The evolution of isolated cavities and hydraulic connection at the glacier bed. Part 2: a dynamic viscoelastic model

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1 Referee #1

Reviewer: 1. Oscillations in h (Sec. 3.2 onward; Figs. 3-5): you describe the "overshoot" and "undershoot" oscillations and analyse the observed factors behind their amplitude and decay rate with a good level of detail. Also you refer to the role of changing bed slope of contact areas (p. 15) and later discuss the implications of the oscillations (p. 24). However, I think that the physical cause of these oscillations is really never made clear or properly discussed in this manuscript. The sentence on p. 15 "These variations in normal velocity are presumably the reason for the significant oscillations in h-bar" doesn't satisfactorily address the cause. Can you please fill this gap by adding a passage or paragraph — at least to discuss candidate mechanisms if the correct one is difficult to determine? (Probably what looks like 'propagating waves' on the ice-base topography in Fig. 7 can aid the discussion.)

Response: I have had a stab at this in two places, the first being five paragraphs from the end of section 3.2

These variations in vertical velocity are presumably the reason for the significant oscillations in $\bar{h}$: when v_3 is larger, this causes uplift of the cavity roof downstream of the contact area, and that uplifted cavity roof causes the contact point to migrate downstream too, causing the contact area to move over time to a flatter location, thereby reducing the amount of uplift. That in turn causes reduced uplift of the cavity roof, so the contact area moves again to a steeper part of the bed, restarting the cycle (albeit with a smaller amplitude on each cycle). I illustrate the interactions between congact slope angle and growth of the cavity further in section 3.3, in particular in figures 8–9, and in the supplementary video V1, which shows an animation of the evolving cavity shape corresponding to figure 3.

In its simplest form, this mechanism is what happens if one rigid corrugated surface is dragged over another (imagine two pieces of corrugated sheet roofing moving relative to each other); in the present case, the ability of the ice to deform is significant, and the lower surface of the ice does change shape to adapt to the rigid bed underneath, which accounts for the approach to a steady state. It is then perhaps not surprising that low effective pressure N gives rises to the most sustained oscillations: deviatoric stresses in the ice are then small, leading to less rapid deformation of the ice as it moves over the bed, and adjustment to a new steady state is slower than when stresses are larger. This is particularly evident in suplementary video V1. .

I confess that the verbal explanation is a bit simplistic as the problem determining velocity is elliptic and a change in contact area slope will have an effect on uplift everywhere: I simply expect that effect to be strongest near the contact area in question, but I did not think it was wise to go into detail in the text.

