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 contact 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 interval 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-dimensional, 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 paragraph 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 replaced "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", equation (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 (originally 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 depth...", 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 other 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 significantly 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 unintiontial 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 initial 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 expansion 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 volume 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)

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

Christian Schoof

June 15, 2023

1 Referee #2

Reviewer: I find the explanation of boundary conditions in section 2.1 after line 17 very difficult to understand. You say there are two alternatives for closing the system of equations for the viscoelastic fluid. What I understand is that these two alternatives are either (10), which you enforce on P (or the whole bed when you consider a fully permeable bed), or (11e), which you enforce on the complement of P. In the case of (11e), the water pressure is given by the equations for the water column described previously. Is this correct? If so, I would consider rewriting this part of section 2.1 such that you first show the two alternatives (10) and (11e), and then explain how the water pressure p_w is modelled in the complement of P. I find this much clearer because what we need to close the equations for the ice dynamics are normal velocity and normal stress boundary conditions.
Response: I have done a fairly major re-write of the boundary conditions, including the suggestion above. In order to make the model for p_w more self-contained, I first stated that p_w is normal stress at the bed as requested, and then deferred all the discussion surrounding large gap permeability K as well as of what happens when h vanishes until after the model is complete. I have tried to give a simple counting argument along the way to make clear that there is nothing missing, although this hardly counts as a proof of existence of solutions (nor is it intended as such)
In full, the revised statement of boundary conditions runs as follows;
To close the problem, I require one additional boundary condition. I consider two alternatives. First, the standard assumption in dynamic models of subglacial cavity formation has been that the bed is rigid yet highly permeable, with a prescribed water pressure p_w^0 everywhere. That assumption is also part of the steady state model by Fowler (1986) and Schoof (2005) that I previously generalized in part 1. Normal stress cannot drop below that water pressure, as water forces its way between ice and bed and opens a gap or cavity. A fully permeable bed gives a boundary condition on normal stress in the following either-or form (Durand et al 2009, Stubblefield et al 2021, de Diego et al 2021)

$$-\sigma_{ij}n_in_j = p_w^0 \qquad \text{if } h > 0 \text{ or } \left(h = 0 \text{ and } \frac{\partial h}{\partial t} > 0\right) \tag{1a}$$

$$-\sigma_{ij}n_in_j \geq p_w^0 \qquad \text{if } h = \frac{\partial h}{\partial t} = 0, \tag{1b}$$

signifying the possibility that compressive normal stress can exceed water pressure where ice is in contact with the bed, and a gap is not about to form: put more simply, in contact areas, normal velocity is prescribed, while in areas with an ice-bed gap, normal stress is prescribed, and the inequality constraints above serve to determine which boundary condition applies where (see also Stubblefield *et al , 2021).*

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 (1)*, 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. Specifically, I assume that there is a (typically small) permeable portion P of the bed at which* (1) *holds, while for the remainder, I assume that an active hydraulic system inside the ice-bed gap redistributes water.*

Specifically, I assume that there is a water column of evolving height h_w inside the ice-bed gap, constrained by $0 \leq h_w \leq h$. Assuming negligible deviatoric normal stress in the water column, local force balance demands that water pressure p_w in that water column (not to be confused with the prescribed ambient drainage system pressure p_w^0, which generally differs from p_w) is given by normal stress at the bed,

$$-\sigma_{ij} n_i n_j = p_w. \tag{2a}$$

Outside of the permeable portion of the bed, there is no water supply, so p_w is not prescribed a priori, but the water column height satisfies a depth-integrated mass conservation equation of the form

$$\frac{\partial h_w}{\partial t} + \nabla_h \cdot \mathbf{q} = 0, \tag{2b}$$

which should be understood in weak form, permitting mass-conserving shocks where necessary. Here $\mathbf{q} = (q_1, q_2)$ is a two-dimensional flux and $\nabla_h = (\partial/\partial x_1, \partial/\partial x_2)$ is the corresponding two-dimensional divergence operator. I assume that the ice-bed gap is shallow (an assumption that I formalize in the next section), and I therefore relate the depth-integrated water flux $\mathbf{q}$ to water column height h_w and an along-bed gradient in water pressure $p_w(x_1, x_2, t)$ as

