Review of 'A numerical model for meltwater channel evolution in glaciers'

This paper attempts, by use of a numerical model for ice deformation and melting, to flesh out the idea that englacial water channels can form through excessive incision of a supraglacial meltwater stream. The authors use a two-dimensional model for ice deformation, with a non-linear ice rheology, to determine the motion of the ice surface. At the same time a section of the surface that is occupied by a meltwater stream undergoes melting due to turbulent dissipation. This melting causes the channel to migrate downwards, and when it reaches a sufficient depth, viscous deformation causes the ice walls to squeeze together and 'pinch-off' above the channel.

It is an interesting study and in principal I would be in favour of publication; there are, however, some important issues that need clarifying. These are listed below, followed by some more specific comments that might also be considered. My greatest query is the method used to calculate the melting rate on the channel boundary, as expanded on below. I think the results of the paper hinge quite strongly on this parameterization and without a better explanation I am not able to assess how appropriate the model and conclusions are.

Major points

1. Section 3.3, second paragraph. The crux of this model is the evolution of the ice-water boundary and the method described here needs better explanation. What does it mean to 'distribute dr_n along P by scaling with $H_{\max}$'? The justification is apparently to achieve maximum melt at the deepest part, but this seems a rather ad hoc (and important) assumption. That aside, however, it is not clear how this works: $H_{\max}$ is a quantity defined on the whole channel, so how does scaling by it make the melting rate form equation (7) vary around the perimeter as shown in figure 2(a)?

   Equation (7) assumes that melting is evenly spread around the wetted perimeter of the channel (dr_n/dt is the normal rate of melting). This is inconsistent with what is described in section 3.3 and in figure 2. At the least, equation (7), or an equivalent, could be brought together with the second paragraph of section 3.3 so that a consistent description of the melting process can be given; I think it would fit better in section 2 since what is described here is really part of the model, quite separate from the numerical implementation. The important thing is to ensure that the formulation conserves energy - that is, that the total amount of wall melting is equal to the energy available (the right hand side of equation (5)). At the moment it is not clear if and how this is the case.

   Given this method of calculating the distribution of wall melting and the fact it is different from that used by Fountain and Walder to obtain the result in equation (8), I do not see how one can expect the results to be even remotely comparable. The argument is made a number of times that the fact the close-off depth predicted by the current model is different from that predicted by equation (8) signifies the importance of the 'transient evolution of the system'; it does not seem well justified to conclude this if the physics included are actually different (though I have no doubt that the transient shape evolution is indeed important). Assuming the melting distribution used here can be described more clearly, perhaps it would be better to tone down the comparison with equation (8).

2. The width of initial ice perturbation. It is claimed in section 3.3 that the width of the channel is not prescribed, which is clearly true to an extent; however, it is not clear to me what the effect of the initial channel shape is, and I am surprised that this was not discussed more. I think it needs to be. In figure 3 (and presumably in the other simulations too) the initial channel is 4 m wide and the depression over that width remains throughout the course of the simulation. What happens if a narrower or a wider channel is imposed initially? Or a different shape?

   Does the channel tend to converge towards a certain shape? It seems from figure 5 that the incision rate is fairly constant except perhaps towards the end when it decreases slightly; it would be interesting if one could simply explain the dependence of this incision rate on parameters (particularly on Q and β) by examining the equations and knowing the shape of the channel tip from the simulations.

3. I think more is needed in the way of discussion about the physics and how realistic this model is. Has this type of evolution really been observed anywhere? The mechanism seems plausible, but I wonder if other processes that are not included might win out in nature. My own experience is that downcutting supraglacial channels more likely become 'englacial' due to filling in with so much snow during the winter that a permanent snow bridge (turning to firn and ice) forms over the top of the channel. Perhaps there could be some discussion of this?

   Also, one might go away with the impression that all supraglacial channels should undergo this process of turning into englacial channels. It is clear that this is not always the case, and some comments as to what competing processes can prevent this from happening in reality might be worthwhile (the long timescales and large fluxes required are perhaps factors in this?).

Specific points

1. Page 2606, line 9, and a few other places - much seems to be made of this being the 'first time' an ice dynamical model is coupled with channel flow. A paper by Cutler (1998) offers a very similar type of model for subglacial conduits and I think deserves some recognition here. In any case, I don't think so much advertisement within the paper is required - the knowledgeable reader will hopefully be able to judge this for themselves.

