The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-193

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Referee comment on "Comparison of ice dynamics using full-Stokes and Blatter–Pattyn approximation: application to the Northeast Greenland Ice Stream" by Martin Rückamp et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-193-RC2, 2021

General comment

The paper addresses the question whether the computationally expensive full Stokes equations are really necessary, or if the cheaper Blatter-Pattyn model is sufficient. They do so by simulating a part of the NEGIS ice stream with both models using the same code (COMSOL) and as similar numerical discretization as possible. The results are then compared using a series of different measures. The experiments show that unless the ice is very stiff, the difference in velocity field amounts to a few percent, but that there is potentially a larger difference in internal ice deformation, which may have implications for paleo reconstructions.

The topic is a very important one and this type of paper is needed. I also appreciate that the authors measure the error in several different ways. However, the study is limited, and I am afraid that the reader might draw the overhasty conclusion that full Stokes is not needed based on these results, while there might be other situations where full Stokes is important. The main limitation as I see it is:

- The lack of a grounding line experiment: I understand that it might be difficult to implement grounding line migration in a commercial software, but I think at the very least the authors can look at a time-independent grounding line problem, comparing velocity fields and buoyancy balance.

Also, I think the authors should consider the following issues:

- The lack of time dependence: The study does not include time dependent simulations. The impact of the model differences on surface evolution can be measured without actually running a surface evolution, I think it would suffice to look at how the velocity field would change the surface after one time step. However, I wonder if it could be a problem that the initial surface has not been relaxed. Are we looking at an artificial initial shock transient? Is that relevant? I would like to see at least a discussion justifying the lack of relaxation.

- The boundary conditions are retrieved by inverting with BP (and a different code). What does it mean for FS that the boundary conditions are consistent with another model? It would be interesting to see another set of experiments, where the slip coefficients are retrieved using FS (e.g. with Elmer) and are then used for both FS and BP. Also, would inverting at a higher resolution make a difference? Would there be high frequency effects that FS would pick up?

- Some tests regarding the numerics are missing. Since quite some effort is taken to treat the models with similar numerics, I would like to see some tests or discussion convincing the reader that the discretization does indeed not impact the result, since the interest is in quite small velocity differences. In particular, the inf-sup stabilization parameter may not be the same for FS and BP (it is not clear if the same stabilization is used for the BP system, but I assume so), and the problem could, if you are unlucky, be sensitive to this. Either change to Taylor-Hood elements or check that varying the

stabilization parameter for the inf-sup stabilization does not alter results. Also, the element aspect ratio varies in the experiments, as the number of vertical layers are constant. Will the numerical errors of FS and BP behave the same when element aspect ratio changes? Perhaps this is not relevant but if so, a comment on why should be included.

- The idea of studying internal layers (section 5.5) is nice, but this part of the study is too limited.

Specific comments

Line 30 - worth to mention that that FS simulation was with coarse resolution

Line 76 - Consider changing the title "field equations" to e.g. "the full stokes equations"

Section 2.3 Explain to the reader in what situation all of the neglected stress components are important. Which are important for shearing margins, which are important at grounding line, etc.

Equation 9: Why breaking out 1/2 but not 1/4?

Section 2.3: Comment on that normally one would manipulate the system in the style of this page: http://websrv.cs.umt.edu/isis/index.php/Blatter-Pattyn_model , maybe write down the "normal" BP system so that the reader more easily understands what you mean by "BP-like" in the next section

Line 112: Add a reference for the difficulties of saddle point problems

Line 137: I worry there could still be numerical issues with how the saddle point FS

system

is solved

Section 3.2: Is the BP-like system symmetric? Does that matter for the linear solver you use?

Line 149-161: shorten this paragraph

Line 180: The paragraph starting here can be clarified, especially for readers who does not have ISMIP-HOM details fresh in mind. Also I think it is the first time the abbreviation HO appears.

Line 193: Comment on why you choose this sliding law, and mention already here that k is to be found with inversion

Figure 1: To me it seems the solutions in Experiment C does not seem to agree well with previous exercises. Comment on this.

Line 207-211: The boundary conditions are not completely clear to me, please write them down as equations.

Line 215: Did you experiment with sensitivity also with respect to vertical resolution? If not please do. Note that the element aspect ratio will change if you only change horizontal resolution, probably it is not an issue here but in worst case it can impact numerics.

Line 222 - 225: Mention that SICOPOLIS use SIA/SSA

Line 226-228: This is an important point to at least discuss in your study. You find a friction coefficient that is consistent with the BP-like model, but use it also for the FS model.

Line 230-233: This paragraph seems a little bit out of place, and the table could be moved to the appendix

Section 5.2: Here I would appreciate a discussion relating the differences to the missing stress components, or perhaps you can just mention that it will come in section 5.4

Line 273: Mention that a discussion on why stiff ice is more sensitive will come in section 5.4

Section 5.4: I appreciate this section!

Line 302: Write vb/vs first, to be consistent with the order in line 300

Line 300 - 310: This is a good experiment. However the figure (Figure 7) is hard to read. Also, comment on how you think the fact the elements are flatter for high aspect ratios impact the result, or why they don't impact the result.

Line 311-314: This is an important check. I think not all readers will understand why you look at the vertical velocity, add a sentence to explain. Perhaps even better, would be too look at how much the surface would move in one time step given this velocity field (this should be easy to compute, you don't have to actually move the surface)

Section 5.5: I like the idea of looking at internal layers, but this part of the study is quite incomplete

Section 5.6: This section fits better after section 5.3

Minor language/esthetics comments:

Line 26 - "Although BP neglects severe.." - is severe the right word to use?

Line 48 - “different results as simpler models”, “as” -> “compared to?”

Line 56 - The sentence starting with “Beside the..” is a bit akward

Line 61 - “consistent analysis” - change for “consistent numerical experiments”?

Line 95 - The sentence “Boundary condition ...is traction free” does not seem to be grammatically correct

Line 99 - “v_b the velocity” -> “v_b is the velocity”?

Line 171: The sentence about the mmr contribution is a little bit confusing

Line 236: This sentence is hard to read

Figure 4: The scatter plots are a very small

Line 330: This sentence is unclear, adding a “particle” or “layer” would help

Figure 6: Increase the font

Line 395: “Seems not an urgent issue” -> “does not seem to be an urgent issue”?