Brief Communication: Time step dependence (and fixes) in Stokes simulations of calving ice shelves'

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Overview

Let me start with an apology to editor and authors: this has taken far too long to complete, global pandemics or otherwise.

This paper addresses how to adapt ice sheet / ice shelf flow models to situations in which overall force balance cannot be satisfied momentarily. Clearly this is not something that occurs for any extended periods of time in an ice sheet or ice shelf, but causes significant problems when it does: the Stokes flow equations that underpin most models for ice sheet and ice shelf flow have no solution when force balance is violated temporarily, since they omit inertial terms.

The obvious situation in which violation of force balance can occur is when an ice sheet that contains no grounded portion within the model domain experiences an abrupt calving event, removing part of the ice and causing an imbalance between buoyancy forces and weight of the ice. When I say 'contains no grounded portion', I really mean, 'is not subject to a Dirichlet or Robin condition constraining vertical velocity on any part of the boundary'; in the computational set-up in the paper, the left-hand boundary of a block of ice is 'attached' to an unmodelled ice shelf by prescribing horizontal velocity and vanishing vertical shear stress σ_{zx}; one could envisage ensuring force balance by altering the latter condition to account for some sort of vertical drag that is dependent on vertical velocity as a regularization (as a friction law, effectively), but that is beside the point.

The paper correctly (and importantly) makes the point that ice shelf models need to be cognizant of the fact that force balance violation is a real possibility, and that the omission of inertial terms leads to an ill-posed model. (I have a few things to add on ill-posedness shortly. There are different ways in which a model can be ill-posed and the distinction between them is important, in my opinion.) The authors also show that retaining the inertial term (specifically, the time derivative of velocity) fixes the problem. However, in order to maintain a stable time stepping scheme for large time steps, the retention of an additional stabilizing term due to Durand et al (2009) that mimics a fully implicit time step is necessary.

This is a useful contribution to the literature and should be published. I have a few comments on presentation and a few issues that could do with clarification, mostly at the authors' discretion. I apologize if I take some time describing these. It seems to be my habit to drone on about things that few others care about so feel free to ignore any or all of what follows,

1. The most important concept that could be clarified for the reader is the notion of ill-posedness, which also needs to be distinguished from the effective stiffness of

the problem that Durand et al (2009) were solving, which led them to invent their stabilizing 'sea spring' mechanism.

The problem in Durand et al's paper is perfectly well posed so long as there is contact with bedrock somewhere, and the normal stress exceeds water pressure on that contact area. In that case, a Dirichlet condition on normal velocity applies there, while the friction law provides a nonlinear Robin condition on tangential velocity (and hence on horizontal velocity if the bedrock boundary is not vertical everywhere). These conditions ensure mathematically that the Stokes flow problem has a unique solution that depends continuously on 'the data' (things like the boundary conditions), making the flow problem well-posed in the usual sense. There is an ample literature on well-posedness of the Stokes equations (going back to Ladzhenskaya in the 1960s) and the fact that pure stress conditions can lead to solvability conditions similar to the solvability condition for a pure Neumann conditions on a Poisson equation are well known in the pde literature, though I wouldn't be able to give you the 'original' citation for this. More on this shortly however.

The problem in Durand et al's work is that despite a formally continuous dependence on the data, the problem is extremely sensitive to slight deviations in vertical position relative to the one the ice shelf 'wants' to adopt. In other words, the problem is poorly conditioned or stiff, which is not quite the same as ill-posed.

Effectively, at leading order, Durand et al are modelling a long viscous beam, which permits extremely large vertical velocities if the lower ice surface does not adopt a very specific shape that is close to hydrostatically supported (though not exactly hydrostatically supported). Their real interest is not in solving the approach to that position, which requires very short time steps, but in modelling the much slower evolution of ice thickness due to horizontal flow. The 'sea spring' is indeed an ingenious way to dampen the vertical motions due to the the beam-like nature of the shelf (effectively, by providing the viscous analogue of a Winkler foundation in elastic beam theory).

Importantly, the sea spring is not intended to deal with a situation where force balance is violated, and I think it is important to make that point. I doubt whether the original authors had even considered that scenario.

