interpretation clear.**

3. *line 1 page 4 - Is Cauchy stress the same as deviatoric stress?*

The deviatoric stress indicates the deviation from the Cauchy (or full) stress, i.e. it negates the average pressure term. For our ~1km thick glacier, the pressure will be ~10MPa everywhere along the bed and therefore the Cauchy stress, ignoring additional water pressure, will be compressive everywhere.

**We will clarify this detail in the paper with an adjustment to equation 2.** However, the stress metric we will be using in the final revision of the paper will no longer the longitudinal deviatoric stress but rather the largest principle Cauchy stress, in response to comments by Reviewer #2.

4. *line 9 page 5 - Just out of curiosity why are 191 and 644 meters so precisely known as to be significant to the single meter? Can the authors tell us what would happen if the numbers were 192 and 643?*

The precision of these numbers is not important to our argument, and is in a small part determined by the mesh resolution (i.e. the peak occurs at a mesh node). Since it is not possible to identify locations to the nearest meter in the figures provided, **we will change to reporting them as "approximately 190 m" and similarly for other such numbers.**

5. *section 3.1 - What is the a priori reason to expect water pressure to be significantly important in the problem? is it for promoting fracture propagation or is it for lubricating the base?*

The significance is that the mechanism we suggest relies on the abrupt reduction in basal shear stress where the ice ungrounds to produce the longitudinal stresses to balance this. If we apply a sliding law where the basal shear stress reduces gradually as a function of effective pressure, we wouldn't expect the same result. **We will add a sentence to the manuscript to explicitly make this point.**

## Reviewer #2

1. *There is one major issue with the analysis, which is that the authors compare longitudinal deviatoric stress with a yield strength estimate from Vaughan (1993). The longitudinal deviatoric stress is problematic for two reasons. First, the various components of the deviatoric stress are not coordinate system invariant and have little physical meaning: a different coordinate system would result in different numbers. It is possible that the authors want to look at, say the components of the traction along the bed (which is well defined) or the largest principle deviatoric stress (which is also well defined). But this raises the more fundamental issue: it is the largest principle Cauchy stress and not the deviatoric stress that controls tensile fracture. And it is clear from the manuscript that the authors are fully focused on tensile basal crevasses. If the authors want to argue that the stresses are sufficient to trigger a tensile basal crevasse then they need to examine the largest principle Cauchy stress. Fortunately, this should be straightforward to compute from the full Stokes model. More problematically for the analysis performed here, for a kilometer thick glacier, the hydrostatic pressure is probably of the order of 10 MPa and may result in a negative (compressive) largest principle Cauchy stress. I should point that this is a common problem when dealing with failure of ice and especially basal crevasses. The most common solution to this problem is to (rather arbitrarily) superpose a hydrostatic pressure associated with water to the largest principle Cauchy stress to simulate the effect of water filled crevasses. This is commonly done and I think the authors could get away with it here if they want. Technically, you can't really do this and the right way to do it is to calculate the Cauchy stress after introducing an infinitely narrow test crack. Doing it the right way, usually results in a compressive stress when using the power-law creep rheology of ice. If the authors go the usual route of*

*superposing a hydrostatic stress field, I do suggest showing the stress with and without water pressure to emphasize that the water pressure is (or is not) critical.*

We have recalculated stresses using the recommended metric of the largest principle stress plus the water pressure, referred to as Effective Principle Stress (EPS) in Benn et al. (2017):

$$\mathrm{EPS} = \sigma_1 + p_w = \frac{\sigma_{xx}+\sigma_{yy}}{2} + \sqrt{\left(\frac{\sigma_{xx}-\sigma_{yy}}{2}\right)^2 + {\sigma_{xy}}^2} + p_w$$

Unsurprisingly, the resultant stress profiles are similar to those reported in that paper. There is a large concentration of EPS in the location where the longitudinal deviatoric stress ($\tau_{xx}$) peaks. The downstream compressive stress peak and the tensile peak located directly at the bottom corner of the calving front are comparatively greatly diminished in EPS as compared with the same features in $\tau_{xx}$. Figures AC1 and AC2 below compare $\tau_{xx}$ and EPS calculated for the example reported in figure 4 of the original submission.

