

Anonymous Referee #2 comments and response

We would like to thank the reviewer very much for the time and effort put into reviewing our manuscript. A point-by-point reply (in blue) to each comment (in black) by Referee #2 are given below. Specific changes in the manuscript are written in blue italics.

This work makes modifications to a well-known and widely used glacial flow model Elmer-Ice to allow an approximation to the nonlinear Stokes problem – the Shallow Shelf Approximation – to be solved for a large proportion of the floating part of the domain, in such a way that the two solves are consistent with respect to the force balance. The significance of such a modification is the expense involved with the Stokes solve. Not only does it have more degrees of freedom, but it leads to a problem which in the variational sense is a saddle-point and not a minimization problem, leading to difficulty in discretisation; and, though it is not flagged by the authors, there is an extra complication in solving for ice shelves, as a floatation condition cannot be assumed, allowing for oscillation and instability in the vertical momentum condition that must be artificially damped. The work is a nice follow-on slash complement to two other works in the literature: Seroussi et al 2012, which couples together different approximations to the FS problem in a diagnostic setting; and Ahlkrona et al 2016, which dynamically couples FS to SIA in an evolving ice sheet, and is deserving of publication.

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i only have 2 general comments:

1) My one general comment is that the underlying premise seems to be that this method will reduce computational resource requirements. The reduction in the test cases seems to be low; but this is explained in the discussion, and I am not presently commenting on this. It is rather that presumably this coupling is meant to be applied to regional and continental scale simulations of marine ice sheets. As SSA is only solved on the shelf, the savings are limited by the portion of the domain covered by ice shelves. Quick googling tells me that ice shelves currently represent 10% of Antarctic total area – significantly less than in the test simulations in the paper. So how much more efficient is such a coupling meant to be compared to FS only continental simulations?

When considering the coupling with ISCAL, the gain by having FS-SSA is not just 10% because much of the interior can be modeled with SIA, such that the area left with FS are mainly (around) ice streams and shelves. Also, we are aiming for paleo-simulations, which may have larger ice shelf in the cold period.

We agree that this was not explained in the manuscript yet and have added it to the introduction: *“The extent of present-day ice shelves is limited to approximately 10 % of the area of Antarctica (Rignot et al., 2013). Therefore, one may question the reduction of computational work by applying SSA to model ice shelves in continental scale simulations of marine ice sheets. However, the coupling is targeted to conducting paleo-simulations, for which much larger ice shelves have been present (Jakobsson et al., 2016; Nilsson et al., 2017). Besides that, the new FS-SSA coupling can be combined with ISCAL. Then, a large part of the interior of a marine ice sheet will be modelled with SIA, such that the FS domain will be restricted to ice streams and areas around the grounding line, when SSA is applied to the ice shelves.”*

But I am curious about the issue that i bring up in my first paragraph, but was

not addressed. Durand et al 2009 discusses their approach to prevent instability in the vertical due to ice shelves not being in floatation (they added a degree of implicitness to the velocity solver – their eqn 15). I have wondered if this potential instability affects the solution of FS for a marine ice sheet, and possibly artificially enforces archimedean floatation. I would be curious to know about how your coupling affects this issue ,i.e. the need for the "fix" – could you remove the fix for your coupled experiments?

We have not tried to remove the fix for our coupled experiments. In Elmer/Ice, the Stokes and surface evolution equations are coupled by explicit time stepping, because we only solve the coupled system once per each time step. To solve the fully coupled system implicitly is very costly and unnecessary in most of the cases in ice sheet simulation.

One exception is the flotation, since it is a fast process compared to the dynamics of the upper surface of the ice. As you mention, the term added in Durand et al 2009 indeed behaves as an implicit time stepping for the motion of the lower ice surface in contact with water. In this case, it is not an additional term, but the whole eq (15) is an approximation of the surface equation. One can show that by taking the time step sufficiently small, the error in the position of the lower ice surface is as small as we wish in Elmer/Ice and therefore we have not tried to remove it.

2) There is not much discussion on how the FS and SSA domains are updated dynamically (node reassignment etc). Although you touch on it in the discussion, i would like to see a subsection in the methods section briefly detailing this, even if it makes use of already-existing frameworks. Apologies if such text is there and i overlooked it.