$$\mathbf{q} = -K\left(h_w, |\nabla_h p_w|\right) \nabla_h p_w + \frac{1}{2}\mathbf{u}_h h_w \tag{2c}$$

where $\mathbf{u}_h = (u_1, u_2)$ is the horizontal component of velocity at the base of the ice, and K is a two-dimensional "gap permeability", which I take to be give by Darcy-Weisbach or Manning-Gauckler power law formulation (see e.g. Werder et al, 2013), of the generic form

$$K(h_w, |\nabla_h p_w|) = k_0 h_w^\alpha \left|\nabla_h p_w\right|^{\beta - 1} \tag{2d}$$

with $\alpha > 1$, $\beta = 1/2$ and $\kappa_0 > 0$ constant. Note that the above also covers the case of laminar Poiseuille flow if $\alpha = 3$ and $\beta = 1$. The second term in equation (2c) *is the contribution of shear to water flux, which remains negigible in all computations reported here.*

Note that equation (2b) *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.*

To avoide the negative fluid pressure singularities common to hydrofracture models (Spence et al 1985, Tsai and Rice2010, 2012), I permit a "fluid lag", in the form of a vapour-filled space between water and ice when water pressure drops to zero (or more strictly, the triple-point pressure of water, which I treat as negligibly small compared with stresses in the ice). This means that fluid depth h_w

and ice-bed gap size h are related through one of the following two possibilities,

$$\text{either } 0 \leq h_w = h \qquad \text{and } p_w > 0, \tag{2e}$$

$$\text{or } 0 \leq h_w \leq h \qquad \text{and } p_w = 0, \tag{2f}$$

and p_w cannot be negative.

The first possibility, condition (2e)*, states that there cannot be a vapour-filled gap between ice and water (of thickness $h - h_w > 0$) if fluid pressure is above the triple-point pressure, in the sense that ice, water and vapour cannot then coexist. This is the default state and correspods to a completely fluid-filled ice-bed gap, as is the case in the canonical picture of subglacial cavities. By the second condition* (2f)*, a water filled gap is possible but need not exist at the triple-point pressure; given the substantial overburden pressure, this is only likely to be reached near the tips of cavities that are in the process of expanding rapidly (e.g. Tsai and Rice, 2010).*

As far field boundary conditions, I consider prescribed normal and shear stress, in the form

$$-\sigma_{33} \to p_i, \qquad \sigma_{13} \to \tau_b, \qquad \sigma_{23} \to 0 \tag{3}$$

as $x_3 \to \infty$, where p_i is overburden and τ_b is the usual 'basal shear stress' of the theory of basal sliding (Fowler, 1981). In addition, I assume the domain is laterally periodic, with period a in both horizontal directions.

The basal boundary conditions for the classical caviation problem with a permeable bed consist of (13d), (13f) and (1)*. The stress and normal velocity conditions in (13d) and* (1) *are sufficient to close the force balance problem (13c) (see de Diego et al 2021,2022 , Stubblefield, 2021, for the equivalent purely viscous problem), while the kinematic boundary condition (13f) serves to determine the gap width variable h that appears in the contact conditions* (1)*.*

By contrast, the equivalent set of boundary conditions for an impermeable bed given above introduces local fluid pressure p_w and fluid depth h_w as variables defined at the boundary, in addition to the gap width h. A simple counting argument shows that the equations (13d) and (13f) combined with (2b)–(2f) *close the problem: the force balance relation (13c) requires three boundary conditions, which are supplied by equations (13d) and* (2a)*. The fluid pressure p_w that features in equation* (2a) *is determined through the mass conservation problem* (2b)–(2c)*. The latter constitute a single equation in fluid depth h_w and pressure p_w, where h_w and gap width are determined through the kinematic boundary condition (13f) and whichever one of the two conditions* (2e)–(2f) *applies, leading to a total of three equations to specify the three variables p_w, h_w and h.*