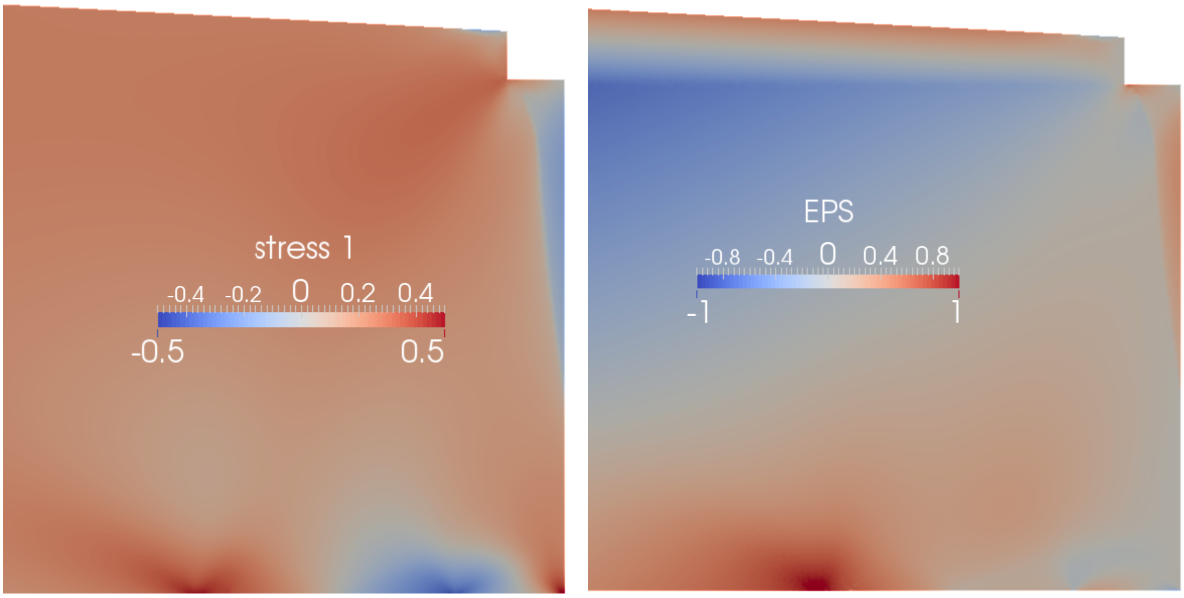


**Fig AC1.** Comparison of $\tau_{xx}$ (left) and EPS calculated for the geometry shown in Figure 4. of the original submission. Units of stress in MPa. The spatial scale is the same as Figure 4. of the original submission.

