

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-193

Anonymous Referee #1

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-RC1>, 2021

General comments

This study addresses a longstanding question in ice sheet modeling: Are there regions where full-Stokes (FS) models are much more accurate than the Blatter-Pattyn (BP) and other higher-order (HO) approximations, and therefore FS models are needed for accurate sea-level projections? By implementing FS and BP-like solvers using the COMSOL finite element package, the authors have developed a useful tool for exploring differences between FS and BP, without complications due to differing numerics. They apply these solvers to the central part of the Northeast Greenland Ice Stream (NEGIS) at a wide range of resolutions (0.1 km to 12.8 km) and find modest differences between FS and BP. As expected, the differences are greatest at fine resolution and in regions with a high slip ratio, high aspect ratio, and/or rough topography. Differences are small at resolutions of 1 km or coarser, as typically used in ice sheet models. Since these differences are small compared to other uncertainties in ice sheet modeling, the authors conclude that FS models are not urgently needed for sea-level projections.

The question is an important one and is not settled. Morlighem et al. (2010) argued that when inverting for basal drag coefficients in ISSM, bridging effects near the grounding line of Pine Island Glacier (PIG) make it essential to use FS models. Other authors, including Nowicki and Wingham (2008) and Durand et al. (2009), have made similar arguments. Today, many ice sheet models with HO approximations are being used for Greenland and Antarctic projections on century time scales, while FS models are used sparingly because of their complexity and expense. As far as I know, the projections based on FS models, including Elmer/Ice, are not dramatically different from projections based on HO models. I am inclined to agree with Rückamp et al. that FS–BP differences are secondary compared to other uncertainties, but it is still important to explore these differences systematically.

The authors take a step in this direction with their detailed NEGIS analysis, but this step does not go far in settling the question. They focus on part of a single ice stream without

addressing other regions, such as PIG, where stress-balance terms neglected by BP could be important. As the authors acknowledge, their analysis does not include prognostic simulations or thermomechanical coupling, and they have tested only one basal sliding law. So the analysis does not strongly support the conclusions.

I would not expect these complex issues to be resolved in one paper, but I would like to see a broader analysis. For example, the authors could simulate an entire ice sheet in a transient run of a few years at the highest affordable resolution. Or they might look at regions where others have argued that FS models are needed. Perhaps this is impractical for the COMSOL solver and would require a different model such as ISSM. But I would like a sense that having developed the COMSOL tool, the authors have pushed it as far as they can. I encourage them to consider other applications that would strengthen their main argument.

Also, the paper needs significant editing for correct English.

Specific comments

Abstract, l. 12: "severe impacts on internal layers of ice sheets". The discussion of englacial advection is limited and does not justify this strong and rather vague statement in the Abstract.

l. 15: "no simplification". This is a bit strong; all model equation sets have some simplifications (e.g., isotropic, temperature-dependent flow factors in FS models).

l. 26: Please cite the papers by Blatter and Pattyn here rather than below at l. 62. Also, I think "severe" should be "several". (Maybe this was also true in l. 12?)

l. 30: "only one contribution". I think this is the Elmer/Ice model? Please include the model name and a citation.

l. 39: "with the analytical solution". I don't think the ISMIP-HOM experiments have analytical solutions. The FS results serve as a benchmark to which HO models are compared.

l. 40: "is prohibited". This is not the right word. Maybe "is not possible"?

I. 43: "huge additional computational amount". Is it possible to give an order-of-magnitude estimate of the additional cost?

II. 45–55: This paragraph mentions some studies that used FS, but it would be helpful to the reader to give more details, for example the argument of Morlighem et al. (2010) that FS is needed near Antarctic grounding lines. Of the three difficulties listed here for FS–BP comparisons, only (i) is addressed by the present study. I wish the authors had pressed their analysis further; see the general comments.

I. 52: "which resolves, e.g. bed differently". The meaning here is not clear.

I. 60: "certainly because FS and higher-order models are too expensive for these long-time integrations". Some HO models, especially depth-integrated models (e.g., Goldberg 2011), are, in fact, practical for long time integrations.

I. 67: "Therefore..." This is not a strong argument for focusing on a subset of NEGIS. Does this region have characteristics that could make it especially challenging for BP models as opposed to FS? The text mentions bed topography, and later slip and aspect ratios, but it is unclear that central NEGIS is an appropriate analog for, say, the PIG grounding line or for glaciers with rougher topography.

I. 74: "ratio between basal sliding and gravitationally driven flow". I think this should be "basal sliding and internal deformation".

I. 100: "usually includes the effective pressure". I suggest "often includes". Weertman-type power laws, for example, do not include effective pressure.

II. 130–140. I like this approach to coding FS and BP in a way that isolates the discarded stress terms and minimizes differences in model numerics.

I. 164: "limiting case". I think what is meant here is that each solver has a range of resolutions where it is applicable, and 0.4 km lies in the overlap region. Please clarify; it would help to state the range of applicability for each solver.

