Responses to Referee comments by Christian Schoof on “Brief communication: Time step dependence (and fixes) in Stokes simulations of calving ice shelves” by Brandon Berg and Jeremy Bassis

We thank Christian Schoof for his feedback on this manuscript. Our responses to comments are given below, with original recommendations/points in black and responses in red. When referenced, line numbers refer to the revised manuscript.

Major Recommendations:

We thank the reviewer for his comments and suggestions. We have incorporated most of the reviewers' excellent suggestions in the manuscript. We have vacillated slightly in our preferred terminology between ill-posed and stiff before settling on “unphysical” for reasons that are described in more detail in response to specific reviewer comments.

I would make clear the distinction between the poorly condition Stokes flow problem in Durand et al and the two flavours of actual ill-posedness seen when your boundary conditions permit force and/or torque balance to fail. It won’t hurt to allude to the latter, even though I don’t imagine many people are trying to solve Stokes flow problems for icebergs — you never know. It would also be reasonable to say that the sea spring mechanism (probably) works well for the second version of the actually ill-posed case, where big departures from equilibrium need never occur.

This is a good point. Our emphasis here was really on pointing out that when the geometry evolves rapidly, as is the case for an iceberg calving event, the sea-spring method becomes problematic and can lead to numerical problems. And when these numerical inaccuracies are combined with, for example, stress based calving criteria, there is the possibility of introducing purely numerical calving instabilities. However, under most circumstances, the sea-spring method remains satisfactory. This is better emphasized in lines 69-76.

We have added additional text to clarify the fact that, for our choice of boundary conditions, global force and torque balance are not necessarily satisfied leading to an ill-posed problem (lines 54-56, lines 110-114). However, if we consider fixed velocity (Dirichlet) boundary conditions over a portion of the domain, there is no rigid body motion (translation or rotation) that can be added to the ice shelf. In this case we can still obtain large velocities that are time step dependent using the sea-spring method when the geometry departs significantly from hydrostatic equilibrium over a portion of the domain.

Starting with a geometry that exactly satisfies global and local force/torque balance and then introducing small changes to that geometry can result in large changes to the velocity. This is what we think the reviewer calls “stiff” or poorly conditioned. Small changes in the initial conditions (i.e. position of the ice water interface) lead to large changes in the velocity solution. This can be partly cured by adding, say, a quadratic drag force due to the water, as the reviewer notes. However, for realistic drag coefficients, this still results in exceptionally large velocities. In fact, for configurations that we tested, the velocities can exceed the speed of sound! Because of this and because of the fact that we wish to avoid any confusion between “ill-posed”, “ill-conditioned”, and “stiff”, we have decided to rephrase and call this behavior “unphysical”. We believe this captures the numerical issue accurately and avoids introducing additional jargon that glaciologists might not be as familiar with.

The paper says ‘However, as we shall show, this assumption is problematic for applications where the ice departs from hydrostatic equilibrium.’ I’d circle back to this at some point and point out that things may not be quite so dramatic as to say that the Stokes equations have nothing to say about what happens during these ‘inertial’ events; they do, but in modified form. I think this will also tie in to the discussion of how to formulate the inertial term in discrete form, see again under ‘minor points’ below.

As stated above, if we consider fixed velocity (Dirichlet) boundary conditions over a portion of the domain, there is no rigid body motion (translation or rotation) that can be added to the ice shelf and we still obtain large velocities that are time step dependent using the sea-spring method. The large velocities are tied to the bending of the ice that occurs in response to removal of ice at the calving front. As the reviewer notes, this is because the problem is “stiff”.

But to simplify our discussion, we have removed equation (8) and the accompanying text regarding the separation of the velocity into viscous and uplift components. Instead, we focus on highlighting the “stiffness” of the problem and how small changes to the ice-ocean boundary location can cause large changes in the solution. In this way, we emphasize the importance of carefully treating the hydrostatic uplift without commenting on the exact nature of the decomposition of viscous and rigid body motion. However, we do add text in the manuscript stating that such a decomposition may be possible (lines 110-114).