As advertised, I return to the topic of oscillations at the end of section 3.3,
Note tha panels (f–j) illustrate the mechanism for overshoot oscillations described in section 3,2: the contact area on the more prominent upstream bump migrates downstream between panels (f) and (h), causing a reduced vertical velocity and subsequently a reduced cavity height being advected downstream as shown in panel (i). In fact, contact area undergoes much more significant change in size and location than in later oscillations: in panel (g), there are two contact areas, one on the larger bed protrusion upstream, and one on the smaller one downstream, while in panel (h), there is a water-filled gap with thickness above the threshold for contact identification everywhere. The main contact area on the larger bed protrusion subsequently migrates upstream again as a result of the reduction in cavity height, with a steeper average contact angle in panel (i) than (g), leading to larger vertical velocities. These in turn cause increased uplift once more, and therefore the subsequent increase in cavity height in panel (j).
I illustrate the oscillation mechanism further in figure 9, where I plot the mean conact angle of all contact areas against time in the same plot as mean cavity roof height $\bar{h}$ *and cavitation ratio* θ*. The oscillation mechanism is most clearly seen later in the interval shown: here, the contact angle shown in red peaks when* $\bar{H}$ *is increasing most rapidly, and the steadily decreases around the maximum of* $\bar{h}$*, as advection causes the downstream end of the cavity to enlarge and the re-contact point to migrate downstream. In time, that downstream migration and reduction in contact angle causes* $\bar{h}$ *to decrease again.*
Reviewer: 2. In Section 3.2, which presents the highly interesting "dynamic" run results in Fig. 3, it would help readers if you add a Supplementary Movie to accompany the figure and its textual analyses, such as those on p. 15. Since you made Fig. 2, going further to make a movie shouldn't be much more difficult. I leave this choice to you but I think that a movie will embellish the study.
Response: I've tried to create a movie of the kind suggested above,(presumably illustrating cavity shape as time progresses along the interal shown in Figure 3 (?); I hope this is instructive. For the sheet fun of it, I have repeated the exercise for figure 11 (forced oscillations in cavity size)
Reviewer: p1, line 13, "pressureized"
Response: corrected
Reviewer: p1, line 17, "possibly other variables that can be computed by a large-scale model". This is vague. At least give an example.
Response: added "...(such as mean cavity size, see e.g. Hewitt (2013) and Gilbert et al (2022))."
Reviewer: p1, line 21, change "an average" to "a spatial average"? I think this helps contextualise your subject
Response: added "...a local spatial average"
Reviewer: p1, line 23-24: the context is clearer if you insert the phrase "in the friction law " in the sentence "By contrast, basal water pressure is generally not assumed to be heterogeneous."
Response: done
Reviewer: p2, lines 7-8. "pr"? Suggestion: "The model *of* Rada and Schoof"
Response: changed to "or". Changed to "of"
Reviewer: Eqn (2): correct the punctuation
Response: done
Reviewer: p2, line 24-26: "...study instead how cavities can expand dynamically along the ice-bed interface from an access point where water is injected at prescribed pressure by an ambient drainage system". Clarify whether you're thinking in two or three dimensions. The next sentence specifies the number of dimensions, but that doesn't help us picture the idea of the current sentence.
Response: I am a little hesitant to restrict myself to a particular dimensionality at this point because the model that I formulate is in principle three-dimesional, although I end up solving it

only in two dimensions. If I say the former, the reader will be sorely disappointed ice there are no 3-D results, if I state the latter, the casual reader may come to the premature conclusion that the model is intrinsically 2-D and therefore equally intrinsically unrealistic. I have reworded this slightly, first by saying that there can be multiple access points at which water is injected *through the bed* (which may otherwise be a source of confusion, as per the review of part 1), and then by stating that the two dimensions in part 1 equate to one horizontal dimension. Discussion of the actual dimensionality of the model in part 2 is still deferred to third-to-last paragraph of section 1, where I describe the main features of the model.

The altered text for the present passage is as follows: *The present paper is part of an effort to dispense with that assumption of a perfectly permeable bed, and study instead how cavities can expand dynamically along the ice-bed interface from a access point or set of access points where water is injected through the bed at prescribed pressure by an ambient drainage system. In a companion paper (Schoof, submitted), henceforth referred to as part 1, I have used a modification of existing steady state cavity models in two dimensions (that is, with only one horizontal dimension) to study cavity expansion under quasi-steady conditions. That is, part 1 assumes an ambient drainage system with a prescribed effective pressure N that varies slowly enough for the cavity roof to be always in a steady state.*

Reviewer: p2, line 29: "varies slowly enough *in time*" – this addition would make it clearer

Response: Done

Reviewer: p2, lines 30-34: your recount of the key findings of Part 1 here comes across as rather imprecise or vague, e.g.,

- line 30 "If cavity enlargement has occurred previously and cavity size has shrunk subsequently". I can imagine that a cavity on a connected lee side that grows slightly and shrinks slightly, without extending over a bump top, also falls within this description.
- line 34 "reconnecting to an existing cavity is easier than creating a new cavity". You probably mean a particularly kind of new cavity, not a new cavity that grows on a connected lee side as N decreases to below some *high* threshold value (e.g. N* = 8 in Part 1).

Response: I have changed the text in this pargagraph to

...If cavity enlargement past a bed protrusion on its downstream side has occurred previously and cavity size has shrunk subsequently due to an increase in ambient effective pressure, then reconnection to the now isolated pre-existing cavities happens at a different set of higher effective pressure: reconnecting to an existing downstream cavity is easier than creating that downstream cavity by enlarging the upstream cavity past the bed protrusion separating the two.

