1 General Reply to all three anonymous reviews of manuscript tc-2011-70

We would like to express our gratitude to all three anonymous reviewers for their helpful and detailed comments. Especially we are thankful for the elegant and powerful melt rate distribution method reviewer 1 suggested. After implementing this approach into our model and re-running all experiments, we can now present a manuscript which contains a more general method for modelling meltwater channel formation. We decided to re-compute all our results with the new model to be scientifically consistent, even though the differences between our old model results and new model results with $\nu = 1$ were found to be minimal.

Below we present replies to all comments of all three reviewers and explain in detail how each comment changed our manuscript. We use the prefix "m" before section and figure numbers when referring to our manuscript, and the prefix "r" for this reply.

2 Reviewer 1

2.1 First major comment: Stokes equation

All three reviewer identified the incorrectly written Stokes equation for non-linear media which was previously stated in the manuscript. We regret this inadvertence and would like to confirm that the model indeed solves the right equation, as already suspected by reviewer 1. The manuscript has been updated and now contains the correct equation as equation (m1), the same which the model actually uses. We have condensed the equation into one line, but it is the same as reviewer 1 mentions.

The boundary conditions which reviewer 1 comments on are now explicitly stated in subsection m3.2 of our manuscript.

2.2 Second major comment: Melt rate distribution

Reviewer 1 presents an elegant and more generally applicable equation for the distribution of melt rate along the water-ice boundary. We have adapted our model to use the suggested parametrization. Unfortunately there is a small mistake in the average melt rate defined by reviewer 1, which we have corrected. Based on our old equation (m7) the average melt rate, $\bar{m}$, is actually

$$\bar{m} = \frac{\rho_{\mathrm{w}} g(\beta + \gamma) Q}{\rho_{\mathrm{ice}} L P} \tag{1}$$

and the local melt rate at any point along the water-ice boundary

$$m_{\mathrm{loc}} = \bar{m} \frac{(H_{\mathrm{max}} - z)^{\nu}}{\int_0^P (H_{\mathrm{max}} - z)^{\nu} ds} \tag{2}$$

as suggested by reviewer 1. We have re-written our section m2.3 to reflect the changes in the model and describe this approach in accordance to the comments of reviewer 1. At this point we would also like to thank reviewer 1 for providing a text passage describing the new melt model, which we partly used. Section m4 (results) includes now two figures displaying the sensitivity of the model to variations of ν and we present results for $\nu = 0.01$, 1.0, and 1.5. A reference to Ng (1998) is included in the manuscript at the appropriate location. As reviewer 1 already anticipates, the discontinuity at the water surface is numerically challenging, thus we present results for $\nu = 0.01$ instead of $\nu = 0.0$. This keeps the melt rate at the water surface equal to zero, but the overall distribution of melt rate along the boundary is still close enough to the $\nu = 0.0$ case to study this parameter set.

Figure m5 is now updated and displays results for our reference run ($Q = 1\,\mathrm{m^3\,s^{-1}}$, $\alpha = 0.0$, $\beta = 0.03$, and $d\theta/dt = 0.0$) and the three aforementioned values of ν. A gray shaded area marks the possible range of solutions for this parameter set. In the new figure m6, we explore the changes in channel shape and width when varying ν. As it turns out ν strongly influences channel width and shape as well as incision depth. Based on these results, we have updated section m4.1.

2.3 Third major comment: Channel close-off process

Reviewer 1 suggests, based on field observations, that net accumulation within the forming channel is the dominant closing process in reality and not viscous creep. We fully agree and have now included a paragraph in section m5 (conclusions) which describes this dominant process.

2.4 Minor points

Bullet points in the list below correspond to bullet points in reviewer 1 comments:

- The comparison between our model and Fountain and Walder (1998) has been removed from the manuscript based on the comment of reviewer 2 that our old equation (m8) is based on a different melt rate distribution. Fountain and Walder (1998) assume a circular channel in which a radial melt rate, uniformly distributed along the channel wall is balanced by ice deformation. This is quite different than our old and the new melt distribution stated above and in the manuscript. Please refer to the first major point of reviewer 2 below for details.
- 'Nye's old rate factor' has been removed from the manuscript.
- We have changed 'perpendicular' to 'perpendicularly'.
- Reviewer 1 is right, the precise distribution of melting along the channel wall will matter in such a case. As stated in our conclusions, further examination is not possible without a model, which fully resolves the turbulent water flow within the channel. Thus this remains subject of further research. We further agree with reviewer 1 that the density difference between ice and water will cause some downward motion. Without the possibility to further investigate this with our current model, we assume that this motion is small in comparison with the presented downcutting rates and that is why we stated 'no significant' downward motion is to be expected.
- The text on the issue raised by the reviewer has been re-written. In the current form it explains more clearly why we draw the conclusion that the temporal evolution is important and why the switch from unpressurized to pressurized channel flow ('flow regime switch' in the former version) plays a key role in the channel evolution.
- We have replaced 'turbulent forced convection' with 'turbulent mixing'.
- The caption of Figure 5 includes now a description that the end of each line marks the depth at which the respective channel gets pressurized. We have created the requested figure, which is now figure (m8) in the manuscript and have added a discussion in our results section.