Without Dirichlet or Robin conditions constraining both velocity components (keeping things in two dimensions throughout this discussion), between one and three solvability conditions arise for a Stokes flow problem. In the scenario outlined in the paper, horizontal velocity is constrained at the inflow boundary on the left, so the only solvability condition winds up being the one that says that the net vertical component of force on the shelf must be zero. If one were to replace the left-hand inflow boundary with another free 'cliff' on which only stress conditions apply analogously to the right-hand boundary, we would get an iceberg, subject to not one but two more solvability conditions: in that case, we would also need a zero net horizontal component of force *and* a vanishing net torque.

Mathematically, this is tied up with the fact that the Stokes operator is invariant under the addition of a rigid body motion $\boldsymbol{r}$ (a combination of a constant velocity and a rotational velocity) to the velocity field $\boldsymbol{u}$, and the number of unconstrained degrees of freedom permitted by the boundary conditions is the number of solvability conditions that arise: in the example in the paper, the horizontal and rotational degrees of freedom in the rigid body motion are constrained by the Dirichlet condiition on on the horizontal velocity component, so we only get one solvability condition, associated with vertical force balance and corresponding to the vertical component of the constant part of $\boldsymbol{r}$.

As I mentioned already, there is an extensive literature that proves that net force and torque balance conditions that are not automatically taken care of by the boundary conditions provide not only necessary but sufficient conditions for the existence of solutions.

When these conditions are not satisfied (e.g., in the present paper, force balance in the vertical may not be satisfied), then well-posedness fails at the first hurdle: there is no solution at all. The sea spring does not solve this problem, as the authors discover: the solution to the Stokes flow problem with no intertial term and only the sea spring stabilizing boundary conditions becomes intrinsically dependent on time step size δt, and therefore has no continuum limit as $\delta t \to 0$. This is where something like the approach in the present paper is necessary.

There is a more subtle ill-posedness that the sea spring mechanism by itself *can* help mitigate. Ill-posedness requires not only existence, but also uniqueness. When force and torque balance conditions are satisfied, there is a solution, but it is not unique: you can in principle add any rigid body motion permitted by the boundary conditions and still obtain a solution (in the case described by the paper , the permitted rigid body motion is purely a vertical velocity, for an iceberg it would be an arbitrary rigid body motion).

In reality of course the motion of a chunk of ice is not indeterminate when force and torque balance are satisfied, and yet you do not need to appeal to intertial terms to figure out what the velocity field is (as the authors here point out, doing so on its own is a bad idea if you want to take long time steps, so retaining inertial terms may not even be a practical solution to the problem).

Given *a* solution $\boldsymbol{v}$ to the Stokes flow problem in which force and torque balance satisfied, you can figure out what rigid body motion you need to add by requiring that the displaced ice surface *after* the next time step is still such that force and torque balance are satisfied after that time step. Assuming for simplicity for the moment that a forward Euler step is used and that the boundary conditions on the boundary are in the form $\boldsymbol{\sigma} \cdot \boldsymbol{n} = \boldsymbol{f}_b(x, z)$ all along the boundary (so there are only Neumann conditions), this amounts to finding a rigid body motion $\boldsymbol{r} =$

$\boldsymbol{r}_0 + \omega(-z, -)$ with constant-in-space $\boldsymbol{r}_0$ an ω such that the domain boundary $\partial\Omega(t+\delta t)$ obtained by translating every point $\boldsymbol{X}(t) = (X(t), Z(t))$ on the boundary to the corresponding new position

$$\boldsymbol{X}(t + \delta t) = \boldsymbol{X}(t) + \boldsymbol{v}(\boldsymbol{X}(t))\delta t + \boldsymbol{r}_0\delta t + \omega(-Z(t), X(t))\delta t$$

satisfies

$$\int_{\Omega(t+\delta t)} \rho \boldsymbol{g} \, \mathrm{d}\Omega + \int_{\partial\Omega(t+\delta t)} \boldsymbol{f}_b \, \mathrm{d}\Gamma = 0$$

and

$$\int_{\Omega(t+\delta t)} \boldsymbol{x} \times \rho \boldsymbol{g} \, \mathrm{d}\Omega + \int_{\partial\Omega(t+\delta t)} \boldsymbol{x} \times \boldsymbol{f}_b \, \mathrm{d}\Gamma = \mathbf{0},$$

where $\boldsymbol{x} = (x, z)$ and $\Omega(t)$ is the ice domain at time t. This amounts to three conditions (the last is really a scalar condition for a two-dimensional domain), sufficient in principle to find the three constants that define $\boldsymbol{r}$: the two components of the constant vector $\boldsymbol{r}_0$ and the angular velocity ω.[1] The case where the horizontal velocity is already constrained by a Dirichlet condition as in the paper works analogously.