Reviewer: Fig. 1: (i) improve size of the arrow for h_w and the placement of h_w; (ii) in the caption, you should add a third sentence to say something along the line of "In this figure, the large cavity meets/overlaps with the stretch P, so it is connected to ambient drainage and its effective pressure is equal to ... [and so on]".

Response: Changed figure. Added the following to the caption:

In this figure, the large cavity overlaps with the connected bed portion P: water freely enters or leaves the cavity at a pressure prescribed by the ambient drainage system through P.

Reviewer: p4, nu is used here for Poisson's ratio but also later (p8 onward) for the small parameter in the shallow approximation

Response: Thank you for spotting that. Changed aspect ratio to ε

Reviewer: p4, around Eqn (3): I think that adding one or more suitable reference for this rheology (chosen for the ice) is necessary

Response: Reviewer: p4, equation (3): I think some readers might not be familiar with the mathematical description of an elastically compressible upper-convected Maxwell fluid. Adding a citation where equation (3) is derived/explained would be very helpful.
Response: I have added a reference to the Bird (1976) Ann. Rev. paper that provides fairly comprehensive references for covariant, objective tensor derivatives in the context of finite strain viscoelasticity models.
Reviwer: Sec 2: To assure readers that the choices of rheology are sensible for the physical problem, I suggest that somewhere in this section you briefly explain why water compressibility (bulk elastic modulus about 2 GPa) can be ignored, while a compressible rheology is assumed for ice (bulk elastic modulus of 8-9 GPa; e.g. Table 1 of Neumeier (2018)), despite the stress coupling across ice–water interfaces. The reason probably is trivial and involves the very different dimensionless Maxwell times of the materials (i.e. when accounting for viscosities), but there may be other reasons.
Response: I have added the following paragraph after eq (11d): *Note that equation (11b) ignores the compressibility of water, while ice is allowed to be elastically compressible by equation (4), despite the bulk moduli being comparable (Neumaier, 2018). This is standard practice in hydrofracture models, whose validity hinges on the assumption of a shallow water layer: in that case, the displacement of the ice-water boundary that results from compression of the water column is small compared with the displacements that result from compression in the ice, simply because compressive strain in water is comparable to its counterpart in the ice, but the resulting displacement (being an integral over strain) is much smaller than in the ice.*
Reviewer: p5, line 9, lower boundary *of the ice* (useful clarification, since b + h locates the upper of the two interfaces in Fig. 1)
Response: Done
Reviewer: p5, line 20-21: "Normal stress... , as water forces its way...". I suggest rewording this sentence because it isn't clear whether the "as"-phrase presents a scenario or reason.
Response repaled "as" with "since" to clarify this is a causal relationship
Reviewer: p6, line 1: "impermeable except in specific locations at which water from an ambient drainage system can enter or exit the ice-bed gap". It would be useful if you describe explicitly (give actual examples of) what such entry/exit routes entail in this three-dimensional formulation. It is hard to picture a connection without knowing which direction or what materials are involved. p6, lines 2-3, "for the remainder" isn't clear and you should "outside P" is that meaning is intended
Response: I have reworded the paragraph as follows:
The boundary conditions above do not permit the formation of hydraulically isolated cavities, or of underpressurized contact areas that remain hydraulically isolated as in part 1. As an alternative to (10), I therefore consider a bed that is perfectly impermeable except in specific locations at which water from an ambient drainage system can enter or exit the ice-bed gap. As in part 1, I assume that there is a (typically small) highly permeable portion P of the bed through which eater can freely flow while remaining at the pressure of the ambient drainage system. Consequently, the conditions (10) hold on P (or strictly speaking, at the upper boundary of P, but since I do not model water flow through the bed, I will continue to state conditions "on P", meaning the interface of the permeable bed with a cavity or the lower boundary of the ice). For the remainder of the bed outside of P, I assume that an active hydraulic system inside the ice-bed gap redistributes water.
Following from the review of part 1, I assume the potential for confusion arises from the fact that access is *through* the bed — I mean, I could put a line through the domain and call that a "channel" on which I prescribe p_w, but that gets a bit awkward, something for a future effort in this direction?
Reviewer: p6, line 17 and Eqn (11d): is the correct symbol k or kappa?
Response: k apparently. Corrected.