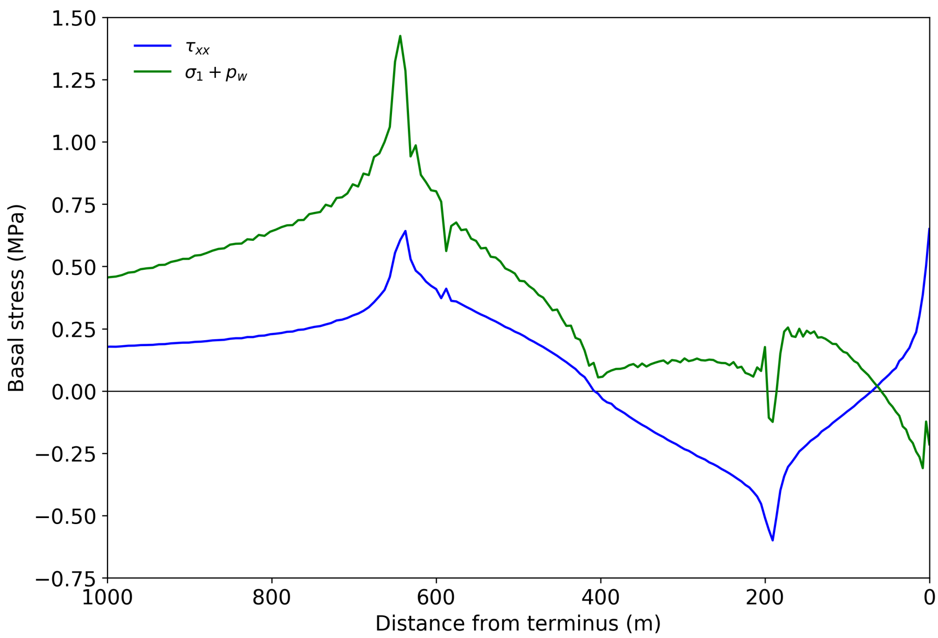


**Fig AC2.** Comparison of $\tau_{xx}$ and EPS along the basal boundary, for the same geometry as in Figure AC1.

As the reviewer points out, the ice hydrostatic pressure is of order ~10MPa at the bed and therefore without the addition of the water pressure, the largest principle stress $\sigma_1$ is negative everywhere. It was suggested that we add a plot to show that the water pressure is critical to produce a positive stress. We feel that this would be unnecessary and not particularly instructive, since $p_w$ has a constant value of 9.1MPa along the bed. However we will add a sentence to make it clear that the addition of $p_w$ is critical to our analysis.

The reviewer suggests that the more correct way to carry out the analysis would be to introduce test cracks into the geometry and look at the crack tip stress. We have looked at this (Fig. AC3), but this problem has the stress tend to infinity approaching the crack tip, so that although our simulations do indicate a substantial stress around the tip, the numerical solutions do no converge with mesh resolution.

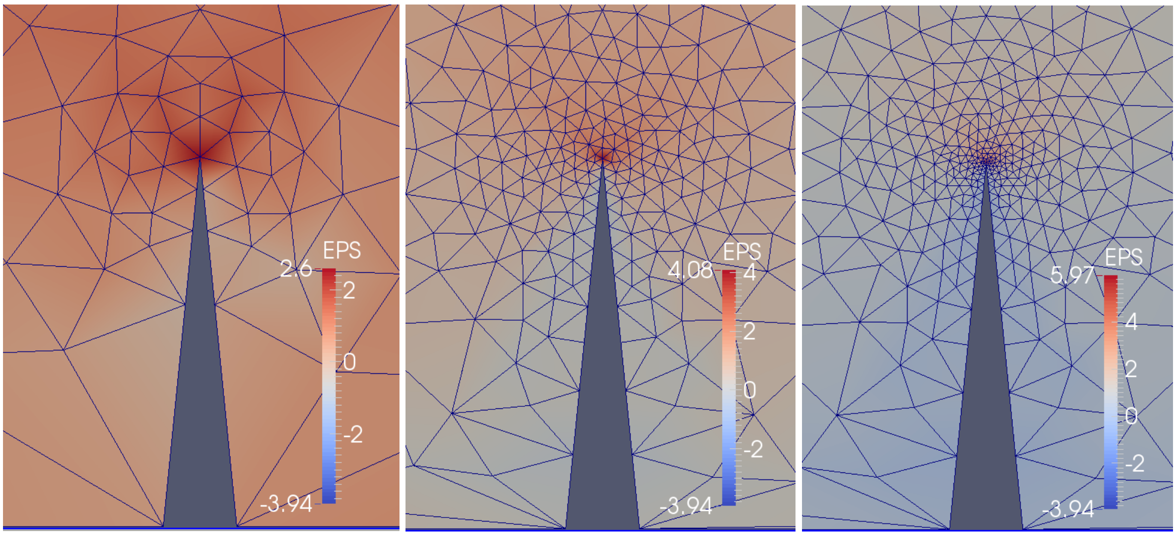


**Fig AC3.** Demonstration of the dependence of the crack tip stress upon the mesh spacing, with increasing resolution from left to right. The crack is 1m wide at the base, 5m high and located at the position of maximum basal EPS. In each figure, the maximum EPS on the colour bar corresponds to the value of EPS at the crack tip.