The counting argument of the previous paragraph is of course simplistic: the determination of p_w, h_w and h couples back to the force balance problem through the velocity components in the kinematic boundary condition. Note also that isolated cavities (the object of our study) are only present if the gap width h is either zero or extremely small between those cavities and the permeable bed portion P. The formulation above incorporates such regions provided the permeability K vanishes when fluid depth h_w does (as it must where the gap vanishes, since $h_w \leq h$). In the interior of a region where the ice-bed gap vanishes (that is, where ice is in contact with the bed), water flux vanishes and hence $\partial h_w/\partial t = 0$ from equation (2b)*. Note that, since there is no water column present in that case, the variable p_w does not represent an actual fluid pressure in such regions, but simply equals the compressive normal stress.*

From the gap width relations (2e)–(2f)*, there are then two possibilities in the interior of regions where $h_w = 0$: either h remains at zero and the kinematic boundary condition (13f) reduces to condition of vanishing normal velocity, so $u_3 = u_1 \partial b/\partial x_1 + u_2 \partial b/\partial x_2$ and ice remains in contact with hte bed, or alternatively normal stress drops to the triple-point pressure and a vapour-filled* *cavity forms. The combination of equations (13d), (13f) and (15)–*(2f) *can therefore describe not only the physics of a water layer separating ice and bed, but also the physics of ice-bed contact areas as required.*

In practice, only very small pressure gradients should be required in order to move water fast enough to fill the ice-bed gap as the latter evolves due to ice flow. That situation corresponds to the limit of a large gap permeability K (or better still, of large k_0): the flux relation (2c) *then simply serves at leading order to impose a spatially uniform water pressure in each basal cavity, as is also the case for the classical cavity model using the permeable bed boundary conditions* (1)*. In that case, shear in the water column also plays an insignificant role, and I retain the second term $\mathbf{u}_h h_w/2$ in the definition of flux in equation* (2c) *here simply to make the switch to a moving coordinate frame employed in section 2.3 more self-consistent (since an advective term will automatically appear under the change to a moving frame).*

referee I also have trouble understanding part of section 2.2, between lines 11 and 21. In line 14 you write: "imposes the boundary condition (14) only when $(x_1, x_2) \in P$ is in a part of the bed to which the ambient drainage system has access". Does $(x_1, x_2) \in P$ already imply that that point is on a permeable point and therefore has access to the ambient drainage system? I also see that condition (11e) is written as (13e) in the non-dimensional system. However, this is in conflict with condition (14). Shouldn't you include (13e) in (15)?

Response: Yes, $(x_1, x_2) \in P$ says that the point (x_1, x_2) is in the permeable part P of the bed. I've reworded this bit as

The second, which I refer to as an impermeable bed, imposes the boundary conditions (14) only for points $(x_1, x_2) \in P$ (that is, for points that lie in a part of the bed to which the ambient drainage system has access). Flow of water occurs only through the ice-bed gap otherwise, satisfying...