Reviewer: p7, line 29, physics *of* (?) ice-bed contact areas
Response: "of" would have been corrected. The sentence has gone as the result of a re-write motivated by the other reviewer.
Reveiwer: p7 (Eqns 11 e, f & g & Eqn 12): all sigma's and p's in this formulation differ from those in Part 1 where they had cryostatic overburden subtracted. I think that you should point this out in this section (even if any of the later analysis employs the subtracted version).
Response: I have added the following note below equations (10), where the issue should first become apparent:
(Note also that the model here is formulated in terms of total Cauchy stress, while part 1 uses a reduced pressure, from which overburden has been subtracted. I introduce that reduction of stress in the next section.)
Reviewer: p8, the equations on this page lack numbering. Is this deliberate? Please check the journal's formatting guidelines.
Response: I suppose it was deliberate, with these equations playing a different role, but I've added numbering in order not to cause trouble.
Reviewer: p8, in the final scaling relation, it may be better to symbolise the water thickness scale by $[h_w]$, as h symbolises the interfacial elevation (which is treated in the second-last scale relation).
Response: I think this was deliberate, since there is no separate scale for the two (unless h_w is intrinsically small compared with h, but that would only occur if all cavities are "dry" and at the triple point pressure). It seems preferable to me to introduce only one scale rather than a second redundant one, for which I would simply end up defining $[h_w] = [h]$ somewhere later. Note that I've likewise only defined one scale for the three different velocity components and coordinates.
Reviewer: p8, line 13: "defined" (towards end of line)
Response: corrected.
Reviewer: p8, line 14: by "forcing", do you mean "ambient"? Consider writing "ambient water water (which is used in this study as a forcing factor).
Response: replaced with "ambient drainage system water pressure"
Reviewer: p9, Eqn 13a and preceding line: as mentioned for page 4, here you seem to be using nu for both Poisson's ratio and the small "shallow" parameter
Response: Replaced (see above)
Reviewer: p9, Eqn 13f: my attempt to derive this gives u-bar and v-bar instead of u1 and v1 in front of the derivatives. Please check.
Response: That is correct. The other reviewer also flagged this (in the context of the moving frame transform removing advection terms). I've corrected the text; the code does not contain this error, which I suspect was the result of cutting-and-pasting from equation (9)
Reviewer: p9, is there an Eqn 13g?
Response: No, just an errant pair of backslashes. I've removed them (note however that hte equation numbering has changed anyway, so this refers to the new equations (16))
Reviewer: p9, Eqns 15c and 15d and next line: the conditions here seem to switch back into dimensional terms (for p_w at least), which comes across as confusing; that is, the p_w here doesn't seem to be the p_w in (13e), which I think is dimensionless. Please check.
Response: I think this is an unfortunate result of the convention of dropping asterisks on dimensionless quantities, p_i here was actually the dimensionless p_i^*, and equations)15c–d) (now (18d–e) were actually dimensionless. To avoid that notational pitfall, I have replaced p_i^* by Σ_0
Reviewer: p9, Eqn (13e) for $sigma_{33}$ at $x_3 = 0$ seems to conflict/overlap with Eqn (14) (applied also at $x_3 = 0$). Perhaps (13e) is replaced by (14) and/or it doesn't apply everywhere along $x_3 = 0$?

Response: Technically I don't think (13e) in the original paper was not in conflict with (14), if you go with the interpretation of p_w in contact areas defined in section 2.1 ("I continue to interpret p_w with compressive normal stress at the bed [...] even if no water is present", equaion (11c) of the original paper — meaning, p_w is then not to be understood as a water pressure. The upshot is that p_w can then differ from the ambient drainage pressure even in P if there is a contact area. This is clearly confusing (the other referee raised the same point), and I would attribute that confusion to the notation used as well as the original description of p_w^* given below the (originallu unnumbered) equations defining the dimensionless variables.

