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Interactive comment on "Modelling the dynamic response of Jakobshavn Isbræ, West Greenland, to calving rate perturbations" *by* J. H. Bondzio et al.

J. Bassis (Referee)

jbassis@umich.edu

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General Appreciation

This manuscript describes a numerical algorithm based on the Level Set Method to track the calving front position of glaciers and ice sheets and applies the model to study the response Jakobshavn Isbrae to calving rate perturbations. The study addresses two long standing problems in glaciology: The first problem involves finding a numerical algorithm that allows the calving front to consistently evolve in 2D. The second involves specifying how the glacier responds to an evolving calving front position and geometry. (There is a third issue, specifying the 'rule' that determine how the calving front evolves,

but that is a different problem from the one considered in this manuscript.)

Most studies that have examined glacier retreat have either used flowline models that can easily track the calving front position using semi-Lagrangian approaches. An exception to this is the PISM-Potsdam model which uses a sub-grid tracking algorithm to allow the calving front to advance and retreat. Hence, at least in my opinion devising an accurate and efficient method that allows the calving front to evolve remains an important challenge.

Overall, portions of this manuscript provide valuable and innovative contributions to the field of ice sheet modeling and this manuscript should be published. I do, however, have some suggestions to improve the manuscript. Foremost amongst these is that I would like to see the manuscript focus more tightly focused on introducing and testing the level set method and appropriate re-titled (e.g., A level set method to track terminus position in 2D ice sheet models or something like this). I realize that many readers may be turned off by a primarily technical manuscript. However, I think there is a community of ice sheet models that will be highly interested in these results. Furthermore, it is my opinion that the authors need to provide a little bit more demonstration of the method to highlight its limitations and show that it is not plagued by numerical issues before I have confidence in the model predictions. Moreover, as I will explain in more detail in later comments, the experiments have some problems that lead me to question the insight that they provide about Jakobshavn.

The experiments are, however, well designed to demonstrate the level set method as part of a more general proof-of-concept so I think relatively minor changes would allow the manuscript to be acceptable for publication provided the connection to Jakobshavn is de-emphasized.

More detailed comments on the Level Set Method The mathematical description of the level set method is in general adequate, but it would helpful to provide readers with a better conceptual basis. For example, my understanding of the level set method is that the function ϕ, defined in Equation 4 is a hypersurface. In 2D, the intersection of the surface ϕ with the plane defined by $z = 0$ corresponds to the boundary of the domain. I would encourage the authors to provide a simple diagram illustrating this.

Equation 4, at first glance, also seems to be ill-defined. The text states that w is the velocity of the boundary, defined in this case as $\phi = 0$. If this is the case then w would appear to only be defined on the boundary (calving front) and not in the interior of the domain, as implied by Equation 4.

A more intuitive approach for me begins with the more general equation:

$$\phi(t) = c \tag{1}$$

where c is an arbitrary contour level of ϕ. Taking the material derivative yields,

$$\frac{d\phi(t)}{dt} + w \cdot \nabla\phi = 0, \tag{2}$$

where w is the vector velocity at point (x,y). From this it is apparent that the level set equations just correspond to the material derivatives of any fixed contour c of the level set function and the above equation tells us how *all* contours evolve.

Irrespective to this discrepancy, the next question is how do you specify w in Equation 4? As the authors define it, w corresponds to the difference between the ice flow velocity u and a calving rate c, where $w = u - c$. (Forgive me for mixing notation here as I adopt the authors notation for calving rate in contradiction to the notation I used earlier where c denoted a fixed contour of ϕ.) Specifying the velocity u is easy as velocity is defined everywhere. Specifying a calving rate globally (and in particularly, far from the terminus) where calving does not occur is less intuitive. In general we can write c is a function of local properties of the calving front, global properties of the calving front (like, say, an integral over some portion of the calving front) and variables that are independent of the calving front. As I understand it, the method the authors adopt cannot accommodate global dependencies and calving rate must be parameterized

in terms of local properties of the front and independent variables. Furthermore, the 'calving rate' is defined everywhere, even in the interior of the glacier/ice sheet where calving is prohibited? I would like to see some more clarification on this to make sure I understand what the authors are saying.

The advantage that Equation (4) provides is that it allows the calving front to be defined and evolved by advecting a continuous variable ϕ. The trade-off is that now one must solve an additional hyperbolic equation, which is non-trivial. This presents additional technical problems.