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", equation (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 (originally 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 usual (but scaled) 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: In equation (12) of page 8, you write far-field conditions for the basal shear stress. Previously, in Schoof (2005), you enforced far-field conditions for the velocity. Why do include this new far field boundary condition here? I think it could be interesting to include an explanation for this choice of boundary condition in the paper.
On this note, I am also confused about the sliding velocity variable u_b. You compute horizontal velocity perturbations u to this horizontal motion, yet you do not enforce the far field condition that $u \to 0$, right? In this case, the actual sliding velocity is $u_b + u$ as $x_3 \to \infty$. I think it would be clarifying to mention this.
Response: It turns out that the two conditions $\sigma_{13} \to 0$ and $u \to 0$ give the same answer in the model of Schoof (2005). I have tried to make this clear in a new paragraph after equation (16):
Note that the condition $\sigma_{13} \to 0$ imposed here does not conflict with the alternative condition $u_1 \to 0$ used for instance in Schoof (2005): in the purely viscous model in the latter paper, σ_{13} behaves as $\partial u_1/\partial x_3$ in our present notation, and $\sigma_{13} \to 0$ implies $u_1 \to$ constant. Setting that constant to zero simply removes the indeterminacy of u_1 in the model above (consisting of equations (13)–(16), which arises because the latter remains invariant under adding a constant to u_1: that indeterminacy needs to be resolved by going to higher order, but does not affect the leading order sliding velocity since u_1 is a small correction to the sliding velocity $\bar{u}$ since $[u]/u_b = \varepsilon \ll 1$: the total velocity is $u_b + \varepsilon u_1$, and therefore remains equal to u_b at leading order regardless of what finite value u_1 approaches as $x_3 \to \infty$.
Reviewer: In the numerical method, do you solve for velocity, stress, water pressure, cavity height and water height simultaneously? Or do you use any staggering of variables in time? I think it could help future researchers who wish to re-examine this problem to have access to the code you used.
Response: I use "backward Euler step" in the usual sense of a fully implicit time step. There is a minor caveat, namely that conservation of water along the bed is solved using an upwind scheme (to avoid extracting water from finite volume cells that contain no water). Simple upwinding is discontinuous and therefore not realistically possible to combine with a backward time step, so the upwind direction is inferred from the previous time step (and I hope that in future there will be better ways of doing this! — since the scheme I use often requires quite small time steps to prevent the upwind direction from flipping back and forth. I make this explicit in the following additional sentence in the second paragraph of section 2.3,
The time step is fully implicit except for the use of upwinding in the discretization of the mass balance equation (18a), in which we define the upwind direction *based on the direction of $\nabla_h p_w$ after the previous time step.* I am happy to make the code available.
Reviewer: The overshoot in the mean cavity size $\overline{h}$ in e.g. Figure 3 is extensively commented throughout the paper. At some points you suggest it could be a numerical artifact, yet you refer to it to argue that cavitation ratio and ice-bed gap are not good proxies for each other (page 15, line 20). Therefore, it seems important to explore whether such oscillations are physical or numerical. This is obviously a difficult task and a full analysis of this phenomenon is out of the scope of this paper. However, a simple computational test would be to compute the cavity height after a step change in effective pressure for different meshes and time steps. If these oscillations were physical, we would expect the cavity height evolution to converge for decreasing mesh size and time steps. I suggest these computations be included in the paper, perhaps in an appendix. It would be very interesting to see this comparison for a case where dramatic oscillations occur, as in Figure 3 at $t = 78$.
Response: This is a very good point, though the proposed numerical test is simple at face value, but turns out not to be so simple in practice, if you're foolish enough to have coded the problem

in MATLAB. 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, although not the extreme ones associated with rapid cavity expansion past a lee-side obstacle). 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 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 significantly 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:, p1, line 13: "pressureized" ¿ "pressurized"

Response: Corrected

Reviewer: p2, line 7: Unclear about meaning of "pr". Do you mean "i.e."?

Response: I meant "or", apparently. Corrected.

Reviewer: p2, equation (2): Add full stop.

Response: Done.

Reviewer: p3, line 10: "practical computational reasons" - What does this mean exactly? Excessive computational cost, unsuitable numerical model for 3D, difficulties in formulating/implementing computational tests? This should be clarified here.

Response: I have reworded this as

Because the MATLAB code I have written is not suitable for full parallelization, I have however not been able to run the model in three dimensions except for very coarse meshes, leaving an obvious avenue for future research.

In plain text, I have been able to run the code in a multithreading mode, using available processors and RAM on a single node, but that is insufficient for useful computation in 3D. It may be possible to make this work in MATLAB, but I haven't gotten there, and it may be plain better to recode in something more suitable.

