Review of 'Modelling the elastic transmission of tidal stresses to great distances inland in channelized ice streams'

This is a generally well written paper, using numerical experiments to explore the transmission of stress generated by tidal forcing at the end of an ice stream. The primary novelty is the inclusion of lateral resistance, which is argued to cause significant decay of longitudinal stress with distance upstream of the grounding line - at least more than has been previously assumed. It is concluded that stress is unlikely to be transmitted more than 2 times the ice stream width, but this is inconsistent with observations from Rutford ice stream. An alternative explanation, the propagation of the tidal signal within the subglacial hydrological system, is advocated.

The arguments seem reasonable, and I think the paper could be published, but I think that before this happens a number of aspects need to be cleared up, and some of the explanations need to be considerably improved. I found it quite difficult to tell what was actually solved in the models, some of the approximations that are made need to be acknowledged more readily as such, and there needs to be some consideration about whether the conclusion is specific to the one set of observations that is mostly considered (Rutford) or holds more generally. In particular, for this latter point, if the stress can be transmitted up to 2 ice stream widths upstream, that could easily be up to 100km for larger ice streams, particularly if one takes into account the possibility of margin weakening etc.

One of the aspects of the model that I found questionable was the treatment of the grounding line as being fixed. In reality the grounding line would move as the tide goes up and down, and by assuming it is fixed it is not clear that the stresses near the grounding line would be properly resolved. Related to this is the model in appendix C, where it is not explained what boundary conditions are imposed on the ice shelf at the grounding line (it should really be a 'free' boundary).

I found the description of the models, in particular the boundary conditions imposed at the grounding line, to be rather unclear, and I think this needs to be improved. All three appendices seem to be about aspects of this boundary condition and ways in which it can be simplified - I think it would actually be clearer to combine these together and make a single appendix all about describing in greater detail what conditions are used for the different models. In appendix B, given that you have a three dimensional model so need to impose boundary conditions at all heights z, I don't really see why it is any harder to impose the full loading condition than the simple condition. In appendix C it needs to be made clear how and where these results are actually used for the rest of the study.

Although I am quite happy with the suggestion of hydrological control, I think that the section in 6.3 should be expanded somewhat, as I felt it seemed rushed and not explained fully. In fact, I would really like to see a more complete analysis of this model including a diffusion equation for the pore pressure distribution driven by the tide, but I leave it at the authors' discretion as to whether they include this. As it stands, however, there are no results of this model shown except an analogy with Gudmundsson (2007) - this analogy should be spelt out more, and some result shown to back up the claim on 2144, line 19 that the observations from Rutford ice stream can be 'explained' using this model. That explanation has largely been the point of this paper, but it seems to run out of steam before completing it.

Throughout, there are odd phrases that are not well written or are grammatically incorrect - a thorough proof-reading, especially of the appendices, is required.

Specific points

1. Section 1.1, and Table 1 - the distinction between observed tidal flexure and observed tidal stress should be made clearer. There is also some ambiguity about what 'stress transmission' really means. What is *observed* is not presumably not the stress - it is something else like seismic activity or changes in surface motion. Best to make clear what is actually observed since that is what you need to explain (in some ways the conclusion of this paper is that it is *not* really stress transmission - at least not through the ice).

2. 2123, line 25 - flow-line models do not *have* to assume no lateral stress at the margins; it is quite common to parameterize lateral shear stress (proportional to flow speed, say).

3. 2125, line 21 - this sentence does not read well. Might be good to explain that it is the deviatoric stress that is important when the rheology is made non-linear, and also that the hydrostatic component of pressure (which is being neglected here) *is* included when considering the stress to apply at the grounding line (see appendix).

4. 2126, line 3 - the applied force is equal to the 'excess' hydrostatic pressure? It should not be equal to the hydrostatic pressure. Make sure all the variables are defined.

5. 2127, line 11 - seems like the section on page 2135-2136 would fit much better here, where you're explaining the viscoelastic rheology, rather than providing similar discussion in different places.

6. 2128, line 1 - what does it mean to say the ice shelf is included 'explicitly'? You need to be more explicit about what the boundary conditions are - a lot of this discussion is relegated to the appendices, but even there it is not very clear what is actually done, and which of the different models are being referred to.

7. Figure 2 is not very clear. It appears as if it's showing the model domain in part (a), but on reading the text I think I understand that the ice shelf is never included explicitly as part of the domain, which is what it looks like in this figure. In part (b), what are the two insets on the left actually showing? The axes need labels. It should be clearer what 0 and 'full' ice stream width refer to on the main panel - do they refer to the transition between fixed and sliding basal conditions?

8. For the three dimensional models, it is not made clear what the domain is, and what are the boundary conditions applied at the lateral edges? The figures show a 10km width 'stream', but presumably this is the region of free slip bed, and the actual domain is wider? In section 4, it is then discussed what the effect of weakening 'the margins' is, but what appears to be done is to weaken all of the domain outside of the middle of the stream - *i.e.* not just the margins. There is also some confusing discussion about the 'margin width', and position of the margins in this context. The position of margins is surely controlled by where the transition from basal slip to fixed conditions occurs, rather than by this additional imposition of a change in ice strength.

9. 2133, line 3 - this paragraph is not at all clear, and needs to be revisited. In particular the 'note' in the second sentence is very vague - what is the 'marginal damage relationship', and what are 'compliant margin models' in line 11?

10. 2135, line 4 - typo Gudmundsson.

11. 2137, line 9 - this comment that the behaviour could be approximated as a linear viscoelastic effect seems to be at odds with the earlier comment about Gudmundsson's work finding non-linear interaction between modes giving rise to a fortnightly oscillation.

12. Figure 11 - the shear margins, which I think should be at the outer edges $|y| = 5$km in this figure, do not appear to have very different viscosity here, as the text suggests - in fact, it appears to be more just that the centre of the ice stream (where the lateral shear is zero) has a noticeably large viscosity, rather than there being particularly weak shear margins.

13. Figure 12 - the labelling of 'nondeforming bed' is not accurate I think? Else there would be no ice stream there. It is not clear to me why there needs to be the implied sudden cut off between the tidally influenced region and that region that is not influenced - a smoother transition would work just as well, and is probably more realistic.

14. 2141 - point 4 here does not seem to be a 'difference' between the models, but rather a comment about this model.

15. 2144, (15) - the h here is presumably not the same h as in (14) or (16)? Need to use different notation.

16. 2145, line 6 - has any evidence been shown for this for ice streams other than Rutford? If so I missed it.

17. 2147, line 15 - the sentence starting here is excessively long and does not seem to make sense.

18. 2148, line 20 - the flotation condition at the grounding line would suggest that the grounding line moves as the tide goes up and down. Are you referring to an average water level here?

19. 2149, (B2) - σ_{flex} does not seem to have units of stress, and is therefore an odd choice of notation. Is this correct?

20. 2149, (B3) - should have no $+$ on the right hand side?

21. 2152, line 4 - the dashed line corresponds to a 'constant loading function' - what does this mean? I struggle to understand where the spatial x dependence for this case comes from in figure C1.