An analysis of instabilities and limit cycles in glacier-dammed reservoirs

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Referee's report

This is an awkward paper to referee. Being Christian Schoof, there is not going to be anything technically wrong with it, but from my perspective it is not properly thought out.

Nor is it well presented. The very first thing to say is that the text is littered with typographical and grammatical errors, so many that I will not list them all here. But there are such errors on page 1, line 16; 1,24; 2,5; 2,6; 2,13; 2,15; 2,20 (twice); 2,29 (the whole second half); 2,35; and so on, and on, and on. Perhaps the best is saved for last, where Schoof's own 2012 paper is mis-referenced (it is part 1, not part 2). Incidentally, all the poor authors cited in the references are demoted to a single initial each.

The paper concerns a model (which is analysed in both a 'lumped' form and a spatially dependent one) for subglacial floods, or jökulhlaups. The paper is motivated heavily by previous work of Fowler and Ng, and seeks to modify this earlier work, by allowing for the case where there is no 'seal' of the subglacial (or ice-dammed) lake, which can then continually leak between floods, as is the case for Summit Lake, according to Fisher in 1973. Note: Fisher, not Fischer.

The improvement consists of showing that with an extra term in the closure equation of the classical Nye-Röthlisberger theory, the model will describe limit cycles even in the absence of a seal. This seems to me the principal achievement of this paper. The extra term invoked is an ingenious addition due to Schoof in 2010 which allows the description of both cavity drainage and channel drainage within the confines of a single model. It is worth offering some comments on this addition.

In its original form, the extra term appears as the first term on the right hand side of the closure equation

$$\dot{S} = u_b h_r + c_1 \Psi Q - c_2 N^n \left(\frac{Q}{c_3 \sqrt{\Psi}} \right)^{0.8},$$

and it describes the opening of cavities by ice flow (velocity u_b) over bedrock bumps (height h_r). The steady state of this equation provides for both channels (N increases with Q) and linked cavities (N decreases as Q increases). There are several comments to make.

(i) We might suppose in reality that u_b will itself depend on N as well as basal shear stress τ_b; might this not then ruin the conclusion? The answer is no, at least for sliding laws of power law type.

(ii) Second, Schoof's 2010 paper indicates a minimum value of $N \approx 2.6$ MPa. This seems very high, and particularly seems unable to explain the very low values of N seen in the Siple Coast ice streams, for example. One might say these are sediment-floored, so that the concept of bed roughness is less clear to understand: does this mean one must abandon this theory in that context? The reason I enquire is that it seems to me that the understanding of sub-ice sheet floods is something this theory should aspire to.

(iii) The boundary condition $N = 0$ is applied at the glacier snout. This is problematic because the closure equation then predicts S increases indefinitely. In the Schoof 2014 paper this is circumvented by saturating the opening term as $u_b h_r \left(1 - \dfrac{S}{S_0}\right)$, allowing for drowning of roughness at conduit size S_0; this allows a steady state to be reached, but one in which $S > S_0$, which makes no physical sense.

In fact, the issue with the boundary condition is that the outlet flow must become open to the atmosphere at some unknown point upstream of the snout, where I think two boundary conditions should be prescribed for N and N_x, corresponding to continuity of water pressure and water flux. This may be important in view of figures 8 and 9, for example.

Now we come to the main issue with this paper, which lies in its style. The paper does not know whether it is for glaciologists or applied mathematicians. The message is in fact fairly simple: here is a modification of Nye which allows limit cycles, even in a lumped version, and allows leakage between floods. But the material is drawn out by over-elaborate interpretations and explanations, and veers off into dynamical systems language which is neither helpful or informative. Starting on page 6, there is a rather long-winded stability analysis, which descends by page 8 to undergraduate mathematics. The only explanation can be that this is meant for glaciologists; but my view is that if they want to learn this material they should do so in textbooks, not in a research paper. And in fact, all you need is figure 4.

It goes on: we get undergraduate discussion of Hopf bifurcation, which by page 14 has slowed to the point of somnolence. And on. The section on asymptotic solutions on page 17 is mostly out of place here. What I actually think should happen is that the paper should be rewritten in two versions: a longer mathematical one which goes to a more mathematical journal (but then suitably prunes the more elementary stuff), and a shorter glaciological version which punches out the results: which are the model and some of the figures really.

What is missing from this is a focus on a particular flood: Summit Lake for example. Admittedly Grímsvötn has been extensively studied, and I don't know whether there are other such locations, but the lack of the possibility of even qualitative comparison to real data is a pity.

One of the issues which may deserve better attention is the occurrence of negative effective pressures, as for example in figure 8. There is some discussion of this (e. g., page 16) but it is not very satisfactory to my mind. Very high input rates can cause fracture-like floods, as in the Gjálp eruption of 1996 leading to the over-pressured jökulhlaup from Grímsvötn, but I don't know that they are that common. And according to Post and Mayo's USGS report, Summit Lake floods have been initiated below flotation. So to me this is a drawback of the model.