

We thank the two referees very much for their constructive comments, and discuss them below. The referees' comments are shown in blue, and our responses in black. The main changes we performed are:

- We added an additional experiment in the manuscript to evaluate the ability of the Level-Set Method to conserve volume, and
- we significantly rewrote many sections of the manuscript, to improve clarity. Our conclusions remain unchanged.

1 Referee 1 (Guillaume Jovet):

1.1 General comments

1.1.1

RC: This study introduces a Level-Set Method (LSM) in order to follow dynamically the migration of the calving front in the Ice Sheet System Model (ISSM). Several experiments are led on the Jakobshavn Isbrae ice stream. In particular, the authors study its geometrical response after applying different perturbations to a given calving rate, which itself accounts for seasonality. The results show that the model is capable to reproduce the subsequent change (retreat, thinning, steepening, acceleration) of the ice stream under enhanced calving rates consistently to physical observations. Moreover, the model reproduces well how the whole system stabilizes after releasing the perturbation, showing a reversible calving front. This is an interesting paper which presents original results. LSM are well-designed methods to track complex geometries with possible changes in topologies. This makes it fully relevant for calving fronts. Overall, I found convincing that ice flow models should indeed incorporate such (or similar) ad-hoc tracking methods of the calving front for projections of future contributions to SLR. However, I think the paper can still be improved before to be published.

1.1.2

RC: I have two main concerns, plus lists of specific and technical comments, which I hope will help the authors to improve this article. My two main concerns are:

LSM can greatly deal with complex interfaces including changes of topology. However, LSM also have counterparts, namely, i) they are usually not mass conserving unless solved by finite volume (this is not the case here) ii) gradients of φ near the interface $\varphi = 0$ tend to flatten after few iterations (if nothing is done) so that the interface gets less and less accurate with time. (I did experiment both problems in a previous work). A classical trick to deal with the last issue is to regularly regularize φ by solving another Hamilton-Jacobi PDE, which admits the signed distance to the interface $\varphi = 0$ as a solution. Indeed, re-initializing with the signed distance function ensures to have safe gradients equal or close to 1. It would be worth to further discuss numerical issues when solving the KCFC, and in particular, to include in the

test-setup (Appendix) the two following checks: i) how the numerical volume of the moving disk change over time, and if it is controllable by the mesh size (as this is the case for the advection velocity) ii) how the gradient of the LS ϕ near the interface $\phi = 0$ behave after few iterations.

AC: i) The Level-Set Method (LSM) as implemented here is not mass conserving. We mention this in the introduction now, and have added a simple test in the appendix, which shows that the method causes between 0.2% and 1% volume loss over 100 years, depending on mesh element size.

ii) From our experience there is no need for reinitialization of the LSF with the kind of experiments presented here. Calving rates are scaled such that over the time span of one year the front velocity adds up to zero (see experiment description). Only strong calving rate perturbations are able to alter this, but even the strongest perturbations (experiment C8) leave the gradient of the LSF in the vicinity of the calving front in the order of one to ten, see plot below. We see, however, that reinitialization of the LSF will become necessary for longer-term runs or discontinuous calving front speeds.