Minor Points:

I would probably make a bit clearer how boundary conditions are important in determining whether an actual ill-posedness can occur in the sense of there being no solution to the Stokes flow problem. As the extended discussion above indicates, the partial Dirichlet conditions in the present paper ensure there is no issue of torque or horizontal force imbalance, but it others may run into these issues in their own research, and look to apply the method developed here. Also, as indicated in the second paragraph of this review, there may be other tricks to ensuring force balance.

We have added text to clarify this. In particular, we have noted that adding global constraints on force and torque balance is possible (lines 110-114). We do, however, note that because ice breaks, global force and torque balance would have to be considered on each intact segment and this becomes increasingly challenging to efficiently identify and manage. Hence, our solution of simply including the acceleration directly into the Stokes equations becomes a more practical solution. As noted in our previous response, we have also shifted our terminology to “unphysical” because unphysically large velocities are still possible when drag is included.

The decomposition in equation (8): for a sea spring model, I don’t think you can argue that the sea spring term $u(x, z)\delta t$ simply causes an additive term $\Delta z_{uplift}/\delta t$…

We have streamlined and simplified this section and have eliminated this equation and explanation. We now focus on the “stiff” nature of the problem rather than a specific decomposition into viscous and uplift components.

Notation: there is quite a bit of randomness about which quantities are in boldface and which are not, especially when it comes to tensors (σ versus ε and I?). Make it consistent to please the eye. . .

Notation has been changed so that both vectors and tensors are all in boldface.

The '/2' should probably be inside the square root in the definition of the invariant εe just after equation (4)

Error has been fixed.

Writing $u(\Delta t)$ on the left-hand side of (8) is confusing as u has a well-defined meaning as the continuum solution of the Navier-Stokes problem as a function of (x, z, t), so changing the arguments of that function haphazardly to Δt is bad form (and actually confused me quite a bit). For starters, the quantity on the left isn't u but a numerical approximation to it, solving a modified problem, so give it a different symbol, and be clear why you are using the Δt argument on the left (your numerical algorithm thus constructed leads to a solution that turns out to depend on Δt, whereas you would want it not to be dependent on Δt.

We have eliminated Equation (8) in response to other comments by the reviewer.

The numerical form of the acceleration term in equation (9): this is defensible when you have no rotational degrees of freedom in the rigid body motion, because the advection term for momentum $u \cdot \nabla u$ is dominated by $r \cdot \nabla r$ (see point 2 above), and in the absence of a rotational degree of freedom, $\nabla r = 0$ so the advection term goes away. As soon as there is rotation, this is no longer true, and you are wel advised to retain the full inertial term $\partial u/\partial t + u \cdot \nabla u$. I realize that the present paper does not allow for that possibility, but I think it is worth mentioning.

In our model, we are using an Arbitrary Langrangian Eulerian (ALE) formulation, which updates all mesh coordinates at every time step based on the velocity field. This led us to use the material derivative in Equation (9). But we have added text to clarify the fact that even in a Eulerian reference frame we could neglect the $u \cdot \nabla u$ term because the velocity field does not contain a rigid body rotation (lines 117-118). We have also added text explicitly stating we are using an ALE formulation (lines 90-92).

Again, equation (9): I am actually not clear how you imagine you are computing this in a 'Lagrangian frame' to begin with, since you are solving, in discrete terms, an elliptic equation (or a parabolic equation with a backward Euler step, which is the same thing); if you genuinely are using a Lagrangian transformation here, please be explicit and speciifc. In terms of implementation, the reduced acceleration term in equation (9) is my biggest concern, even if I believe it to be leading-order correct (in the Reynolds number, see above) for the vertical-motion-only case discussed in the paper.

We are not quite sure that we understand the reviewer's question. In our implementation, we are solving a parabolic equation with a backward Euler step. The update to the ice geometry is done using a fully Langrangian formulation in which we update the mesh coordinates at every time step. Of course, we do need an initial condition for particle velocities. For the initial condition on velocity to use in the backward Euler step, we choose a uniform velocity field equal to the inflow velocity in the horizontal direction and zero in the vertical direction. The choice of zero initial velocity in the vertical direction is motivated by the experimental design, in which we imagine an ice shelf that is initially perfectly at hydrostatic equilibrium, and thus should have nearly zero vertical velocity before calving.   We have added text to section 4 of the manuscript to clarify the precise initial condition on velocity (lines 118-121).