2. The writing is on the whole quite clear, but there are a number of places where I think the same thing could be said more succinctly. Lots of qualifying adjectives are used, presumably in an attempt to be precise - but they are often unnecessary: for example, 'studied meltwater channels' (2606, line 17 and other places), 'adequate numerical models' (2607, line 12), 'successfully model' (2608, line 6), 'temporal evolution' (2611, line 27), 'presented model', (2614, line 2). 'in our physical representation' (2608, line 17) is unnecessary, and there are other places like this where deleting a few words might make it read more smoothly.

3. Equation (1) - η should be inside the divergence since it is not constant (I am assuming this is a typo rather than what is actually solved - otherwise this is a major point that would require recalculating everything).

4. 2608, line 21 - Is it really necessary or warranted to introduce the name ‘Stokes-Glen’ fluid? In standard fluid dynamics this is simply a power-law fluid, and it does not seem necessary to make it sound any more unusual or special than it is.

5. Equation (6) - the notation γ seems to be introduced without every really being used - since the temperature gradient is always referred to later as $d\theta/ds$, it might make more sense just to leave it as that, or else to make more use of the (dimensionless) γ (in the figure captions, for instance). This term is presumably only an issue for volcanic-related meltwater - otherwise most meltwater must be derived from the melting ice surface and would remain close to $0°$. This might be worth a comment here.

6. Equation (8) - this probably could do with a little more explanation (see above too) - my recollection of Fountain and Walder’s argument is to balance the melting within a circular channel with the creep closure rate (for circular channels) driven by cryostatic ice pressure. Why not just write A^{-n} in place of $\tilde{A}$ rather than having to separately define it?

7. 2610, line 16 - ‘easily’ - I wouldn’t devalue your work so much. If it is really that ‘easy’ I would expect to see results of more simulations than the rather sparse selection shown here.

8. 2611, line 13 - ‘we place the lateral boundaries’? I’m not sure where they are moving from otherwise.

9. 2613, line 2 - choosing the timestep small enough to avoid numerical instabilities seems a good start, but presumably it would be a good idea to check that it is also small enough to converge to the correct result. I was drawn to wonder about this by the footnote on 2616, suggesting that t_{final} could be accurately quoted down to the resolution of a single timestep. Is it true that if one used a smaller timestep one would get the same t_{final}?

10. 2613, line 23 - This statement about pressurized channels seems to run counter to the main thread of the paper, that the evolution of the channel shape - the fact that it is *not* circular - is important. It is not at all clear why, just because the channel is pressurized, the melting and closure rates should balance each other. This would be the case in a steady state, but the point is that you’re not in a steady state (at least, not *necessarily*). Likewise, if the channel were circular (as in Fountain and Walder’s suggestion), one might argue that the pressure can adjust so that radial creep balances radial melting, but I don’t think the channels predicted here are circular when they close off.

11. 2616 - The Nusselt number you use should be defined somewhere (maybe just in caption to table 2). Also, the Nusselt number presumably changes during the evolution of the channel - this might be worth a comment. The assumption of strong turbulent heat transfer was implicitly built into the model in equation (5), so it is a good thing that this does turn out to be the case.

12. 2616, line 16 - ‘reducing β by one third’?

13. 2618, line 8 on - I don’t think it’s necessarily the case that you need resolve details of the turbulent flow in order to be able to account for pressurized flow. In the present formulation you use a lumped parameterization of the turbulent flow, and there is no reason to stop using such an approach because the pressure is not atmospheric - one could simply prescribe a non-atmospheric pressure on the ice interface for the ice-dynamics calculation. The bigger problem seems to me to that consideration of the

third dimension may be required in order to determine what the non-atmospheric water pressure is. But a simple starting point might be to take the water pressure within the channel as hydrostatic, below the level at which the complete close-off occurred (one presumably has to imagine that the channel slopes upwards into the page and would still be open to the air some distance further upstream. Using such an approach, I wonder if including pressurized flow within the current model might be more feasible than it is made to sound here. It would be interesting to determine if the channel continues to move downward to the base of the ice, or really does stop englacially.

References

Cutler, P. M. 1998 Modelling the evolution of subglacial tunnels due to varying water input. *J. Glaciol.* **44**, 485–497.