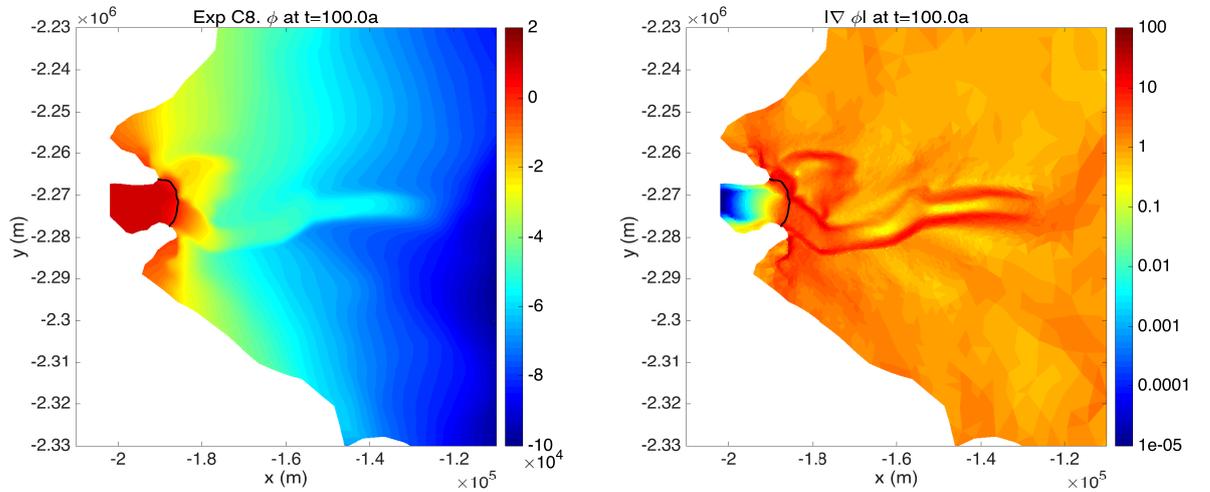


Figure 1: LSF (left) and a log-plot of the norm of its gradient (right) after 100 years of experiment C8. The black line denotes the calving front position.

iii) We listed the numerical issues solving the KCFC in section 2.3 of the submitted manuscript.

1.1.3

RC: The authors describe well the experiments setup, and also introduce a measure P for the time integration of the applied perturbation. Although the discussion part explains well all ice flow mechanisms and how they feedback each other, the direct outcomes of Experiments A, B and C in term of impact of the perturbation w.r.t. p_0 and Δt are little discussed. In addition, we expect that the measure P you introduced (arbitrarily) takes its sense after being corroborated with the results, but this come later on in a single sentence and without clear evidence. I therefore recommend the authors to better emphasize all the outcomes of Experiments A, B and C (in particular the consequences of changes in parameters p_0 and Δt) in the discussion (and in the conclusion as well).

AC: We see that the introduction of P is not justified, and replaced it by $\Delta t(1 - p_0)$. We

have put more emphasis on p_0 and Δt in the discussion and results presented in figures 7 and 8.

1.2 Specific comments

1.2.1

RC: abstract The abstract is not very efficient. In particular, the 5 first sentences should be made further concise so that the “Here, we present ...” comes earlier.

AC: Done.

1.2.2

RC: 1.3-4 p5488 “It is well suited ... partial differential equations”. I don’t see the point. PDEs are by nature challenging to implement in parallel since partial derivatives couple nodes by contrast to systems of ODEs (for instance).

AC: What we intended to say was that the solution of the KCFC does not require new numerical tools, but can be achieved using already present (parallel) solver routines. We corrected the sentence in the introduction.

1.2.3

RC: 1.5-10 p.5491 Quantities should be more rigorously introduced. E.g. the time interval $[0, \infty)$ comes before the time variable t is introduced, “then” in “if ..., then x belongs to ...” are in fact all “ \Leftrightarrow ”. In addition, i is defined as an abstract domain, and one has to wait 3 more pages before it is said that i corresponds to the ice domain.

AC: Done.

1.2.4

RC: 1.10 p.5492 This is an interesting point to use the LSM horizontally while keeping vertically the traditional ice thickness. You should motivate your choice even if this is easy to guess for ice modellers. From a general LSM perspective, this is not obvious.

AC: We have included a motivation for this approach in section 2.2.

1.2.5

RC: eq. (9) should be motivated, or at least say that by requesting $n \cdot \nabla S = 0$, we want to be sure that S keeps constant at the interface (or the calving front) when following the normal unit, which points outside the ice domain. In what is it important that the thickness and the velocity keep constant in the neighbourhood of the calving front?

AC: Done, see also sections 2.2 and 2.3 of the manuscript, where we have included explanatory sentences.

1.2.6

RC: 1.10-11 p.5494 “The strongly . . . updates”, any evidence to support this statement? “High viscosity of ice makes that computing the BC exactly at the front or at one mesh size distance does makes a big difference”, is that what you mean? This doesn’t look obvious to me.

AC: We found the sentence to be not correct, and have therefore removed it.

1.2.7

RC: 1.28 p.5498 Last sentence of Section 3: This measure P should come later. It makes no sense to introduce it in Section 3.

AC: As mentioned in point 1.1.3 we exclude P from the manuscript.

1.2.8

RC: 18-14 p-5499 several times, “local high” or “topographic high” should be “local maximum”?

AC: We defined the term “local high“ in the results section, to address readers with either mathematical or geoscientific background.

1.2.9

RC: At that point, I would briefly recall for unaware readers that grounding line on retrograde slopes are usually unstable and briefly explain why (+ references).

AC: Done, see discussion.

1.2.10

RC: 113-19 p.5500 You mostly comment the time derivative of ΔVol , maybe drawing the derivative instead of the function ΔVol would make more sense for Fig. 8?