Thanks for pointing this out, indeed the manuscript will improve by adding more information on the technical implementation of the coupling, this information is added to Sect. 3.3 The algorithm: *“First, the shortest distance d to the grounding line is computed for all nodes in the horizontal footprint mesh at the ice shelf base. Then, a mask is defined that describes whether a node is in Ω_{FS} , Ω_{SSA} or at the coupling interface x_c , based on the user defined d_{GL} . Technically, the domain decomposition is solved by the use of passive elements implemented in the overarching Elmer code (Råback et al., 2016), which allow for deactivating and reactivating of elements. An element in Ω_{FS} is declared passive for the SSA solver, such that is not included in the global matrix assembly of A_{SSA} , and vice-versa. An element may switch from Ω_{SSA} to Ω_{FS} , for example during grounding line advance. Then, the coupled iteration either starts with the initial condition for u_{FS} if the element is in Ω_{FS} for the first time, or the latest $u_{FS}(t)$ computed in this element, before it switched to SSA.”*

p2 l 10: MISMIP3D

It is called MISMIP3d in the title of the paper (Pattyn et al., 2013).

p3 line 7: strain rate tensor

Changed.

p3 l 9: here they are just the Stokes equations. ("Full Stokes" arose because glaciologists realised the equations they had been solving for years were an approximation to Stokes with power-law rheology)

Since our main target public are glaciologists we will keep it like that.

eq 4: your notation for the the 2nd invariant of tensors (in this case D) is a bit subtle, you might consider something else – this is simply a suggestion though

We have written out the 2nd invariant in velocity gradients, Eq. (5) in the revised manuscript.

p3 l 22: "h represents the horizontal components" is a bit ambiguous, and you do not

use the subscript in eq (6)

We have rewritten 'h represents the components in the x-y plane', and have added the subscript to Eq. (6) (Eq. (7) in the revised manuscript) as well.

eq (9): is z^* a function of effective pressure, which is not defined, or of H as it appears to explicitly be from eq (10)?

Indeed, it is a function of H, which we clarified by writing

"In line with Gladstone et al. (2017), instead of modeling N , a hydrostatic balance is assumed to approximate z^ , the dependence on H is written, $z^*(H)$."*

Section 2.3, abbreviations of two dimensions and three dimensions seem awkward, and be clear by 2D you mean the x-z plane.

Thanks for pointing this out, we have rewritten the section concerning two and three dimensions to:

"The number of nodes in Ω_{FS} is then approximately $(1 - \theta)N_h N_z$ and in Ω_{SSA} it is θN_h , neglecting shared nodes on the boundary. For a 3D physical domain, FS and SSA have 4 and 2 unknowns, respectively. Hence, the memory needed to store the solution with a coupled model is proportional to $2N_h(\theta + 2(1 - \theta)N_z)$. For a 2D simulation in the x-z plane, where FS has 3 unknowns and SSA only 1, the memory is proportional to $N_h(\theta + 3(1 - \theta)N_z)$."

p 5 l 22: say what 3 variables are for FS in 2D.

After rewriting of the section, this suggestion did not seem applicable anymore.

p 5 l 23: say what you mean by stokes being a saddle-point problem as many readers will not know what this means.

We think this it is not necessary to define a saddle-point problem here, but have rephrased:

"Furthermore, the FS equations are particularly difficult to solve. In mathematical terminology, they pose a saddle-point problem."

p 5 l 23: "with all its consequences for numerical treatment" – give examples of these consequences, with references. Most readers will not be familiar with this literature, and others (like me) will be only familiar with some of it.

We have added *"This requires special numerical treatment such as stabilization of the finite element discretization (e.g., Helanow and Ahlkrone, 2018) and special iterative solvers for the resulting system of linear equations (Benzi et al., 2005). By elimination of the pressure, the SSA equations do not form a saddle-point problem."*

p 6 l 25: just θ , not θN_z

Thanks, changed.

p6 l 26: say this is approximate, as it must be due to boundaries etc.

We have added that it is approximate since boundaries are neglected.

p6 l 6: by "number of unknown variables" do you mean degrees of freedom; or (u,v,w,p) i.e. 4 versus (u,v) i.e. 2? You are saying that, per node, the former takes twice as long to assemble in a matrix versus the latter? I find this difficult to believe in general. What about differing orders of polynomial in the basis functions, and complexity of interaction between DoFs of different variables?

This is also an approximate expression. If the order of the methods is the same and the complexity of the interaction between the components is similar then the formula is an estimate. Changed to:

"The work to assemble the matrices grows linearly with the number of unknown variables.