In the updated manuscript, I have defined the dimensionless reduced normal stress (reduced in the sense of having removed overburden) by p_n as a more common symbol for compressive normal stress, that is

$$p_n^* = \frac{p_w - p_i}{[\sigma]}.$$

I have changed the verbal definition of p_n^* below equation (15) to the following *N^* is the (scaled) usual effective pressure defined as the difference between overburden and the water pressure in the 'ambient' drainage system to which the bed is connected in the permeable regions P, while p_n^* is a reduced normal stress, defined as the difference between local normal stress p_w (the latter being equal to water pressure where water is present between ice and bed) and overburden. Where water is present, p_n^* is then the negative of the effective pressure defined in terms of* local *rather than ambient drainage system water pressure.* I've written the conditions on P in equations (17) (of the revised manuscript) explicitly in terms of p_n, as well as writing the water flux in terms of p_n. The reason for retaining p_n is that it makes the numerical implementation of the inequality constraints in both (17) and (18c–d) simple, playing the same role as de Diego et al's (2022) Lagrange multiplier λ.

Reviewer: p9, line 24, τ_b^* — you wrote earlier that asterisks are dropped

Response: Changed to plain τ_b

Reviewer: p10, awkward on lines 22 and 32 where the text switches back to referring to dimensional quantities when describing the numerical method of solving the dimensionless model of the last page

Response: I will defend line 22 on the basis that the structure after semi-discretization is that of a generic compressible elastic problem, for which there is no good dimensionless template in the text but there is in the later citation to Kikuchi and Odean. I have rephrased this slightly as

... takes the mathematical form of a compressible linear elasticity problem, with velocity taking the place of displacement, and "elastic" moduli that differ from the usual E and ε (which would become 1 and ν in dimensionless terms): the effective moduli in fact depend on step size δt as well as τ_M and ν.

On line 32, I have changed the text to *... I use an upwind scheme for* $\mathbf{q}$, which would have been the correct thing to say to begin with.

Reviewer: p11, line 14: the description here "code is implemented for both two- and three-dimensional domains" is a little confusing as the next line indicates that the code isn't used for three dimensions. The difference between "implementation" and "use" isn't clear.

Response: Changed "implemented" to "written so it can be used in both two- and three-dimensional domains"

Reviewer: p11 line 22: on declaring these choices for a and h0, it is useful to say that they make the N (dimensionless) in this manuscript directly comparable to N* in the Part 1 manuscript, as the effective pressure scalings are then the same. Section 3.1 later doesn't clarify this matter when comparing Part 1 and Part 2 results.

Response: changed to

...which is identical to equation (10) of part 1 with $h_0 = 1$, $a = 2\pi$, and therefore makes the dimensionless parameter N here be the direct equivalent of N^* in part 1.
Reviewer: p11, line 23: hiccup after "In that"
Response: Changed to "In that case,..."
Reviwer: p12, line 2: if I have guessed the intended sense here correctly, I would expect to read "highest" rather than "lowest" in this phrase. Please check.
Response: Quite so. Changed to "highest"
Reviewer: p12 line 12, spurious curly bracket [note: I'm counting downward from line 5]
Response: Corrected.
Reviewer: p12, line 13: this lead phrase ("As measures... that ... ")doesn't seem grammatical [Again counting downward from line 5]
Response: In my defense, if I had written "As measures of cavity size, I compute cavitation ratio and mean water deoth...", you might have been less perturbed. I've changed this passage to
... two commonly used measure of cavity size are mean cavity size $\bar{h}$ and cavitation ratio θ (Thøgersen et al, 2019). I compute both of these from the following formulae,

$$\bar{h}(t) = \frac{1}{a}\int_0^a h(x,t)\mathrm{d}x, \qquad \theta(t) = \frac{1}{a}\int_0^a H(h(x,t) - h_\epsilon)\mathrm{d}x$$

