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: 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 condiitons 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

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

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, Tsa 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 premeable 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", 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 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 rexamine 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 $t = 78$.
textbfResponse: 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 decribing 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 refering 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 caviies are very shallow at their upstream ends, and the postprocessing of the dynamic model results uses a theshold 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 expans 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 (folowing 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