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Geoscientific Model Development Discussions

Interactive comment on "Dynamically coupling Full Stokes and Shallow Shelf Approximation for marine ice sheet flow using Elmer/Ice (v8.3)" *by* Eef C. H. van Dongen et al.

Anonymous Referee #2

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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.

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? 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?

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.

Specific comments:

p2 l 10: MISMIP3D

p3 line 7: strain rate tensor

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)

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

p3 l 22: "h represents the horizontal components" is a bit ambiguous, and you do not use the subscript in eq (6)

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)?

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

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

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

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.

p 6 l 25: just \theta, not \theta Nz

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

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?

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?

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 xc 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)

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.

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?

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

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 –> varied

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?

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

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

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

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

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