where H is the usual Heaviside function.Note that θ is simply the fraction of the bed that is cavitated, since $\theta = a^{-1}\sum(c_j - b_j)$, the sum being taken over all cavities in one bed period. Both θ and $\bar{h}$ could be used to parameterize cavity geometry in a large scale subglacial drainage model (the scale of individual cavities being "microscopic" in these models, see ...)
Reviewer: p13, lines 3-4, while I understand this opening sentence, it would help readers if you add a sentence or insert a phrase to clarify whether Fig. 2 shows solutions in the moving or absolute frame of reference
Response: Changed to
The solutions of the dynamic model (plotted against the original, as opposed to moving, coordinate x in figure 2)...
Reviewer: p13, line 11, unclear what "the latter" refers to; clarify
Response: My understanding was that "the latter" would refer to the last noun used ("cavity") in an effort to avoid recycling that noun. I've changed this to
At the instant when a cavity becomes isolated, that cavity is generally not in steady state ...
Review: Fig 2 caption, line 2, the phrase "the bed b is shown in grey" confuses b (the bed surface) with the bed interior (described as grey in colour)
Response: Thank you for forcing me to be consistent. I have taken out the "b" here.
Reviewer: p15 & Fig 3: perhaps this will be said later, or I've missed it. Although your focus on p15 is on the oscillations, it is useful to point out that the asymmetric response in Fig. 3 (h-bar doesn't stabilise towards the same final value when N is step-changed to a certain value from different directions in this run) is related to the "irreversibility" of new cavity formation reported in Part 1 for the partially permeable case. This is in contrast to the reversible behaviour in Fig. 5 (fully permeable).
Response: I have added the following paragraph towards the end of section 3.2
While the dynamic behaviour of the fully permeable bed case is similar to the impermeable bed, there are two notable differences. First, as in the case of reconnection of a previously isolated cavity for the impermeable bed case in figure 4, drowning of the smaller bed protrusion for the permeable bed does not cause the significant overshoot oscillation that is apparent at $t = 78$ in figure 3. Second, the

irreversible nature of cavity expansion at that point in time in figure 3 is absent for the permeable bed case in figure 5, confirming the steady state results of part 1.

Reviewer: p15 last paragraph: you caution about the nature of the simulated oscillations at lowest N. But elsewhere in this section, you don't explicitly say whether you interpret the simulated oscillations at higher N (in fig. 3 and later figures) to be 'real', not dominated by numerical artifact — although the writing seems to imply 'real'. Please clarity as a suitable place.

Response: I have rearranged the relevant material and added an additional figure to address this (prompted by similar comments from the ohter referee). I have managed to run an abbreviated version of the of the computation in figure 5 having halves the cell size along the bed (the fully permeable bed case of figure 5 is easier to solve, and exhibits what appear to be the same kind of oscillations in $\bar{h}$ as are evident in figure 3). The comparison between the solution with standard and double resolution is shown in the new figure 6, and described in the last paragraph of section 3.2 (for the sake of a better flow of the text, I also moved the material in the sixth paragraph of the original section 3.2 (starting with "Repetition of an earlier note of caution…") to the penultimate paragraph. The final two paragraphs of the updated section 3.4 state

The cavitation ratio is very close to unity (typically around 0.96–$0..98$*) for the long-lasting oscillations at low N identified above (between* $t = 258 - 420$ *and* $t = 200 - 260$ *in figures 3 and 5, respectively). With such a small contact area, only about 3–6 nodes in the finite element mesh are in contact with the bed. (Note also that the numerical method treats a bed cell as either separated from the bed with* $h > 0$*, or in contact with* $h = 0$*, and the cavity end point location therefore jumps in increments of a single cell size, giving the plots of* θ *and of cavity end point location against* t *a non-smooth appearance, while the mean ice-bed separation* $\bar{h}$ *is much smoother.)*

A very small number of nodes in contact with the bed raises the question of numerical artifacts. A comprehensive study of mesh size effects is beyond the scope of the work presented here. Due to the limitations of working in a MATLAB coding environment, it is difficult to refine the mesh signficantly beyond what is used in the computations reported above. For the case of a fully permeable bed (which typically permits larger time steps), I have been able to refine the mesh to double the number of nodes on the bed for a relatively short computation. A comparison for a shortened version of the computation in figure 5 is shown in figure 6. While there are differences, these are mostly in the detail: the cavitation ratio time series is significantly smoother for the higher resolution results (as might be expected), and the oscillations in $\bar{h}$ *are also somewhat smoother. There are however no dramatic changes of the kind that one might expect for a mesh that is effectively very coarse around the contact area, lending confidence to the conclusion that the sustained oscillations in* $\bar{h}$ *at low effective pressure are a robust feature of the solution.*

Reviewer: p16, lines 10-12 (irrelevance of viscoelasticity in Fig. 5): I have been wondering about this when reading p13-15. Can you please clarify whether viscoelasticity is also insignificant in the runs in Figs. 3 and 4 (besides 5) — in causing the oscillations — if that is true?

