

The Cryosphere Discuss., referee comment RC2 https://doi.org/10.5194/tc-2021-193-RC2, 2021 © Author(s) 2021. This work is distributed under the Creative Commons Attribution 4.0 License.

Comment on tc-2021-193

Josefin Ahlkrona (Referee)

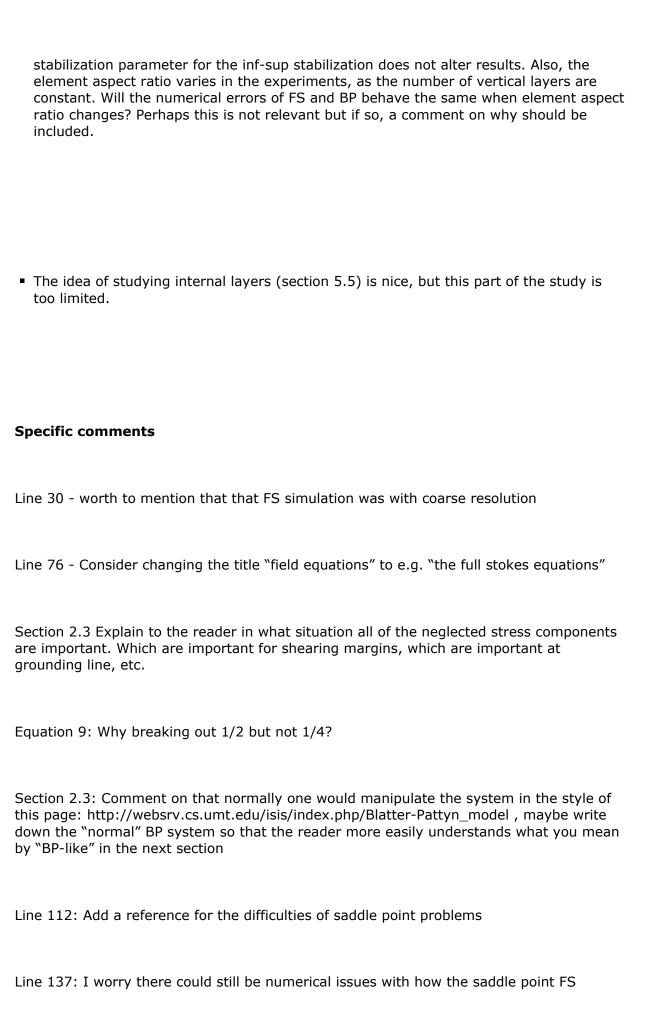
Referee comment on "Comparison of ice dynamics using full-Stokes and Blatter-Pattyn approximation: application to the Northeast Greenland Ice Stream" by Martin Rückamp et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-193-RC2, 2021

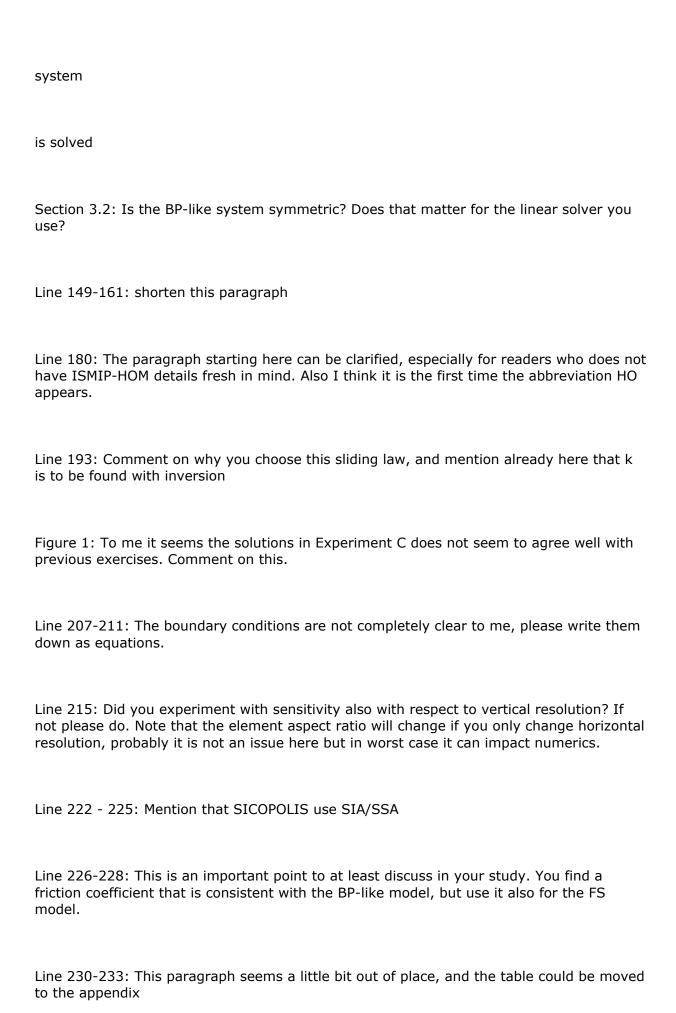
General comment

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.
Al	so, 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





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

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

Minor language/esthetics comments:

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"?