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.
Reviewer': p4, line 13: close brackets.
Response: Done
Reviewer: p4, line 19: "then with the change in stress related to the corresponding linearized strain as (eq)". Rephrase this clause, it is phrased incorrectly.
Response: Reworded as
... then the change in stress is related to the corresponding linearized strain as ...
Reviewer: p4, line 28: Avoid initiating sentence with mathematical symbol.
Response: Moved "here" to the start of the sentence.
Reviewer: p5, line 11: "ensures ensure" ¿ "ensures"
Response: Corrected.
Reviewer: p5, line 17: Consider rephrasing the sentence "First, the standard assumption in dynamic models of subglacial cavity formation (references) has been ...". Perhaps write "First, we consider the standard assumption in dynamic models of subglacial cavity formation (references), which has been ..."
Response: Changed to
First, I consider the standard assumption in dynamic models of subglacial cavity formation, namely that the bed is rigid yet highly permeable, with a prescribed water pressure p_w^0 *everywhere.*
Reviewr: p5, line 26: "in contact areas, normal velocity is prescribed". If by contact areas you mean areas where $h = 0$ (which is the most intuitive definition), this sentence is not correct. We will also have contact areas which are about to detach, $\partial h/\partial t >= 0$. In this case we prescribe the normal stress and compute the normal velocity.
Response: That is indeed correct; in fact the passage describing the role of inequality constraints was also misplaced. I have reworded the relevant bit to say
put more simply, in contact areas, normal velocity is prescribed so long as compressive normal stress exceeds water pressure, or else, normal stress is prescribed if the ice is about to detach from the bed, and the inequality constraint serve to determine which boundary condition applies where (see also Stubblefield et al, 2021). By contrast, in areas with an ice-bed gap, normal stress is always prescribed.
Reviewer: p6, equation (11b): Add comma.
Response: Done.
Reviewwer: p6, line 21: "flux q" ¿ "the flux q"
Response: I think we agreed on this in part 1 as well. I'm still going to argue that, flux being uncountable, leaving out the article as legitimate here. No doubt the copy editor will have their say if the paper makes it that far.
Reviewer: p6, line 21: Consider rewriting "... by the first, pressure-gradient-driven term". Perhaps "... by the first component of the flow, the pressure-gradient-driven term".
Response: This passage has disappeared as part of the larger re-write of the text on boundary conditions.
Reviewer: p 7, line 9: "implying that a source term that is omitted in (11a)" - this clause does not make sense, please correct.
Response: Ditto.
Reviewer: p7, line 29: "also capture the physics ice-bed contact areas" - Typo?
Response: Ditto

Reviewer: p8, line 23: "N* is the (scaled)..." - Avoid starting sentence with mathematical symbol
Response: Merged with previous sentence to say
while N^* is the usual (but scaled) effective pressure
Reviewer: p9, equation (15b): Add comma.
Response: Done.
Reviewer: p10, line 13: Specify that the mixed finite element method is used to solve for the velocity and stress variables. A mixed FEM is used in Stubblefield2021 and deDiego2022 to solve for the velocity and pressure and the velocity, pressure and normal stress at the bed, respectively.
Response: I have added a note to say
(There is a technical difference here in the sense that the latter authors use mixed finite elements in velocity, pressure and normal stress at the bed, whereas the compressible problem considered here naturally calls for mixed finite elements in velocity and the full Cauchy stress tensor; key to handling the boundary conditions is the use of mixed elements for normal stress at the bed.)
and removed the Stubblefield et al reference in this single place, since that decomposition appears to be unique to the de Diego et al work.
Reviewer: p10, line 19: I can see how a moving frame eliminates the advection terms in (13a), since these are advected by $(\overline{u}, 0)$. However, I do not see how the advection terms disappear in (13f). Do you mean (13b)?
Response: I think this has been deal with above; (13f) was stated incorrectly, and the correct advection velocity is $(\bar{u}, \bar{v})$ as in all the other advection operators. As a result, the advection does disappear under the change in coordinate system. I have corrected (13f) in the updated manuscript.
Reviewer: p11, line 19: "transverse normal stress" ¿ "the transverse normal stress"
Response: See above for our disagreement re: definite articles.
Reviewer: p11, line 24: How small are the intervals around x_P?
Response: a single cell size. I have added a note saying "(the small interval being a single cell / element)"
Reviewer: p12, line 11: Indicate which endpoint is upstream and downstream for each cavity.
Response: I have clarified this in the second sentence of this paragraph, to say
identify cavity end points b_j and c_j respectively as the the upstream and downstream end points of any finite intervals above a minimum threshold size ...
Reviewer: p13, line 1: "the inherent heterogeneity involved in an unstructured mesh" ¿ In what way does the degree of uniformity of a mesh influence possible oscillations in the cavity shape?
Response: I have added my reasoning for this at the end of the paragraph by saying
... an underlying steady state solution in the original coordinate system becomes a travelling wave solution in the travelling frame used for computation. Any grid effects (small or large) are then bound to be periodic, including those involved in the contact area moving relative to the mesh (which presumably account for uplift and therefore cavity shape).
Reviewer: p13, line 5: "at least for the moderate values of N for which the dynamic model produces a recognizable near-steady state within a reasonable time span" ¿ Does this mean that for smaller values of N, the difference between the cavity shapes produced with both models start to differ visibly? If so, I suggest that an additional panel be added to figure 2 for a value of N for which the models in part 1 and 2 start to differ. If future work is to be produced on this topic, researchers should have an idea of the ways in each the numerical model you propose produces potential inconsistencies. Figure 2 as it stands now indicates an almost perfect consistency between both models, yet what you write suggests the contrary.
Response: That is not what I mean to say; instead, the numerical results for lower values never approach what looks like close to a steady state to the naked eye over the time periods of compu-

