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The Cryosphere Discussions

Interactive comment on "Applicability of the Shallow Ice Approximation inferred from model inter-comparison using various glacier geometries" *by* M. Schäfer et al.

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The main topic of this paper is the comparison of lower order approximations to a full system model of ice flow. As such it is one in a long series of papers which finds that the shallow ice approximation (SIA) is not a good replacement for a full system model (FS) if the geometry is not shallow, i.e. violates the assumptions that go into the SIA. What seems to be novel is that these tests are conducted on various 3D geometries instead of the usual 2D cases. It is, however, hardly surprising that the disagreement in 3D is big, given that the same conclusion holds in 2D.

This publication needs a lot of attention to the important topics detailed below. My recommendation is to withdraw the publication and resubmit it once the discussion provides results that are important to a wider community.

General comments

Throughout the whole discussion it is implicitly assumed that the FS results represent the "truth" against which all other results are compared. However, the description of the finite element model (and the other models) is not complete so that it is hard to judge whether there could be big discretization errors in any of the models. Specifically there is no statement about the element types and approximation orders used in the FS model, and important details of the mesh, such as the number of elements in the vertical and their coarseness, are not given.

Model intercomparisons should always include statements about the verification of the individual models, i.e. their performance on geometries for which analytical solutions are known. One of the tests that should have been made (and mentioned) is the infinite inclined slab.

One feature that is repeatedly noticed and seems unusual is that the SIA velocities are higher than the full system ones over the whole model geometry [p572,l11; p573,l6]. Why this is so is not immediately obvious to the reader, and a closer investigation of the reasons would add to the usefulness of the discussion. One would expect that the SIA velocities do not agree with the FS if the ice thickness changes rapidly, or if gravity is not close to perpendicular to the surface (i.e. small surface and bed angles). Tests on very long, smooth glaciers, or infinite domains wold help.

Several times a "negative feedback" for the SIA is mentioned [abstract, p574,l16; p575,l18; p581,l10] without a good explanation of what this feedback consists of. The description on [p576,l2] is not very elucidating in that respect.

The discussion of the model intercomparison leaves out almost all interesting aspects. First, the CPU time [p578,l20ff] is not a good measure of performance of a code. Effects of memory access and – if the system does not fit into memory – swapping may completely dominate the solution time. Statements about "wall time" and the times used to assemble and solve the equation system (per iteration) would be better. In the comparison of solution times, results for the same approximation order of the equation system should be given (e.g. same degree of vertical discretization, if the FE elements have linear weighting functions).

The main advantage of the SIA is that it creates much sparser matrices with a very narrow band structure as compared to the HO and FS models. In an intercomparison paper the sizes of the system matrices (degrees of freedoms) and the width of the band structure resulting from the different methods should be stated.

The description of the solvers and preconditioners is sparse, even if they dominate the performance (and rate of convergence) of the codes. Most likely each code uses very different solvers and preconditioners which are tailored to the problem. These should be listed to allow a meaningful comparison of the relative performance of the different models.

The paper would benefit by streamlining the English by a native speaker.

Specific comments

p560, l1 - 7 Move this discussion to the introduction, or leave it away entirely, as it does not add to the topic of the paper. In this context also sea level rise has to mentioned nowadays.

p561,l10ff The distinct notation of diagonal and non-diagonal parts of the stress tensor with σ and τ is confusing: use one symbol (e.g. σ).

p563, l9-10 leave away this statement which is unnecessary for the topic of the paper

p566, l3 "artificial number" could be named finite viscosity (c.f. the discussion in e.g. Hutter (1983)) p566, l15 what is an “expression function”?

567, l13-15 This process is called a fixed point iteration.

p568, l21 Declare the function spaces of the weighting and test functions used.

p569, l4 Both the Stabilized Method and the Bubble Methods serve to alleviate the saddle point problem, and have no relation to the nonlinearity of the flow law which most probably was solved with a fixed point iteration.

p572, l18 (and also [p573, l18]) What is an “amplitude” in this context?

p574, l1-10 This discussion of sliding seems unnecessary. It is not clear how sliding contributes to a better agreement of different methods, if they don’t agree on the simplest cases. It sees that the main improvement is, that the relative errors get smaller, since the velocities are higher (by adding a constant basal velocity).

p574, l17 “extra deformation” should be “higher mass fluxes”. Higher deformation leads to higher velocity, not vice versa.

p575, l20ff This discussion is very confusing and should be substantially improved (see also the general comments on the feedback). Also a half-sphere glacier seems to be better suited for an intercomparison.

p577, l5ff These definitions seem awfully complicated for a model intercomparison test. Especially the statement on [p577, l22] is hard to understand in that respect.

p578, l3-5 What are you talking about here? Why is there no steady state for a valley glacier geometry? The argument in the last sentence is hard to understand.

p578, l16 The glacier does not re-advance due to high deformation rates, but due to mass fluxes that are higher than the steady state values.

p580, l15-20 The explanation what SIA models are should not be in the conclusions.

p580, l22 If the SIA is not useful for modeling these types of glaciers (which is in the assumptions about this theory, c.f. the discussion in Hutter (1983)), why is it done, and what can we learn from the intercomparison?

p580, l25 SIA will produce Forbes bands, simply because the shallower valley depths at the sides produce slower flow velocities.

p580, l27ff As discussed in the general comments, there should be a thorough investigation of this effect. Why is does the SIA model produce higher velocities than the other models. The local character cannot be the explanation, since longitudinal stresses (in the HO and FS models) would lead to lower viscosity, and therefore higher flow velocity.

Fig 1 This figure seems not necessary as is. At least one would expect to see some loops for the iterative solution of the nonlinearity, and for the time dependent adjustment of geometry.

Fig 2 This seems unnecessary as well. It is rather unusual to call the flow line direction y and the transverse direction x.

Fig 13 This figure is very hard to understand. Where is the feedback?

Technical corrections

p558, l18 “entirety”

p562, l5 leave away “complex”

p562, l22 “by Hutter”

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Interactive Comment

p564, eq12 typo in the derivative $\frac{\partial S}{\partial i}$

p565, l2 Replace “Most of the time” with “In many implementations”

Interactive comment on The Cryosphere Discuss., 2, 557, 2008.