

Interactive comment on “Free–Surface Flow as a Variational Inequality (*evolve_glacier v1.1*): Numerical Aspects of a Glaciological Application” **by Anna Wirbel and Alexander Helmut Jarosch**

Anonymous Referee #2

Received and published: 8 June 2020

I enjoyed reading this manuscript and feel it is an importance piece of scientific work. I however agree with the other reviewer that the title is arguably somewhat inexact, if not even a bit misleading. The manuscript does not focus exclusively on the issue of free-surface flow with a pos. thickness constraint, but is in fact much wider in its scope. The discussion about the variation inequality is short and it is actually unclear how it is applied or solved. It is also unclear to what degree this work uses the cited work of Jouver and Bueler, 2012. It appears that the thickness constraint is imply plugged into the PETSc solver. Furthermore, I would have liked to see how the active set of the KKT system is actually updated and when it is considered to have converged. That is to say, if that is indeed the solution method applied. This, and other technical details

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are somewhat missing in the manuscript.

I suggest refocusing this work and presenting it more as a new full Stokes ice-flow model. The paper is quite descriptive at times. I would have liked to see the equations and I guess some of the description could be shorted significantly by just listing the equations in their weak form and specifying the FE spaces. I felt the discussion all they way done to page 7 was very much describing the standard approach. Having said that, in the particular case of this journal, this is presumably justified, but I still feel having all the equations listed in one place as a clearly defined mathematical system might be a good option.

As far as I could see, most of the test presented related to how accurately the (SUPG stabilized) mass-transport equation is solved. The test are useful and doing these and similar test is an essential part of the model-development phase. I did not see that the thickness constraint is mass conserving, and the discussion on page 23 suggested that it is in fact not. However, almost all of the tests and the associated discussions revolved around the stabilisation of the surface elevation equation and this mass-conserving aspect was not really addressed or analysed. I in fact doubt that the thickness constraint can be locally mass conserving for any finite time step. I never saw the details of the method, but if this is solved using PETSc as a constraint minimisation problem, then I suspect the corresponding Lagrange multipliers can be thought of as fictitious mass sources.

Equations A2-A4 are referred to as being on variational form, but I do not see a variational form there. Also, should τ not be inside the integral as it is element dependent and therefore spatially variable? Most of the discussion in the appendixes is presumably 'common knowledge'. I would list these equations as a part of the whole system, but is there any reason to have an appendixes on Crank-Nicholson, Runge-Kutta and Backward Euler in a professional journal?

All in all, this is a good manuscript. I know that some of my above comments might

be a bit on the negative side, but then again reviewers are support to help improving things my pointing at things that can/should be improved.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-58>, 2020.

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