Response: I have tried to address this in the fourth paragraph of the updated section 3.3, because I think the issue fits most naturally there:

Importantly, this initial "hydrofracture" (which is not hydrofracture in the true sense, as it corresponds to a pre-existing fracture being re-opened) has very limited extent. In fact, the same initial fact occurs every time that N goes through a step change, regardless of whether the cavity expands significantly afterwards. For step changes in N that do not lead to large-scale expansion by drowning of a smaller lee side protrusion, that brief "hydrofracture"episode is the only part in the process of cavity enlargement that involves elastic effects (in the sense of occuring over a shorter interval than the Maxwll time).

Reviewer: Fig 4b panel: to help readers, please add the labels "cavity" and "contact", as done in

fig 3; one or two places would do.
Fig 5b panel: to help readers, please add the labels "cavity" and "contact", as done in fig 3
Response: Done.
Reviewer: Fig 6 caption, line 2: is "18" a typo?
Response: Yes. it looks like this was an unintiential cut-and-paste of an equation reference.
Reviewer: p19, lines 5-7 (delayed/final rapid increase in $\bar{h}$): Unlike the earlier phases of the evolution, for this final phase/part of $\bar{h}$ rising, you don't give or hint at any physical mechanism. What controls or causes it? Or what delayed it, causing it to lag behind the rapid rise in theta-bar in Fig. 6a? Does the cause involve water transfer?
Response: The intiial rapid growth in θ while $\bar{h}$ only changes by a small amount is an example of hydrofracture, with an associated time scale controlled by water flow (as described in the context of the first hydrofracture "event")¿ The slower exapnsion thereafter is viscous. I've tried to clarify this in the eighth paragraph of section 3.3:
...It is only during this slower expansion that the cavity depth $\bar{h}$ increases more rapidly this phase is much longer than a single Maxwell time and is again associated with viscous deformation of the ice.
Reviewer: p19, line 14: in this passage it is worth pointing out also the brief recontact seen in panels g and h
Response: I have already addressed this in the context of changing the text in response to the first main point in this review, see above.
Reviewer: Fig 8d: most steps in N have vertical lines. Add vertical line for the step at $t = 260$?
Reviewer: This is a little odd; in my copy, a vertical line does show up at $t = 260$
Reviewer: Fig 8 caption, line 2: hiccup in P value. Last line: I suggest moving "at $t = 260$" to elsewhere in the sentence
Response: Corrected P. Moved $t = 260$ to say "...and departs at $t = 260$ from the solution not indicated by arrows"
Reviewer: p20, line 1: columns of 9? figure 9?
Response: Corrected.
Reviewer: Paragraph across p20-21: this description seems brief for the interesting result in column 1 of figure 9. If I'm reading Fig. 9a correctly, the connected cavity is longer (larger?) when water pressure (effective pressure N) is lower (higher)? Is this phase relation due to a time delay originating from viscous flow? Can you venture to say more?
Response: I think (based on having previously commented out the passage I have now reinserted) that I had not gone deeply into this because the mechanism had been briefly identified in the literature. I have re-inserted the omitted passage (with appropriate referencing) to say
In column 1, a relatively small isolated cavity forms before periodic behaviour is established. That cavity then remains isolated throughout the pressure cycle. The effective pressure N_M in that isolated cavity is in antiphase with the forcing effective pressure N in the connected cavity. This behaviour is familiar from field observations in parts of the glacier bed that are not hydraulically connected (Andrews et al, 2014,Rada and Schoof, 2018). A simple way to interpret the antiphase pressure variations is in terms of the portion of overburden supported by the isolated cavity (Murray and Clarke, 1995, Lefeuvre et al 2015): when forcing effective pressure N is low, a larger fraction of overburden is supported by the connected cavity, reducing normal stress on the isolated cavity, and therefore also reducing water pressure in the cavity, which corresponds to a higher effective pressure N_M (defined as overburden minus water pressure in the isolated cavity)
Reviewer: p23, line 6, the value here (1.0653) differs from that in Fig. 2a-b
Response: The digit "6" appears to be a typo here. Corrected.