3 Reviewer 2

3.1 First major point: Melt rate distribution and comparison of Fountain and Walder (1998) with our model

Reviewer 2 raises the valid concern that our previous description of how we formulate and implement the water – ice thermal transfer in our model was not sufficient to understand some key parts of our work. Reading through the comments made by reviewer 1 & 2 on this part of the manuscript makes it clear that we have to elaborate more on how this part of our model works. Given the highly relevant comments of reviewer 1 on this matter and the fact that we have completely rewritten section m2.3 based on these comments, we think that all issued raised by reviewer 2 with regard to this major point are now cleared. Equation (r1) and (r2) above and their counterparts in our manuscript explains how the melt rate is distributed along the channel wall and how $H_{\max}$ is used to do so. These equations also ensures that energy is conserved. Reviewer 2 also suggested that such a detailed description is better placed in section m2, so our changes to the manuscript with respect to the water – ice thermal transfer part should please reviewer 2.

Towards the end of the first major point, reviewer 2 doubts that a comparison between the our model and the maximum depth estimated by Fountain and Walder (1998) is possible because the assumed channel shape and melt rate distributions are different. Reviewer 2 is right, Fountain and Walder (1998) assume a circular channel with uniform, radial melting and we use equation (r1) and (r2) from above. Thus we have removed the comparison with Fountain and Walder (1998) from our manuscript. At several locations in the manuscript we still refer to Fountain and Walder (1998) as this is a highly relevant paper for our work.

3.2 Second major point: Initial ice depression and simple model

To explore the effect of initial surface depression shape on the evolution of a forming channel, we have included the new figure m6b in the manuscript. Several different starting geometries are presented and it is found that the evolving channel shape is independent of the initial depression geometry.

As demonstrated in figure m6a, the channel does converge towards a certain shape and the key parameter controlling this shape is ν.

The incision behavior can not be explained by simply evaluating equation (r1) and (r2) for a given parameter set. In the new version of figure m7 we display one result from such a calculation in comparison to the full model. After 500 days of evolution a difference of approximately 44 m exists between the full model and such a simple approach.

3.3 Third major point: How realistic is this model

The third major point of reviewer 2 is actually closely related to the third major comment of reviewer 1 (cf. sec. r2.3). We have expanded our discussion of the model in the conclusion section to elaborate more on how realistic the proposed mechanism of englacial channel formation by supraglacial channel incision is. Here we would like to point out that this mechanism is not proposed by us (cf. introduction section of manuscript), but is a result of our modelling efforts: by letting the described, coupled model evolve over time, the transition from supraglacial to englacial channel occurs.

3.4 Specific points

The enumerated list below corresponds to the enumerated list of reviewer 2.

1. The question on how much "advertisement" for a "new" approach in science is appropriate in a publication is raised quite often in reviews. We do agree with reviewer 2 that one should not oversell ones results. The other two reviewers do not raise any concern on how we present our results, so we did not change our manuscript in that respect. However, we have included the reference to Cutler (1998) in our introduction, as this is a relevant paper.

2. We do appreciate this comment on the writing style of our manuscript. As reviewer 2 correctly assumes, the qualifying adjectives are used to be precise. As reviewer 1 & 3 do not have any concerns with our use of these adjectives, we prefer to leave our manuscript writing style as it is.

3. It was indeed a typo as suspected by reviewer 2. See reply to reviewer 1 comment on this above, section r2.1.

4. Reviewer 2 is right, using the term power law fluid is sufficient.

5. We now make more use of γ throughout the manuscript, especially in the figure captions and also included a comment on the volcano-related usefulness of this parameter.

6. This part of the manuscript has been removed.

7. We have removed the word "easily" to not devalue our work.

8. This sentence has been rephrased. Instead of "move" we now use "placed" to avoid any confusion with fluid motion.

9. Reviewer 2 raises a valid point here. The time step chosen limits the resolution at which t_{final} can be resolved. We have rephrased the section in our manuscript to state clearly that the accuracy for estimating t_{final} is influenced by the chosen time step and change the respective footnote.

10. The final shape of our modelled channels is indeed circular for $\nu = 1.0$ and $\nu = 1.5$. In case of $\nu = 0.01$ the channel collapses at the end of the simulation. We present now shapes of final channels in figure (m6c). Thus we think the argument that the circular, pressurized channels adjust so that radial creep balances the melting is valid. Nevertheless we take this comment as motivation for further research on how these pressurized channels would evolve, as we have already stated in our conclusions.

11. The equation used to calculate the presented Nusselt numbers is now included in the caption of table (m2). We also have now included a comment that these numbers change with time and how we present these changing values.

12. Here we actually mean "to one third" as we change $\beta = 0.03$ to $\beta = 0.01$.

13. Again we take this very helpful comment as motivation for further research. Reviewer 2 is right that expanding the model into the third dimension is crucial as well to understand the evolution of such channels. However we think that resolving the flow within the channel in more detail is the direction we want to expand our model. This should create a more general and wider applicable model than just adding yet another parametrization for channel flow. We also agree with reviewer 2 that is would be worthwhile to study the evolution of these channels beyond the point of pressurization and include this in our future research plans.