Suppose that this work for FS in 3D is $4C_{FS}N_h N_z$ in the whole domain, for FS $4C_{FS}(1-\theta)N_h N_z$ in Ω_{FS} , and for SSA $2C_{SSA}\theta N_h$ in Ω_{SSA} . The coefficients C_{FS} and C_{SSA} depend on the basis functions for FS and SSA and the

complexity of the equations. The reduction in assembly time for the matrix is $q_{ass} = 1 - \theta + C_{SSA} \theta / 2C_{FS} N_z$. If $C_{FS} \approx C_{SSA}$ then the reduction is approximately as in (16). The same conclusion holds in 2D. Therefore, the reduction of that part is estimated to be similar to the reduction in Eq (15).”

p 6 l 16: is there a theoretical basis for d_{GL} – such as the estimate of the non-hydrostatic boundary layer from Schoof (2011, JFM)? Or arbitrary?

We have no theoretical basis for d_{GL} . As mentioned in the discussion we propose that further studies let Ω_{SSA} be determined automatically, not necessarily based on d_{GL} but for example based on a tolerance for the vertical variation of the horizontal velocities (that should be close to zero in order to allow for a smooth coupling to SSA) or by using a posteriori error estimates based on the residual as derived in Jouvét (2016).

p 7 eq (17): this is an initial guess for the membrane stress at the interface, yes? Would a better one not be $\sigma_{FS} \cdot \vec{n} = \rho_w g(-z)$ if $z < 0$, i.e. the condition which would be applied if x_c was a calving front? (This would be exact in the 2D case with no buttressing seaward of x_c ; and i wonder if it would reduce iteration count in general)

Good point, probably it would reduce iterations, we have considered this initial guess during the implementation as well. However, this would only be beneficial during the first time step, or when the coupling interface x_c has changed position. Any other time, the algorithm will take f_{SSA} from the previous iteration and this will thus be a good initial guess.

p 9 l 12: "assembly time ... almost doubles" – i think this is the issue you address in the discussion? would be good to say that it is addressed in the discussion, as i was confused by this when i read it.

Thanks for pointing this out, we have added “*This issue is due to usage of passive elements and is addressed in the Discussion (Sect. 5).*”

p 9 l 15: is v velocity in the y -direction? better say so. aside from no normal flow, what is the other BC at the lateral boundary? no stress? no flow?

We have changed $v=0$ to $u \cdot n=0$. The unset boundary conditions for remaining velocity components, by the natural boundary condition resulting from a partial integration of the stress divergence in the weak formulation, automatically apply a vanishing Cauchy-stress vector in that direction. This information has been added to the Appendix, below Eq. (A3):

“*Furthermore, there is a lateral boundary Γ_l for $\Omega_{FS} \in \mathbb{R}^3$, where the normal component also vanishes: $v|_{\Gamma_l} \cdot n = 0$ and we assume a vanishing Cauchy-stress vector for unset boundary conditions to velocity components, such that the integral over Γ_l vanishes.*”

p 11 line 8: 58% of the nodes – you mean in the projection of the grid to the x - y plane?

Yes, thanks for pointing this out, changed to “58% of the nodes in the horizontal footprint mesh are located inside Ω_{SSA} ($\theta = 0.58$)”.

p 12 paragraph at line 13: found this discussion a bit difficult to follow – is there any way the main performance points can be summarised in a table? also limited \rightarrow varied

Thanks for pointing this out, we have added a table and reduced the amount of information in this paragraph, referring to the table instead.

p 14 discussion at line 10: if you were to make this change re: node assignment at a lower level, how would it affect the ability to easily update the Full Stokes sub-domain?

This would not affect the ability to easily update the Full Stokes sub-domain, it would be based on the same mask as the passive/active updates are done now (see the extra information added in Sect. 3.3, in reply to the second general comment). It was shown for ISCAL that this can be done efficient and dynamically in Elmer/Ice (Ahlkrona et al. 2016).

p 16 line 24: you previously used A as a symbol for a matrix

Thanks, changed to B such that there is no confusion with A being previously defined as a system matrix. However, B (or A in old version) is a matrix, with derivatives in the elements.

p 16 line 4: did you define f_{CF} ?

We had only given the FS version of the boundary condition at the calving front (Eq. (12), (13) in the revised manuscript) and added: " f_{CF} , as in Eq. (13) but integrated over z " to the Appendix.

p 16 eq (A7) and below: suggest to use w as a test function symbol

We chose to stick to v for a test function.

p 16 eq (A9) the 1st boundary integral is over Γ_{SSA} not Γ_{SSAint}

Indeed, updated.

References

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