The method proposed above is not quite the same as the sea spring (since there the constraint of force balance being satisfied after the next time step is built into the Stokes solver, rather than requiring a post-processing step to find the rigid body motion required to ensure force balance on the next time step), but the two approaches are close to each other.

Recommendation: 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 equlibrium need never occur.

2. As a sort of brief follow-on from the first point and the role of rigid body motions, this point concerns the discussion about the validity of the Stokes equations. If you use the non-dimensionalization procedure that is usually used to justify the Stokes equations from the Navier-Stokes equations for the case you are looking at here (failure of force balance, or potentially force and torque balance), you do not

[1] There is a caveat in the sense that the horizontal motion of an iceberg will remain indeterminate even with this additional constraint requiring force and torque balance after the next time step, because horizontal force balance will be satisfied trivially (identically) if the boundary force $\boldsymbol{f}_b$ is given by buoyancy, so an additional constraint such as zero mean velocity would be necessary.

really conclude that the Reynolds number has suddenly become $O(1)$ or large and therefore the full Navier-Stokes equations must be solved.

Instead, what you find is that you end up having to decompose the velocity field into two parts. I'll spare you the bit where I dress this up with mathematics and just describe what happens. The first part is a rigid body motion whose evolution is controlled by Newton's second law using the net force on the chunk of ice, and by the equivalent of Newton's second law for the evolution of angular momentum. The magnitude of this first part is much larger than the second part, so the *motion* of the chunk of ice as it settles into a new position in which force and torque balance are satisfied is simply that of a rigid block (intuitively obvious, i guess).

The second part is the solution of a *Stokes flow* problem, sans inertial terms, and this second part controls internal stresses during the settling process (if those are the main concern, which I think they are). Call this second part of the velocity the viscous velocity. The Stokes flow problem for the viscous velocity has the same boundary conditions as the original Navier-Stokes problem, but the body force is amended by subtracting the inertial terms generated by the rigid body motion. This ensures *apparent* force and torque balance in the problem for the viscous velocity, although if you want the viscous velocity to be unique (not particularly relevant since the motion is controlled by the rigid body motion and the viscous stresses are unique) you have to add something like requiring that the mean of the viscous velocity and the mean rotation due to the viscous velocity vanish.

In other words, you have

$$\boldsymbol{u} = \boldsymbol{r}(x, z, t) + \boldsymbol{v}(x, z, t)$$

with $|bmr$ a rigid body motion

$$\boldsymbol{r} = \boldsymbol{r}_0(t) + \omega(t)(-z, x), ,$$

where $\boldsymbol{r}_0$ and ω depend on time but not position. The ratio $|\boldsymbol{v}|/|\boldsymbol{r}|$ scales as Re, the Reynolds number that one would normally compute from the viscous velocity scale for the size of the applied body and surface forces. As a result a boundary point $\boldsymbol{X}(t)$ on $\partial\Omega(t)$ evolves as

$$\frac{\mathrm{d}\boldsymbol{X}}{\mathrm{d}t} = \boldsymbol{r}(\boldsymbol{X}(t)).$$

because $\boldsymbol{v}$ is tiny compared with $\boldsymbol{r}$: while force balance is violated, the ice domain moves as a rigid body. Assuming for simplicity again that stress $\boldsymbol{\sigma} \cdot \boldsymbol{n} = \boldsymbol{f}_b(x, z)$ is specified everywhere at the boundary, the rigid body motion satisfies

$$\begin{aligned}
\int_{\Omega(t)} \rho \, \mathrm{d}V \frac{\mathrm{d}\boldsymbol{r}_0}{\mathrm{d}t} &= \int_{\Omega} \rho \boldsymbol{g} \, \mathrm{d}V + \int_{\partial\Omega} \boldsymbol{f}_b \, \mathrm{d}\Gamma, \\
\frac{\mathrm{d}}{\mathrm{d}t} \int_{\Omega(t)} \rho \boldsymbol{x} \times \boldsymbol{r} \, \mathrm{d}V &= \int_{\Omega} \boldsymbol{x} \times \rho \boldsymbol{g} \, \mathrm{d}V + \int_{\partial\Omega} \boldsymbol{x} \times \boldsymbol{f}_b \, \mathrm{d}\Gamma,
\end{aligned}$$