AC: With this figure we want to show the qualitative impact of calving on the total ice volume. Annual volume change rates can be estimated well from the annual steps in the graphs. On the other hand we don’t think that it would be intuitively clear to the reader to time-integrate the volume change rate in order to obtain the total volume loss. Alos, due to the seasonally changing calving rate the time derivative of the ice volume would oscillate even stronger than the ice volume itself. Figure 8 is of secondary importance to the manuscript, and we think that the space in the manuscript for a second figure is not justified. We therefore would like to maintain this presentation of the results.

1.2.11

RC: 1.17 p.5500 “Enhanced calving causes additional . . . measure P”, how did you corroborate “Enhanced calving causes additional . . . measure P" Vol to P? Is it a coarse/visual estimate from Fig 8? If P proves to be a good measure, you should show it more accurately.

AC: We included a subplot to Figure 8, that shows $\Delta Vol \simeq \Delta t(1 - p_0)$.

1.2.12

RC: The expression “ice modelling” comes often and stands for “ice flow modelling”. I find “ice modelling” too general. It would be better replaced by a more precise expression. Also, “calving front” sounds to me more common term than “ice front”, which is adopted in this paper.

AC: Done.

1.3 Technical comments

1.3.1

RC: 1.9 p.5490 “B the ice viscosity parameters”, this is confusing, one might think that B is the viscosity.

AC: The terminology question around this parameter has been brought to our attention by other reviewers several times. Terms like for example “ice stiffness parameter”, used by MacAyeal (1989), can be confused with similar terms originating in elasticity theory. We therefore chose this denotation.

1.3.2

RC: 1.17 p.5491 you should recall the meaning of the acronym “LSF” in Section 2.2.

AC: Done.

1.3.3

RC: 1.17 p.5491 “We can propagate the unit ...” → “We can define the unit ...”?

AC: We replaced “propagate“ by ”extend“.

1.3.4

RC: p.5492 You could maybe rename eq. (7) into (KCFC)?

AC: We agree with the referee that the KCFC is the most important equation of this manuscript. For reasons of consistency we would prefer to maintain the numbering order, however.

1.3.5

RC: 1.1 p.5493 “we need to propagate them ...”) “we need to prolong/extend them ...”?

AC: Done.

1.3.6

RC: 1.14 p.5493 “semi-implicit finite difference scheme”, I think “finite difference” comes implicitly with “semi-implicit” and doesn’t need to be said.

AC: We replace “semi-implicit finite difference scheme” by “semi-implicit time-stepping scheme”.

1.3.7

RC: 1.23 p. 5493 “using the Continuous Galerkin”, I would remove “Continuous Galerkin” since the statement also applies to “Discontinuous Galerkin”, the problem is the subgrid scale, not the type of approximation functions, isn’t it?

AC: Done.

1.3.8

RC: 1.6 p. 5494 “Then we consider ... of ice”, is this sentence correct?

AC: We rephrased the sentence.

1.3.9

RC: 1.16 p.5494 Sentences normally never start by a mathematical symbol.

AC: Done.

1.3.10

RC: 1.28 p.5494 How “correct” must be understood? Be more precise.

AC: We included a clarifying sentence in the manuscript at that position, and the Appendix.

1.3.11

RC: 1.29 p.5494 “cancels out over time”? you mean over mesh refinements?

AC: We refer to the point above.

1.3.12

RC: 1.15 p.5495 Some readers might be more familiar with the acronym CFL, so I would employ both.

AC: Done.

1.3.13

RC: eq. (10) You should replace the oblique symbol by a true letter to denote the kind of mask function which applies the calving only near the front. Also, better not use a dot \cdot for a simple multiplication by contrast to scalar products.

AC: Done.

1.3.14

RC: 1.16 p.5497 I don't see why π multiplies the sin.

AC: We include the factor π to attain a one year time integral of the seasonal calving rate scaling $s(t)$ of 1: $\int_0^1 s(t) dt = \int_0^1 \max(0, \pi \sin(2\pi t)) dt = 1$.

1.3.15

RC: 1.8 p.5498 "Increased ... retreats", you say twice the same thing in the same sentence.

AC: Done.

1.3.16

RC: 1.11-12 p.5498 "Resulting ... (Fig.2)", consider rephrasing.

AC: Done.

1.3.17

RC: 1.15 p.5498 say that this refers to Exp. C4.

AC: Done.

1.3.18

RC: 1.26 p.5498 "peaking at the point of further retreat" sounds redundant.

AC: We rephrased the sentence.

1.3.19

RC: 1.12 p.5499 "discrete location", could you clarify me what you mean by "discrete"?

AC: We see that the adjective is not appropriate, so it is dropped.

