

The Cryosphere Discuss., referee comment RC2
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Review comment of: A comparison of the performance of depth-integrated ice-dynamics solvers

Anonymous Referee #2

Referee comment on "A comparison of the stability and performance of depth-integrated ice-dynamics solvers" by Alexander Robinson et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-239-RC2>, 2021

Review of "A comparison of performance of depth-integrated ice-dynamics solvers"

Summary:

The manuscript by A. Robinson and colleagues proposes an analysis of the performance of several depth-integrated stress balance approximations by combining a theoretical analysis of the performance with a comparison of results from several existing ice flow models that include them. They start with a 1d case and then compare the results for a more realistic simulation of the Greenland ice sheet. The paper is well written and usually clear to follow, though a few steps (listed below) could use some more details to facilitate following the derivation of the stability analysis. In the case of the L1L2-SIA solver, the results are quite different between the stability analysis and the two Yelmo and CISM applications, but there is no clear explanation of this difference. I would be curious to learn what the authors think it the source of this discrepancy. Detailed comments below list relatively minor points, mostly to clarify the manuscript.

Detailed Comments:

p.1 l.6: Add that the simulations are done with finite differences and that you use con Neumann stability analysis.

p.1 l.18: thousands -> maybe put a more exact number

p.1 l.19: kilometers -> square kilometers

p.1 l.20: add that this is difficult to do at the high resolution required to accurately represent important processes

p.2 l.7: I don't think the SIA is the most widely used approximation anymore (at least not used alone), it's more historical and used to be the most common one

p.4 Eq.6: I was a bit confused by this integration, it would help to put a few more details on the steps to get to this form

p.5 Eq.9: Is that form assumed for all the approximations? If so, mention it here.

p.5 Eq.10: It is not clear how this integration is performed with the varying μ , add indication of the main step/information needed, I could not figure out how to get to this result.

p.6 l.13: What are the implications of having a frozen bed with non zero basal stress? In which cases is that possible?

p.6 Eq.19: It might be simpler to say that in that case $\beta \gg 1$ so β_{eff} converges toward $1/F_2$

p.7 l.8: "in in" -> "in its"

p.7 Eq.22: Add an explication about the derivation of this equation coming from the SIA and the parallel and normal components of τ .

p.7 l.10: maybe add that this kind of analysis is designed for finite differences

p.9 Eq.32: Add where it comes from and the assumptions made.

p.9 l.33: Perturbation of what?

p.13 l.25: What is the conclusion?

p.13 l.21: Rewrite the entire equation given that assumption

p.17 l.9: What values are used in that range?

p.18 l.14: I would be a bit more nuanced that it is not really the case for sliding.

p.18 l.20: It does not really agree for $\Delta x < 10^3$

p.18 l.33: Why could be the reasons why it is not stable as expected with more comprehensive models?

p.19 Fig.1: Add legend for the different lines on the figure. Caption: "in some other panels" -> "In several panels"

p.20 l.8: How do you measure it?

p.20 l.11: How much does it adaptive timestep varies temporally?

p.26 l.1: Make sure that the results and data are actually archived