Even if ϕ is initially defined in a way that it is smooth and differentiable, there is no guarantee that it will maintain differentiability as it evolves. A trivial example that illustrates this is advecting a sinusoidal curve with fixed velocity v. Even this simple example develops kinks in finite time. As I understand it, the authors deal with this problem by smoothing out the contours by adding diffusion. But this creates it owns problems: too much diffusion will act to smooth out spatial structure in calving fronts. Moreover, it is necessary to ensure that the scheme used to solve the equations conserves mass.

The above observations motivates my most significant suggestion: I would like the authors to include additional test cases to assess performance of the method. I provide a couple suggestions below.

A simple test to perform is to see how well the model is able to reproduce a sinusoidally shaped calving front advection with constant velocity v as more and more cycles in the sinusoid (are added e.g, set the calving front to $x = sin(N\pi x/L$, and vary N assuming periodic boundary conditions in x with velocity v in the y-direction).

A related issue is proving that the model conserves mass (or at least reasonably approximates conservation of mass). There are a variety of tests that that authors could apply to this. One test that could potentially be very revealing is showing that the model is able to reproduce the advance of confined ice tongue with zero lateral shear along the margins. This case is essentially one-dimensional and there is a well known analytic solution for ice thickness, strain rate and ice velocity for constant accumulation rate. In the absence of calving, it would be possible to test to see if the numerical model is able to reproduce the change in terminus position of the predicted analytic solution, along with appropriate mass conservation diagnostics. Similarly, given the extrapolation and lack of a sub-grid parameterization of calving front position, I would like to see that the model can produce advance and retreat symmetrically for an imposed calving rate. My guess is that the model will do very well in all this diagnostics, but it would be helpful to readers to see this more explicitly.

Experiments

My understanding here is that the authors use an inverse method to obtain basal traction coefficients in the sliding law based on observed ice thickness and velocity at some time where they have observations. The authors then spin up the model to steady-state prior to the onset of their experiments. However, because the input data corresponded to a time when the actual glacier was far from state-state, their modeled glacier evolves into a new state that is very different from the state they started with and very different from any observed Jakobshavn state. I'm skeptical that we can learn much about the dynamics of Jakobshavn if all simulations start with an ice sheet configuration very different from that observed. The choices that the authors made are defensible, but the glacier modeled is really Jakobshavn-like and at best a crude representation of the actual glacier as it has been observed. Moreover, the primarily results found (acceleration in deep water, stabilization on high points on the bed) are already well known as a generic consequence of form of the calving law used here. This is a numerical feat worth celebrating, as previous results were obtained for flow line models, but is not surprising by itself.

Finally, as I understand it the authors specify a calving law that depends exclusively on water depth. This is fine for a proof-of-concept, but for a study that attempts to provide insight into glacier behavior I would like to see some justification for this. Is this calving law tuned to observations? Does it fit the original Brown (1984) empirical fit? If the

calving rate function is based on its agreement with observations of Jakobshavn, then the fact that the terminus position mimics observations is merely a consequence of tuning an empirical formula and cannot be used as evidence the model is performing appropriately. Similarly, given the fact that the geometry of the glacier is very different from Jakobshavn based on the spin up, what does it mean for the model to be able to reproduce the observed terminus behavior?

These points become less relevant if the authors decide to focus on more generic results, but I would be much more comfortable if the authors started from a state more closely resembling Jakobshavn prior to retreat and provided some physical justification for their calving law if they want convince readers that their results provide intuition about the dynamics of Jakosbhavn.

Minor comments

It would helpful to see a table with experiments described so that readers can quickly see the differences.

Line 20, page 5498, why is ice front configuration in quotes?

page 2518: What is a floor line??

The assumption that sea water pressure defines the effective pressure at the bed seems an especially dire approximation far from the calving front. I understand that this is done because you need something and the model, presumably, doesn't include subglacial hydrology. However, I wonder if it wouldn't be best to ignore the dependence of sliding on subglacial water pressure or at least show that the model predictions are not sensitive to this assumption.

page 5492 and Figure 4: I'm not sure that I fully understand the calving rate terminology used here. What the authors call "calving rate" is defined as a two dimensional field. However, my interpretation of Equation 6 and conventional glaciological terminology would suggest that the calving rate (or calving velocity) is only defined at the calving front. See my earlier comments on this.

Page 5494: The authors authors activate or deactivate an entire element as being “ice filled” if one of the vertices intersects with the hyperplane. However, ice thickness and velocity appear to be extrapolated assuming continuous values outside of the ice sheet domain. Does this conserve mass?

Interactive comment on The Cryosphere Discuss., 9, 5485, 2015.