4 Reviewer 3

4.1 General Appreciation

Reviewer 3 states that the motivation in the introductory section needs strengthening and we agree that it is sometimes hard to define applications in a pioneering paper. We have improved our introduction section and included the 1996 Gjálp eruption (Gudmundsson et al., 2004) as an example for application of our model in our manuscript. Figure (m7) now includes data from th drainage passage following this eruption to compare with our model results. We have included other examples of application in our improved introduction as well.

The new figure (m6a) identifies clearly ν as the controlling parameter for channel width. This result is now discussed in the manuscript. Different combinations of the parameters Q, α, β, and γ cause different penetration depths and we try to explain these dependencies in more detail in our revised results section and in figure (m8).

4.2 General Points

The enumerated list below corresponds to the enumerated list of reviewer 3.

1. This part of the introduction is indeed a bit generic. We have revised this part to strengthen the motivation for our research.

2. Without any indication to which section of the manuscript reviewer 3 refers to in this comment, we are not able to give any reasonable reply to this comment. The penetration depths of each simulation is a result of our modelling and this is quite clearly described in our manuscript. Maybe reviewer 3 is more satisfied now as we include figure (m8) which displays incision depth dependent on several key parameters of the model and figure (m6a) which demonstrates the dependence of channel width and incision depth on prescribed meltwater distribution.

3. The two figures mentioned above in point (2) should answer this question.

4.3 Minor Points

1. Both other reviewers have commented on our equation (m1). As mentioned above, we have corrected this equation.

2. We got rid of this name as it was not well received by reviewer 2 & 3.

3. The main difference between Röthlisberger (1972) and Raymond and Nolan (2000) is that Röthlisberger considers frictional heat produced in the water flow and energy loss due to temperature adjustment to reach the pressure melting point in the closed, pressurized channel. This results in 2/3 of the frictional energy is available for melting in Röthlisberger (1972). Raymond and Nolan (2000) do not consider pressure as it is an open channel and add water temperature to the equation (γ). This allows for modelling of positive meltwater temperatures, which provide extra energy.

4. We already state a value for n_c in table (m1), but we have now included a reference to table (m1) at this location.

5. The section on analytical depth (Fountain and Walder, 1998) has been removed from the manuscript.

6. Likewise this is not needed anymore, but reviewer 3 was correct, A^{-n} was wrong, it should have been $A^{-1/n}$.

7. We have changed "perpendicular" to "perpendicularly".

8. "stay" has been replaced with "remain".

9. We have inserted a reference to the respective subsections at this location.

10. "would" has been replaced by "were to"

11. Instead of stating a typical time step here, we have included a reference to Table (m2), which lists all pinch off times.

12. "is pinched" has been changed to "pinches".

13. We do not state that the channel stops downward migration at channel pinch-off. At t_{final} the channel shape changed to near circular as displayed in figure (m6c). At this stage the Röthlisberger argument should be valid. Reviewer 3 is correct that the addition of gravity would change things. Also the other two reviewers have commented on this, and we have now expanded this and our conclusion section to elaborate more on this subject.

14. Parts of the chosen parameter sub-set are motivated by meltwater drainage from subglacial eruptions. We now include data from the 1996 Gjálp eruption as an example, which explains our selection better. The rest of the explored parameters should demonstrate the model sensitivities over a range plausible for glaciological studies. We have rephrased this sentence to make the parameter choice more justified.

15. An increased ice surface slope tilting towards the channel as displayed in our figure (m1) naturally increases the gravity driven ice flow towards the channel. Any inclined surface of a fluid, driven purely by gravity, tends to level its surface. This is basic fluid dynamics and should not require further explanation.

16. We have changed the vertical scale in figure (m5) and all other, similar figures. Reviewer 3 is right, it makes more sense to label the axis like this.

17. The caption of figure (m5) explains now in more detail that the channel tip migrates downward.

References

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

Fountain, A. G. and Walder, J. S.: Water flow through temperate glaciers, Rev. Geophys., 36, 299–328, 1998.

Gudmundsson, M. T., Sigmundsson, F., Björnsson, H., and Högnadóttir, T.: The 1996 eruption at Gjálp, Vatnajökull ice cap, Iceland: efficiency of heat transfer, ice deformation and subglacial water pressure, B. Volcanol., 66, 46–65, doi:10.1007/s00445-003-0295-9, 2004.

Ng, F. S. L.: Mathematical Modelling of Subglacial Drainage and Erosion, PhD. Thesis, University of Oxford, Oxford, 1998.

Raymond, C. F. and Nolan, M.: Drainage of a glacial lake through an ice spillway, in: IAHS PUBLICATION, edited by: Nakawo, M., Raymond, C. F., and Fountain, A., no. 264 in IAHS Publication, International Association of Hydrological Sciences, 199–210, Washington, USA, 2000.

Röthlisberger, H.: Water pressure in intra-and subglacial channels, J. Glaciol., 11, 177–203, 1972.