tation, as indicated by the wiggly lines for $\bar{h}$ in figure 3. I'd be happy to add an extra panel but would need guidance on which non-steady profile to use.
Reviewer: p13, line 17: "steady state mean water depth $\overline{h}$" - $\overline{h}$ refers to the mean cavity size, which coincides with the mean water depth in the cases considered here. I suggest you avoid referring to $\overline{h}$ as the mean water depth here because it could be confusing for the reader.
Response My apologies. I've changed the wording to "mean cavity size", except in the abstract (which references water sheet thickness). However, in that instance I do not reference $\bar{h}$.
Reviewer: p14, Figure 3: It could be interesting to show values for e.g. $\overline{h}$ at the steady states obtained with the model from part 1 if the same time history for N was followed (allowing for quasi-steady states to be achieved by small changes in N, as opposed to the step jumps we see in Figure 3). This would give a valuable insight into how the dynamic evolution of cavities differs from its steady counterpart, which is one of the main goals of this paper.
Response: I have added this to figure 3, along with the cavity end points predicted by part 1, combined with a brief discussion at the end of section 3.2, stating
Figure 3 provides further comparison between results of the dynamic model of the present paper and the steady state solutions of part 1, in the form of green lines showing mean cavity depth $\bar{h}$ in panel a and cavity end point positions in panel b, computed as in part 1. Panel a shows that, for small N and for the time intervals over which N is held steady, there are continued oscillations of non-negligible size, which I discuss further in the next section. These have time-averaged cavity depths $\bar{h}$ that are somewhat smaller than the predicted steady state results. For larger N, the residual oscillations discussed above are of much smaller amplitude, and have time-averaged $\bar{h}$ that agrees closely with the steady state results, but also remains slightly smaller. This is true except once an isolated cavity forms at $t = 636$: the steady state results as computed using the method from part 1 predict a smaller isolated cavity than that which is trapped in the dynamic solution as discussed above. In all cases, cavity end point positions late in each interval of fixed N agree closely with those predicted by the part 1 steady state solver, although upstream cavity end points computed by the dynamic model (shown in red) are systematically located slightly downstream of the locations predicted by part 1. This may in part occur because cavities are very shallow at their upstream ends, and the post-processing of the dynamic model results uses a threshold value of $h \geq 5 \times 1^{-4}$ to identify one of the finite volume cells at the bed as part of a cavity.
Reviewer: p14, Figure 3: This figure would be more readable if the ticks of the x axis were aligned with the vertical grid lines. Throughout the paper you refer to the times where jumps in the effective pressure take place (e.g. t = 78) and its not entirely obvious which points these are. The same goes for the remaining figures in this paper of a similar type.
Response: I absolutely see the rationale for this. However, in practice, with the uneven intervals on which N is changes, this becomes not just unsightly but quite hard to read where N is changed more rapidly (towards both ends of the time domain shown) — where I assume you don't just want the tick marks aligned, but the tick mark labels.
Reviewer: p15, line 24: "That contact area motions occurs around the top of the prominent bed protrusion at x = 0.8." - This sentence does not make sense, change "that" ¿ "these"?
Response: Changed to "That contact area motion…"
Reviewer: p17, Figure 5: As I wrote above for Figure 5, it would be very nice to include results for the steady solution here too, in order to see for example whether the oscillations in $\overline{h}$ occur around the steady states predicted in part 1.
Response: Done.
Reviewer: p19, Figure 6: "bed 18" ¿ "bed given by (18)"
Response: I've already removed the reference to equation (18) at the behest of the other reviewer.