1.3.20

RC: 1.8 p.5502 “The non-linear rheology softens” could be further accurate like “The ice rheology softens” or “The Glen’s flow rheology softens” since “non-linear” could also mean the other sense (higher strain implies more viscous).

AC: Done.

1.3.21

RC: 1.20 p.5504 the velocity would better read $v = (\cos(\pi/4), \sin(\pi/4))\text{km a}^{-1}$ (without dot and without mixing unit in the definition).

AC: Done.

1.3.22

RC: 1.25 p.5504 What means the “standard deviation of the numerical error”, standard deviation means you have a large number of point? I would have expect to simply consider one norm of the error with respect to mesh size.

AC: We have rephrased the sentence.

1.3.23

RC: Figs 5,7 It would be simpler to print “exp. A, B1, B2 and B3” on each figure instead of using intermediary letters a), b), c) and d).

AC: Done.

1.3.24

RC: Fig. 7 Even if this is for improving the readability, I’m not sure I like the shift by 0.5 factor because the curve gets wrong. What about simply splitting the y axis into several ones (shifted each other)?

AC: We added a second y-axis in the plot in the colour of the velocity value to the right hand plot column.

1.3.25

RC: Suggestion: “along-trough” \Rightarrow “longitudinal”, “across-trough” \Rightarrow lateral/transversal.

AC: We would like to highlight that the profiles are oriented along and across the deep trough, which happens to be roughly parallel and transverse to the lat/lon degree grid. However, the description “longitudinal”/“lateral/transversal” does not capture the characteristics of the tracks accurately. We therefore prefer to maintain this description.

2 Referee 2 (Jeremy Bassis):

2.1 General Appreciation

2.1.1

RC: 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.

AC: Done.

2.1.2

RC: 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.

2.2 More detailed comments on the Level Set Method

2.2.1

RC: 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.

AC: We intended figure 3 to clarify this, but realize that contour lines of the LSF are missing in order to do this. We therefore added contour lines of the LSF to this figure.

2.2.2

RC: 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 $\varphi = 0$. 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:

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

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

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

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.

AC: We understand that the derivation and motivation of the Level-Set Equation was not clear enough in the manuscript. We now include the proposed derivation for the Level-Set Equation in the manuscript.

2.2.3

RC: 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 particular, 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.

AC: We’d like to point out that we are not using a calving rate parameterization in this manuscript, but need to provide both a 2D calving rate field and ice velocity. Ice velocities therefore need to be extrapolated onto the ice-free domain, as noted in section 2.2 of the manuscript. However, we see that we have not been clear enough here, and rephrased the explanation in section 2.2 and the motivation for the choice of calving rate in section 3.2. for more clarity.

2.2.4

RC: 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.

AC: We refer to point 1.2.2, and the last paragraph of section 2.3 in the manuscript.

2.2.5

RC: 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 its own 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.

AC: We discuss the reinitialization of the LSF and the loss of mass-conservation in point 1.1.2. Spatial resolution of calving front features which we can resolve is on the scale of the front element diameter (500 m). The artificial diffusion we use is small, especially when compared to current uncertainties in observed calving rates.

2.2.6

RC: 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 $x = \sin(Nmx/L$, 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.

AC: The proposed setup, found as “Ice Shelf Ramp” in for example Greve and Blatter (2009), is a 1D setup, for which an analytical solution exists. ISSM is able to numerically reproduce the analytical solution (see Fig. 2). However, since the thickness of the ice tongue is non-linear once it is allowed to expand freely, numerical and analytical solutions are then hard to compare. We have plotted the evolution of the ice volume in the manuscript, Fig. 11, and refer to point 1.1.2.

