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Interactive comment on "An analysis of instabilities and limit cycles in glacier-dammed reservoirs" *by* Christian Schoof

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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.

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Discussion paper

I'm not sure my mere name warrants such confidence in the results, but I'll take this to mean I actually haven't made any mistakes. The apparent awkwardness is as difficult to answer at this point as it may have been for the referee to review the paper in the first place. In view of this, I will respond to the specific comments later. As far as the presentation is concerned— or rather, the number of typos and other superficial but annoying errors — I'm happy to concede the paper may have been submitted in too much of a hurry. My apologies for that. With regard to the author initials, I'll simply point out that I am using the Copernicus bibliography style file. As such the complaint is probably best addressed to the publisher.

The paper concerns a model (which is analysed in both a 'lumped' form and a spatially dependent one) for subglacial floods, or joÌĹkulhlaups. 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.

Fair point. I must have allowed my teutonic roots to influence my spelling.

The improvement consists of showing that with an extra term in the closure equa- tion of the classical Nye-RoÌĹthlisberger 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.

Indeed. I'd hope that beyond the qualitative statement that limit cycles are possible by adding a mechanism by which the drainage system remains 'open' in the refilling phase, an analysis of how flood magnitude and timing depends on forcing and geometry a valuable, too.

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 ub 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.

This is true; I have deliberately skirted this issue as the kind of glacier junctions at which dams are likely to occur often have awkward geometries — this is certainly true for Summit Lake, or the field site at the Kaskawulsh Glacier that has motivated my own interest in this subject (and before you ask: I do intend to publish data from said site, but inclusion in this paper would sure;y break the bounds of what is reasonable for paper length, even if I were to shorten the analysis). Those awkward geometries matter in the sense that treating ice flow as a function of local N might be stretching credulity. As the reviewer rightly points out, making u_b dependent on N doesn't ultimately break the mechanism being investigated, so long as u_b remains bounded below by something greater than zero. It just makes the flood cycle even less simple to describe. I'd be happy to add a brief discussion to the supplementary material if desired, but don't think this will add much to the main paper.

(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.

This, I believe, is actually a reference to the fact that classical R-channel models with Nye (1953)-type closure rates consistently overpredict effective pressures, not just in the sense that they would require effective pressures larger than overburden, but in the sense that *measured* overburden pressures are usually significantly smaller. That is not an original observation of mine. The main reference to this that I'm aware of is Hooke, R.LeB. and Laumann, T. and Kohler, J., 1990, Subglacial water pressures and the shape of subglacial conduits. *J. Glaciol.* 36(122), 67–71. The point is that flatter channel shapes allow smaller effective pressures to balance predicted dissipation rates in the flow, and therefore to reconcile observation and theory. In the context of the model being used here, this issue is discussed at length in Schoof (2014) as cited in the present manuscript.

I agree that this is an appealing direction to develop outburst flood models in, and in particular, that the question of what the lateral aspect ratio of a channel actually is deserved further attention (ideally building on the vastly underappreciated D.Phil. thesis by Felix Ng.) Probably beyond the scope of this paper, though. As far as the concept of bed roughness becoming nebulous for deformable beds is concerned, I'm inclined to agree for relatively fine-grained beds with a narrower grain size distributions — as would apply for the formerly submarine bed areas of West Antarctica, for instance. For polydisperse grain size distributions, where there are larger cobbles and boulders mixed into the till, I'd argue that there are likely to be bed protrusions that can support cavities as in the canonical hard bed picture.

(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 $u_b h_r(1 - S/S_0)$*, allowing for drowning of roughness at conduit size S0 ; this allows a steady state to be reached, but one in which* $S > S0$ *, 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.

What is really at issue here I think is the way the cavity opening rate "goes away" for large S (note that this is only relevant to the spatially extended model, so I will restrict my discussion to the latter). In the model as posed, as in the earlier Hewitt et al (2012), Schoof (2010) and Schoof et al (2012, 2914) papers, this is done by writing the opening rate as $u_b(h_r - h)/l_r)$ for a continuum "sheet", or equivalently as $u_b h_r(1 - S/S_0)$ for an individual conduit. This does have the somewhat unintended consequence of leading to the opening rate becoming negative in an unbounded way when S exceeds the threshold S_0. A better way of dealing with the idea that bed roughness cannot indefinitely lead to a constant opening rate as conduit size grows (which is probably robust) might be to write the opening rate as $u_b h_r f(S/S_0)$ with $f(x) \to 1$ as $x \to 0$ and $f(x) \to 0$ as $x \to \infty$; something like $f(x) = (1 + \tanh(x))/2$ would do.