Given all comutations are done with the same bed shape, that should be ok, I hope.
Reviewer: p19, line 16: "the a less-advanced" ¿ "a less-advanced"?
Response: Corrected.
Reviewer: p19, line 1: "an initial advance of the cavity end point from $c4.1$ to $c4.5$ over a time interval around 10^{-2}" - I do not see this in figure 6. Over a time interval of 0.01 I see an advance from around 3.9 to around 4.1.
Response: I think this a misunderstanding due to poor wording on my part: I mean the rapid expansion *after* the cavity first expands rapidly but only by a small amount, and then grows "viscously" for a while, and then experiences a second episode of rapid growth (at different times following the change in N, depending on how larger that change in N was). I've changed the beginning of this paragraph to clarify:
The subsequent rapid expansion of the cavity (following the second phase of slower cavity growth, and corresponding to the "drowning" of the smaller bed protrusion) can be separated into two parts: an initial advance of the cavity end point from $c \approx 4.1$ *to* $c \approx 4.5$ *over a time interval around* 10^{-2}*, somewhat shorter than a single Maxwell time. This part of the cavity expansion is marked with "rapid connection" in figure 7(b), and is effectively another example of hydrofracture. ...*
Reviewer: p19, line 4: "htis" - "this"
Response: Corrected.
Reviewer: p19, line 13: "leading oscillatory" ¿ "leading to oscillatory"
Response: Corrected (the text here has been amended more substantially due to a comment from the other reviewer)
Reviewer: p20, Figure 7: This figure could be improved by adding visible marks indicating the endpoints of the cavities.
Response: Done.
Reviewer: p20, line 1: "9" $\mapsto$ "figure 9"
Response: Corrected
Reviewer: p20, line 6: "the smaller bed protrusion upstream of N" - Do you mean M?
Response: Yes. I have corrected this.
Reviewer: p21, Figure 8, caption: "$P = \{./65\}$" $\mapsto$ "$P = \{.65\}$"
Response: Corrected to say $P = \{4.64\}$
Reviewer: p21, line 8: "a extended" ¿ "an extended"
Response: Corrected.
Reviewer: p22, line 9: "the greater ability of the solution to relax towards a steady state" - The reader could judge the validity of this statement if information on the steady states was included in Figure 9. I suggest values for N_M and x associated to steady states be included in Figure 9.
Response: I think steady states would perhaps be a bit contrived here, so I've taken the offending sentence out (especially as the next little bit argues that relaxation to a steady state is perhaps not a likely scenario).
Reviewer: p22, line 10: "what the extent" $\mapsto$ "what extent"
Response: Corrected.
Reviewer: p23, line 17: "it is plausible a critical value h_c could plausibly be defined" ¿ Avoid repetition of plausible/plausibly?
Response: Indeed. I've removed "plausibly" here.
Reviewer: p 24, line 27: "Once a a set" $\mapsto$ "Once a set"
Response: Corrected