2.2.7

RC: 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

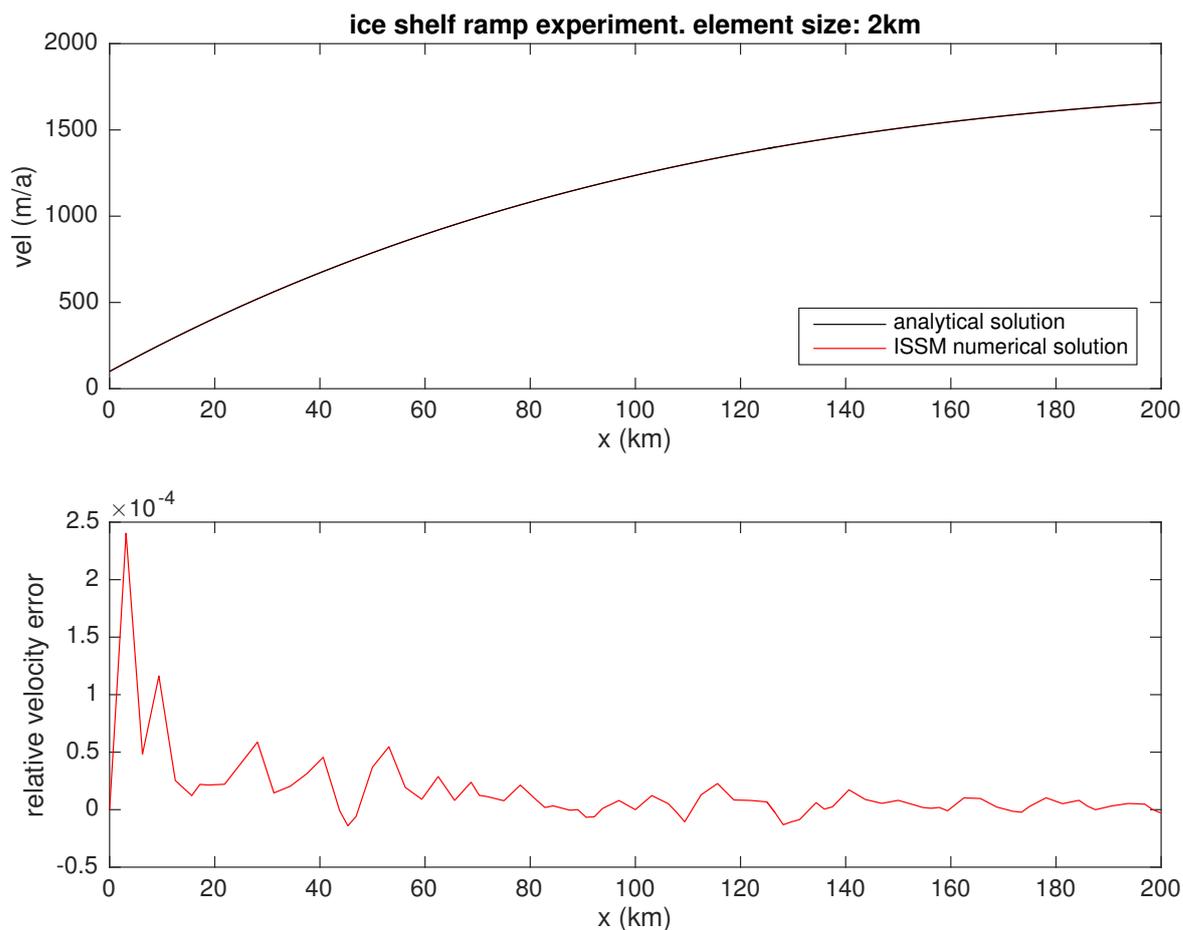


Figure 2: Top plot: Analytical and numerical solution to the Ice Shelf Ramp setup. Bottom plot: Relative error of the numerical solution.

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.

AC: This is a good point and we think the circle experiment in Appendix 1 addresses this question.

2.3 Experiments

2.3.1

RC: 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.

AC: We refer to our addition to section 3.1 for arguments why the qualitative results of this section however do remain valid for Jakobshavn Isbræ.

2.3.2

RC: 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.

AC: We de-emphasized those results in the manuscript, and added references to the respective literature, see results and discussion part.

2.3.3

RC: 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 et al. (1982) 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?

AC: We refer to point 2.2.2 and section 3.2 in the manuscript for motivation of the chosen calving rate field. For the fact that both observed and modelled calving front positions show qualitative agreement, see also an added comment at the end of the discussion.

2.3.4

RC: 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 Jakobshavn.

AC: We refer to the answer in point 2.3.1 above.

2.4 Minor comments

2.4.1

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

AC: Done.

2.4.2

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

AC: Done.

2.4.3

RC: page 2518: What is a floor line??

AC: We meant flow line, of course. Done.

2.4.4

RC: 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.

AC: Using basal effective pressure as a factor in the basal stress boundary condition is commonly used in ice sheet modelling. We agree that this is a crude approximation far from the grounding line, and have added a note in section 2.1.

2.4.5

RC: 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.

AC: We refer to the earlier point 2.2.2 and section 3.2 in the manuscript for motivation of the chosen calving rate field.

2.4.6

RC: Page 5494: The 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?

AC:

We discuss the question of volume conservation in point 1.1.2 and the Appendix, and motivate the choice of extrapolation in sections 2.2 and 2.3 of the manuscript.

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