One approach to avoid this issue and test the likelihood of the crack to grow or stagnate would be to use the methods of linear elastic fracture mechanics (LEFM) to calculate a stress intensity factor for the crack tip (e.g. van der Veen, 1998; Krug et al., 2014).

We feel that following this method would entail a significant extra addition to the paper at this stage. Therefore our preferred choice is to use the EPS metric. Using this metric, the substantial growth in concentrated EPS following the geometric perturbation would lead to the formation of a crevasse at the same location as the original deviatoric stress metric. Other similar modelling studies (e.g. Nick et al., 2010; Todd et al., 2014) apply the Nye zero stress criterion (Nye, 1957) to calculate the depth of crevasses. Although we do not calculate crevasse depths, we will refer to this criterion so relate the high stresses to locations where crevasses will form.

The change of reported stress metric will require numerous minor changes to figures and text. **Figures 3, 4, 5, 7, 8 and 9 will be revised with the EPS metric replacing the longitudinal deviatoric stress. An additional description of the EPS metric will be added to section 2.1. Minor changes to the text corresponding to the updated figures will be made in sections 3, 3.1 and 4 where appropriate. References to Vaughan (1993) will be removed. A reference to Nye (1957) will be added in section 3.**

2. *Another aspect of the analysis that is somewhat problematic is that the authors are comparing their stress metric to the yield strength estimated by Vaughan (1993). My understanding, however, is that Vaughan (1993) examined various yield strength envelopes,*

*finding that the Von Mises stress envelope provided the best fit to the observations. The Von Mises yield criterion, however, is only equivalent to tensile failure in uniaxial loading, which is not the case for the model considered here. Recalling that the second deviatoric stress invariant invariant is proportional to the Von Mises stress, what I suggest is that in addition to the largest principle Cauchy stress, the authors also consider showing the second deviatoric stress invariant as an additional stress metric. This stress metric can be more directly compared with Vaughan's estimated yield strength. Note that in two dimensions, the second effective deviatoric stress invariant is equal to the maximum shear stress and thus the failure mechanism predicted by this envelope would be shear, rather than purely tensile failure and, if the authors go this route, the authors will need to be careful to point this out. Although we speculated that shear failure is important for tall calving cliff in Bassis and Walker (2012), I'm not aware of any strong observational evidence supporting shear failure in calving so the authors may want to take this suggestion under advisement as the broader community has doubts about the viability of shear failure.*

As discussed in our response to point 1 above, this issue is avoided as a result of switching to use of the EPS stress metric.

3. *Another minor point is that the authors convincingly argue that time scales they are interested are short compared to the time scales of flow and thus they can ignore the effect of ice flow on their experiments. However, if the time scale of interest is short compared to the time scale of flow, then this would seem to imply that an elastic rheology would be appropriate. This is surprising to most, but the elastic stress can be quite different from the viscous stress and this is primarily a consequence of the non-linearity in the creep flow law used.*

We are pleased that the reviewer finds our arguments regarding timescales convincing. We agree with this minor point that the stress should include an elastic component, though we suspect that the boundary conditions are the key factor, rather than nonlinearity. Here we have neglected elastic forces along the lines of (e.g. Benn et al., 2017; O'Leary and Christofferson, 2013) but accept that inclusion would change our results quantitatively. We do still expect a substantial increase in stress around the grounding line, since the force formerly acting on the bed downstream must be transferred upstream.

4. *The authors should be a little bit careful when discussing sliding laws, water pressure and the stress regime because glaciers are actually three-dimensional with bumps in the bed. In three-dimensions, these bumps play a pretty big role in controlling the stress transmission upstream because portions of the calving front maybe well grounded whilst other portions are close to flotation.*

**We will add a paragraph to the discussion along the lines of the following:**

"The reader should note that our model geometry is highly idealised. In reality, glacier beds are highly non-uniform, with variations in geometry, water and overburden pressure. Bedrock bumps therefore play an important role in controlling the stress transmission upstream. It is plausible that these variations could result in basal stress concentrations of a similar magnitude to the mechanism discussed here."

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