I. 188: "On purpose...". Why was a region chosen far upstream of the grounding line, given that grounding lines are important for sea-level projections and might be regions where FS–BP differences are large?

I. 193: Why a Budd-like friction law? Most ice sheet models are now using a power law, a Coulomb law, or some hybrid combination. See, e.g., Sect. 2.1 of Asay-Davis et al. (2016). In general, the analysis would be more compelling if it included more than one basal friction law.

I. 209: What is meant by a symmetry BC? Which field or fields are symmetric across the boundary?

I. 212: The wide range of resolutions is a strong point of this study.

I. 220: "very extreme". I'm not sure why the authors chose some unrealistic values. I would suggest three values: $E = 1$, plus values that are on the low side and the high side but still physically plausible (say, 0.5 and 3). Below, when there are FS-BS differences with $E = 0.1$ or $E = 6$, it is hard to know whether to take those differences seriously.

I. 256: "up to 43 m/a compared to FS". To give readers a sense of the percentage error, please state the FS value.

I. 260: "for very soft ice...". Since $E = 6$ may be unrealistic, the significance of these differences is unclear. Similarly for the stiff-ice case, I. 273. See the comment above.

I. 273: "Maximum differences...". Is this in relative rather than absolute terms?

II. 315ff: I don't think Sect. 5.5 adds much to the paper. I would guess that the FS-BP differences are small compared to the vertical diffusion that would be associated with remapping variables onto a modest number of layers, but this could only be shown in a prognostic run. I would drop this section if it isn't possible to say more.

I. 330: "whether FS or BP-like is located below or above each other". Please clarify what this means.

I. 351: "particularly in extreme cases..." See comments above about unrealistic E values.

I. 359: "It might be favorable..." I agree with the statement, but it is not strongly supported by the single example. See general comments.

I. 361: "our simulations are not prognostic". As stated above, this is an important limitation. Analyzing a prognostic problem, if possible, would strengthen the paper.

I. 367: The authors cite Morlighem et al. (2010), but that paper draws a different conclusion (that FS models are essential). Does the NEGIS analysis cast any doubt on the Morlighem conclusions?

I. 375: "there are indications that small initial differences become much larger over long time integrations." It is good to acknowledge a study's limitations, but this is another example of the analysis being too limited to draw broad conclusions.

I. 382: "The model disagreements still tend to diverge below...". This wording is unclear. Maybe "The models still do not agree at..."

I. 387: "a view on particle pathways..." Again, I think a diagnostic run is not sufficient to draw conclusions on englacial layering.

I. 391: "FS will start to matter...". This has been shown only for regions that are like NEGIS in relevant ways, where there are no differences introduced by long-term advection, thermodynamic evolution, or grounding lines.

I. 395: "the use of FS seems not an urgent issue." See the general comments.

Tab. 1: This is a short list of constants. Are there any others?

Fig. 4: The inset panels in the upper left of each panel are not described in the caption and are hard to read. Possibly expand to full panels with a separate caption.

Fig. 5: The axis labels are hard to read.

The figures labeled as A2 to A10 are not related to Appendix A. These should be included instead as supplementary material.

Technical corrections

The paper contains many grammatical errors and uses of non-standard English. This is a partial list.

Title: Usually I have seen "Northeast" rather than "North East" for NEGIS

l. 2: "its owing" is not idiomatic

l. 3: "consequences caused by" is redundant

l. 7: "increases" -> "increase"

l. 15: "is by using" -> "is given by"

l. 26: "computational" -> "computationally"

l. 32: "fidelity to accurately simulate" is awkward. Maybe "ability to accurately simulate"

l. 43: "computational amount consumed by" -> "computational cost"

l. 55: "processes and interactions that **make** it difficult"

l. 68: "higher variable" -> "highly variable"

l. 74: "the enhancement factor" -> "an enhancement factor"

l. 83: "isometric" -> "isotropic"

l. 99: "outwards of" -> "out of"

l. 109: "explained below" -> "as explained below"

l. 110: "are forming" -> "form"

l. 122: "simplifications ... reduces" -> "simplifications ... reduce"

l. 126: "are following" -> "follow"

l. 147: "computational amount" -> "computational cost"

l. 188: "upstream **from** the grounding line", "downstream **from** the ice divide"

l. 193: "friction type" -> "friction law"?

l. 232: "towards" -> "and", "consumed computational resources" -> "computational costs"

l. 236: "compared exemplary" -> "compared"

l. 244: "excepted" -> "expected"

l. 264: "unveils" -> "shows" or "reveals"

l. 267: "show increasing trends" -> "increase"

l. 293: "leveled out" -> "compensated"

l. 309: "less" -> "fewer"

l. 378: "alleviate" -> "allow" or "enable"

l. 379: "issues" -> "differences"

l. 393: "uncertainties" -> "uncertainties in"

Tab. 2 caption: "Number for" -> "Number of", "exemplary listed" -> "listed"

References

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