where the second equation can be cast in terms of the sum of a moment of intertia (that is constant due to the volume $\Omega(t)$ moving as a rigid body) times $\mathrm{d}\omega/\,\mathrm{d}t$ and the time derivative of the moment of inertia associated with the barycenter of $\Omega(t)$, which can be written in terms of the time derivatives of $\boldsymbol{r}_0$ and ω. In short, the current position and orientation of the rigid body $\Omega(t)$ uniquely defines the derivatives of $\boldsymbol{r}_0$ and ω, while $\boldsymbol{r}_0$ and ω determine the change of position and orientation of $\Omega(t)$ — basic non-continuum mechanics, if you will.

The viscous velocity $\boldsymbol{v}$ by contrast satisfies the modified Stokes flow problem

$$\nabla \cdot \boldsymbol{v} = 0$$

$$\boldsymbol{0} = \nabla \cdot \boldsymbol{\tau}(\boldsymbol{v}) - \nabla p + \rho \boldsymbol{g} - \rho \left(\frac{\partial \boldsymbol{r}}{\partial t} + \boldsymbol{r} \cdot \nabla \boldsymbol{r} \right)$$

on $\Omega(t)$, subject to $(\boldsymbol{\tau}(\boldsymbol{v}) - p\boldsymbol{I}) \cdot \boldsymbol{n} = \boldsymbol{f}_b$ on $\partial\Omega(t)$, where $\boldsymbol{\tau}(\boldsymbol{v})$ is the viscous relationship between deviatoric stress and velocity, p is pressure and $\boldsymbol{I}$ the identity tensor. The construction here ensures that the relevant force and torque balance relationships for $\boldsymbol{v}$ are automatically satisfied, while $\boldsymbol{r}$ can be solved for a priori, so the fictitious force term in the momentum balance equation is known before $\boldsymbol{v}$ is solved for.

This is *similar* to what is alluded to in equation (8) in the paper (more on that below), but not the same, since you can effectively solve for the rigid body motion ($\Delta z_{uplift}/\Delta t$) and the viscous velocity separately, but that is not how the algorithm in the paper works. I just think it is worth pointing out somewhere that a construction of this kind is possible, which retains the idea of a Stokes flow describing the stress even as inertial effects kick in: in particular, the inertial motion is independent of rheology and viscous stresses, so long as the latter do not cause further domain changes by *fracturing* (they do not by causing viscous deformation of the ice body, that is far too slow, see the point about the ratio between the velocity magnitudes above).

As a side note, I think equation (8) is misleading if one were ever to try to allow rotational inertial motions, more on that under 'minor points' — the clue is in the form of the fictitious force term above.

Recommendation: 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.

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.

- The decomposition in equation (8): for a sea spring model, I don't think you can argue that the sea spring term $\boldsymbol{u}(x,z)\delta t$ simply causes an additive term $\Delta z_{uplift}/\delta t$ As a simpler example, consider a Neumann condition in a Poisson equation

$$\begin{aligned} -\nabla^2 u =& f && \text{on } \Omega \\ \frac{\partial u}{\partial n} =& g_n \qquad \text{on } \partial\Omega \end{aligned}$$

  and call the solution of this problem u_{visc}. Then modify (regularize?) the boundary condition as

$$\frac{\partial u}{\partial n} + cu\delta t = g_n$$

  To the best of my knowledge, the solution to the modified problem cannot be written (as a function of δt!) in the form

$$u = u_{visc} + \frac{u'}{\delta t},$$

  which is effectively what equation (8) is saying, albeit for a more complicated elliptic problem.

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

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

- Writing $\boldsymbol{u}(\Delta t)$ on the left-hand side of (8) is confusing as $\boldsymbol{u}$ 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 $\boldsymbol{u}$ 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.

- 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 $\boldsymbol{u} \cdot \nabla \boldsymbol{u}$ is dominated by $\boldsymbol{r} \cdot \nabla \boldsymbol{r}$ (see point 2 above), and in the absence of a rotational degree of freedom, $\nabla \boldsymbol{r} = \mathbf{0}$ so the advection term goes away. As soon as there is rotation, this is no longer true, and you are well-advised to retain the full inertial term $\partial \boldsymbol{u}/\partial t + \boldsymbol{u} \cdot \nabla \boldsymbol{u}$. I realize that the present paper does not allow for that possibility, but I think it is worth mentioning.

- 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.

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