Having made the choice of cut-off function we have made here (where f goes to ∞ linearly as $x \to 0$ instead of vanishing), we can ask what difference this makes. In a model where cavity opening vanishes for large conduit sizes, the dominant balance near a glacier margin where $N \to 0$ would be between the melt rate $c_1 Q \Psi$ and closure rate $c_2 S N^n$. This would still leave the problem singular at the margin with $S \to 0$, but not in a pathological way (and the problem could further be regularized to maintain finite S by supposing that the glacier ends in a cliff so N is small but finite, or by supposing that the channel evolution equation is not cast in terms of St but the material derivative $St + u_b Sx$.[1]

For that case, we can construct a near-margin form of the solution, and further ask how different the solution to other versions of the model is. In particular, for a pure channel

[1]This idea is due to Ian Hewitt, who may indeed have published it somewhere. While unappealing for cavities that are tied to bed roughness, the advection term must play a role near the snout, where melting happens not only because of subglacial water flow, but also from the surface, and ice flow must compensate for that.

model with a vanishing cavity opening rate, we get

$$S_t = c_1 Q_\Psi - c_2 S|N|^{n-1}N, \qquad Q = c_2 S^\alpha |\Psi|^{-1/2}\Psi, \qquad \Psi = \Psi_0 + N_x$$

and given a fixed discharge Q, a steady-state near-terminus solution can be constructed by noting that

$$\Psi = (Q/(c_3 S^\alpha))^{1/2}$$

so

$$c_1 c_3^{-2} Q^3 S^{-(2\alpha+1)} = c_2 N^n$$

and hence

$$N_x = -\Psi_0 + c_1^{-2\alpha/(2\alpha+1)} c_3^{-2/(2\alpha+1)} Q^{(1-4\alpha)/(2\alpha+1)} \left(c_2 N^n\right)^{2\alpha/(2\alpha+1)}$$

which is clearly solvable form $x < L$ with a boundary condition $N(L) = 0$; the near field behaves as N $\sim \Psi_0(L-x) - \frac{2\alpha+1}{2\alpha n+2\alpha+1} c_1^{-2\alpha/(2\alpha+1)} c_2^{2\alpha/(2\alpha+1)} c_3^{-2/(2\alpha+1)} Q^{(1-4\alpha)/(2\alpha+1)} \Psi_0^{2\alpha n/(2\alpha+1)} (L - x)^{(2\alpha n+2\alpha+1)/(2\alpha+1)}$
$S \sim c_1^{1/(2\alpha+1)} c_2^{-1/(2\alpha+1)} c_3^{-2/(2\alpha+1)} Q^{3/(2\alpha+1)} \Psi_0^{-n/(2\alpha+1)} (L-x)^{-n(2\alpha+1)}$ Clearly, we can see that N remains well-behaved (and indeed positive, so the channel need not be partially open to the atmosphere!) while S blows up in a power-law fashion. We can go further and ask what the stability properties of the channel-only problem look like in the near field and construct a linearization. This is best done by changing the dependent variable S into something that remains bounded. An obvious choice is

$$Y = S^{1-\alpha}$$

The dominant balance when linearizing the problem above about the steady state ($Y = \bar{Y} + Y'$, $N = \bar{N} + N'$, $\Psi = \bar{\Psi} + N'_x$, $Q = \bar{Q} + Q'$, where barred quantities are steady state solutions and primed quantities are small perturbations) works out to be Y'$_t$ $\sim$

$\frac{3}{2}c_1c_3\bar{\Psi}^{1/2}N'_x$
$Q' \sim \frac{\bar{Q}}{2\bar{\Psi}}N'_x$

The germane question with using different model formulations that do not suppress the cavity opening term as above is whether they lead to the same solution away from a small region near the margin. As in, does the “regularization” of the model make any difference? In view of the question about numerical results tin figures 8 and 9, the question I will try to address is whether discrepancies between the channel-only model advocated above and the model used in the paper become more pronounced at large water throughputs in the model, which is the parameter regime that these calculations look at. For more moderate throughputs, the good agreement between lumped and spatially extended model suggests the issue of what happens near the margin (which does not feature in the lumped model) becomes less relevant.

The model used in the paper replaces the above by

$$S_t = c_1 Q_\Psi + u_b h_r(1 - S/S_0) - c_2 S|N|^{n-1}N, \qquad Q = c_2 S^\alpha |\Psi|^{-1/2}\Psi, \qquad \Psi = \Psi_0 + N_x.$$

In order to look at the difference from the channel-only model obtained by putting $u_b h_r = 0$, I will scale this by defining

$$[S] = \left(\frac{[Q]}{c_3[\Psi]^{1/2}}\right)^{1/\alpha}, \qquad [t] = \frac{[S]}{c_1[Q][\Psi]}, \qquad [N] = \left(\frac{c_1[Q][\Psi]}{c_2[S]}\right)^{1/n}, \qquad [x] = \frac{[N]}{[[\Psi]}$$

where $[Q]$ and $[\Psi] = \Psi_0$ are assumed to be given. Putting

$$S^* = \frac{S}{[S]}, \qquad N^* = \frac{N}{[N]}, \qquad \Psi^* = \frac{\Psi}{[\Psi]}, \qquad Q = \frac{Q}{[Q]}, \qquad t^* = \frac{t}{[t]}, \qquad x^* = \frac{x}{[x]},$$

and immediately dropping the star decorations, the model becomes

$$S_t = Q\Psi + \delta - \nu S - S|N|^{n-1}N, \qquad Q + S^\alpha|\Psi|^{-1/2}\Psi, \qquad \Psi = 1 + N_x$$

where

$$\delta = \frac{u_b h_r}{c_1[Q][\Psi]}, \qquad \nu = \delta[Q]^{1/\alpha} c_3^{-1/\alpha} [\Psi]^{-1/(2\alpha)} S_0^{-1}.$$