Reviewer: p23, line 10, "will also"; "will" seems redundant
Response: Changed to "also confirms"
Reviewer: p23, line 14-15, the message delivered here is "the subsequent growth of mean cavity depth h-bar... and of the cavitation ratio theta-bar ... causes a hydraulic connection to be established", but I don't think that it makes physical sense to consider these as cause and effect. (The next sentence seems to be fine as it uses the word "predictor", which conveys a correlation, not physical causation.)
Response: This is hopefully just awkward wording: the growth in $\bar{h}$ and θ does correspond to an enlargement of the cavity to the point where it is no longer confined by high normal stresses around it (see part 1) and grows past the lee-side bed protrusion. I've changed the paragraph to say
... These insights are however misleading in a dynamic situation: figure 2 shows that the it is not the instantaneous drop in N below some critical value that causes a hydraulic connection to be established. Instead, a drop in N causes mean cavity depth $\bar{h}$ and cavitation ratio θ to grow. That growth eventually allows hydraulic connection as a bed protrusion on the downstream side is "drowned".
Reviewer: p23, 2nd and 3rd paragraphs: these paragraphs seem to be written to address the context that (/the question whether) a specific variable threshold can be used (in macroscopic drainage models) as proxy for connection. These paragraphs will work better if you outline or remind us of the context at their start; doing this will serve to help the whole section. Currently this context emerges slowly, and I have long forgotten it since Sec. 1.
Response: I have changed the start of the second paragraph of section 4.1 to
One might therefore be tempted to parameterize cavity connection in large-scale drainage models in terms of effective pressure N reaching a threshold value. The insights from steady state calculations are however misleading in a dynamic situation: ...
Reviewer: p23, line 25: "having a simple critical value hc" – for what purpose?
Response: Reworded to
A plausible alternative to having a simple critical value h_c for cavity connection in a large-scale model is to recognize that...
Reviewer: p23-24: on these pages, you should highlight that here you're attempting to derive insights for drainage modelling in (I presume) three-dimensions from simulated behaviour in two dimensions. I am not sure that this translation from one to the other necessarily applies; the text on these pages conveys it as automatically valid for all aspects being considered. (This issue is linked to – but not the same as – the general limitations of using a two-dimensional model.)
Response: I agree with all but the last sentence here (in parentheses) — presumably if I had a local 3-D model, I would learn about issues of connectivity in two horizontal dimensions. I suppose you could argue that I might find that connectivity is anisotropic, so the permeability of large-scale drainage models would have to be a tensor, with more complicated criteria for connectivity along different principal axes. I think that would be a very speculative thing to bring up here. Either way, I was hoping to defer discussion of 3D models and the difference that you might see in them to section 4.3; I don't think I have much to add to what I wrote there, certainly not in a way that isn't confusing.
Reviewer: p25, line 9, here you refer to the shape and volume of an "isolated" borehole. Do shape and volune matter because we are considering a borehole that has closed at the top by ice deformation? Please clarify in the text
Response: In my experience, boreholes usually freeze shut before they close due to creep, but then I work on relatively shallow polythermal glaciers. I have changed the wording here to
which itself is of unknown shape and must preserve its volume (assuming the borehole has closed,

as is typically the case, see e.g. Rada and Schoof 2018) while subject to non-uniform stress field at the bed.

Reviewer: p26, lines 4-6: in this passage, what end-to-end connectivity means is obscure to me.

Response: I have clarified this (hopefully) by adding text to say

end-to-end connectivity (meaning, water is free to flow from one side of the domain to the other)