We can repeat the exercise of finding steady states. Assuming without loss of generality that the scaled flux $Q = 1$, we find $\Psi = S^{-2\alpha}$ and

$$S^{-(2\alpha+1)} + \delta/S - (nu + N^n).$$

Hence S = (nu +N$^n - \delta/S)^{-1/(2\alpha+1)}$,
$N_x = -1 + (\nu + N^n - \delta/S)^{-1/(2\alpha+1)}$ which the channel only model replaces by S = (nu +N$^n)^{-1/(2\alpha+1)}$,
$N_x = -1 + (\nu + N^n)^{-1/(2\alpha+1)}$ We want to know whether for larger $|L - x|$, the full and channel-only models will agree. This will be the case provided N agrees between the two models, and the latter will be the case if the correction δ/S remains small compared with $nu + N^n$ as well as having $\nu \ll 1$. This will be the case so long as $S \sim \nu^{-1/(2\alpha+1)}$ near $x = L$ is large enough, in other words, if $1 \gg \nu \gg \delta/nu^{-1/(2\alpha+1)}$ or $\nu \ll \delta^{2\alpha/(2\alpha+1)}$. The definitions of δ and ν above show that $\nu/\delta^{-2\alpha/(2\alpha+1)}$ increases with $[Q]$, all other parameters being constant, so we would in fact expect closer agreement between full and channel-only models for large $[Q]$.

We can go further and look at the linearization of the problem, again in terms of the variable Y used above (or rather, its obvious dimensionless counterpart); the dominant balances when adding the cavity opening term become Y'$_t \sim \frac{3}{2}\bar{\Psi}^{1/2} N'_x + \delta \bar{Y}^{1/(\alpha-1)} Y'$ $Q' \sim \frac{1}{2\bar{\Psi}} N'_x$ By similar construction to the above, if the steady state converges to that for the channel-only model as $|L - x|$ becomes large, so will the linearized solution for small δ; in other words, the additional term due to cavity opening will remain a small correction. This suggests that the stability resutls in the main paper remain robust for large water throughput rates.

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.

Style may be where the referee and I won't agree. I am happy to shorten some of the material in the paper where appropriate, such as the linear stability analysis. The existing text undoubtedly can be optimized in that sense, but I don't think that's the issue. I understand the rationale for splitting work between "mathematical" papers and "glaciology" papers. This has been practised by a number of researchers in the past (Hutter, Morland, Fowler etc.) and even I have been known to try. However, in my own experience, what happens is that these mathematical papers, to the extent that they are taken up by anyone, get cited by glacioloists, not by applied mathematicians or fluid dynamicists outside of glaciology. The only exception are perhaps those dealing with numerical analysis of glaciological partial differential equations. A brief trawl through an indexing website like Web of Science should confirm that impression.

In short, there seems little point in these separate mathematical papers for an imaginary specialist audience. At the same time, I do not believe in simply saying "here are our mathematical results, but you wouldn't really understand so we won't explain any of the detail", which is the risk I see in writing a "glaciological version". What I do see in glaciology is an increasing number of researchers who have solid background in physics or similar disciplines. These researchers have the ability to understand mathematical material but may need a more didactic approach than the simple assumption that they have not only taken a course in dynamical systems theory, but actually remember its contents. This is the audience I'd like to reach here. Yes, doing so may mean a more pedestrian pace for the fully-fledged mathematician as a reader, but there are few enough of those around that I'm disinclined to worry (except about the referee, who I assume is an applied mathematician). I should add: I understand that a paper is not a textbook, but slightly more explanation to get a point across does not go amiss, and I think the manuscript as submitted is honest about what is ultimately textbook material and what is not (although Stogatz may admittedly be a more suitable textbook for the target audience than Wiggins).

I would add that a 'didactic approach' to presenting mathematical material in glaciology has been taken previously, even where that material arguably has limited novelty in a global (as opposed to discipline-specific) sense: to name but one example, a number of papers published in the Journal of Glaciology around 2011 (primarily by Bassis and Dukowicz et al) have elaborated on the fact that Stokes' equations are equivalent to a minimization problem — something that had been known to applied mathematics and fluid dynamics at least since the 1960s, but was apparently not widely known in glaciology. Whether the referee (who presumably hails form an applied mathematics background) would regard those papers as giving an undergraduate introduction to the calculus of variations I can't tell, but these particular papers clearly have had some impact (with 8 and 30 citations, respectively).

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