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Interactive Comment

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

G. Leysinger Vieli (Referee)

g.j.m.c.leysinger-vieli@durham.ac.uk

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GENERAL COMMENTS:

The authors of this paper are comparing the performance of three numerical 3-D models of different complexity on various glacier geometries. The models used in this paper are the often used Shallow Ice Approximation (SIA), a higher-order model (HO) including longitudinal stresses and the full-system model (FS) solving the full Stokes equations. Inter-comparison of such models have been done previously in 2-D and to do the same in 3-D is important for assessing and improving glacier and ice sheet models. The authors also use different types of glacier geometries, which they produce synthetically, and bed/surface slopes. In a diagnostic set of experiments they compare the velocity fields obtained by the 3 numerical models and in a prognostic set of experiments they look at their final velocity field and surface geometry of the ice masses without and with mass-balance.

The diagnostic case with some arbitrary geometry, far away from any steady-state, can be undertaken but in my opinion makes no sense. What is to be expected from those experiments is that for different sets of equations (the 3 different models) different velocity fields will be obtained. Looking at the flux gradient would show why the velocities are so different for the different models. However modellers are interested in how good the models perform in reproducing glacier evolution and geometry and the diagnostic experiments don't tell us this. Furthermore, the geometries chosen in this paper are extreme and far from steady-state, so that the model comparison does not tell how good they are for glaciers. The whole section 5.1 and the figures 6-10a can therefore be omitted.

The prognostic case is set up in a better way, as they run the models for some time and for most mass-balance cases the steady-state is calculated. However I feel that the geometries are so extreme that for many cases the calculations might not be long enough and the reader may want to know how the glacier changes geometry with time for the different models (comparing the surface evolution would be interesting!). Especially in the valley glacier case where no steady-state was calculated and the calculations have been compared after the ice thickness at one specific location has reduced to 65 percent of the initial ice thickness. In the case where the glacier is very steep and thick this happens very quickly (1.4 years) and it is obvious that the glacier is far from its steady-state and will certainly adjust and change further. A better criterion would have been to look at which point in time the glaciers show hardly any deformation, and one would have known that the calculations are approaching steady-state. The observation of the author (p. 574, line 15) that the velocities of the different models approach each other with time indeed indicates that in a steady-state they may not be very different at all.

For the prognostic case with included mass-balance the authors chose 3 different mass-balance distributions for the 3 glacier geometries. For the conic and the valley glacier the chosen mass-balance shows a linear dependency to altitude. But as the initial surface altitude is used to calculate the mass-balance distribution, the mass-balance is constant with time at each grid-point (instead of dependent to altitude it is rather x,y-dependent). Constant is also the mass-balance for the flattened half-sphere. Adding a constant mass-balance is o.k. but if the results with the different models show that the geometries (surface elevation) are different (model dependent), as hinted on page 578, line 7, then a mass-balance-altitude feedback should be considered, where the mass-balance depends on the changing surface elevation at each grid. In such a case a realistic altitude mass-balance function should be used, as the uncertainty in mass-balance (and the uncertainty in the rate factor) is much larger, at least in 2-D, than the uncertainties introduced by the model assumptions (Leysinger Vieli and Gudmundsson, 2004). This is important for modelling real glaciers as even the best model doesn't improve the modelling if the mass-balance is not accurate.

This paper does not assess or compare the glacier geometry and length change with time (evolution) obtained with the different models. However, this is what in general glacier models are used for. Therefore we should judge the models on their performance to reproduce the observed glacier evolution/geometry and not on their simulation of a velocity field for a given (rather arbitrary) and fixed geometry. It is given that he surface geometry of a glacier is coupled/a result of the models mechanics (flow field). To me it seems that this paper has been set up to prove that the SIA is not valid and I think this is unfair towards the SIA, especially as modellers are interested to use a simple model if the case allows for it. Therefore this paper should in my opinion rather show how the 3-D glacier models compare for surface evolution and length changes for different slopes and different aspect ratios. For the mass-balance simulations 'realistic synthetic' glaciers should be used (or even 'real' glaciers with 'real' mass-balance). The

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paper doesn't need to prove anything but to show how the different models perform for the different (realistic) cases.

Conclusions - are in my view not conclusions, as the first few paragraphs are a repetition of common knowledge but not findings of the paper. This is misleading. It seems to me that a proper discussion of the modelling result is missing and that it lacks clear conclusion.

In my view this paper cannot be published in this form and would need improved model experiments and interpretation to be considered for publication. I encourage the authors to do this by e.g. building up their synthetic glaciers with a given mass-balance for the SIA case and calculate the steady-state. Once a steady-state from the SIA is obtained one can calculate the steady-state from this initial geometry for the other models. Then such a study as outlined in this paper can be undertaken.

SPECIFIC COMMENTS:

Some specific points arising from this paper which should be changed/considered in the new paper:

It should be mentioned in the title or at least in the abstract that the inter-comparison is for 3-D models as this is the new part of this paper. 2-D model inter-comparisons have been done before.

The quantifications in the abstract don't mean anything as they refer to an arbitrary time point and will be very different for steady-state (e.g. much smaller than so far observed; page 558, line 9/10).

The equations are very detailed; it is interesting and useful for modellers to see these details but most equations are not new and therefore can be put into an Appendix. (Equations in section 2, 3, 4 and 5).

I suggest that either sliding should be included with experiments or been left out entirely. Paragraph 5.1.4 doesn't add anything to the paper and could be omitted.

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Quantify the difference in observed velocities (e.g. page 573, line 15 - how many orders or percentage difference?) and in observed thickness (e.g. page 575, line 17) where the snout difference seems only to be of one grid-size for the SIA (see also comment to Figure 11).

Prognostic - Why not show and compare the evolution of the 3 different models for the different geometries, so that modellers learn about the conditions in which they can use a specific model. Show a figure with a map view of the glacier or even a 3D view of the tongue at different stages for the different models, instead of showing a 2-D figure.

Time unit - the time is used in years. Glaciers with different thickness and velocities need different time for their evolution; it might be more useful to use non-dimensional times. So the different glaciers are easier to compare.

Prognostic with mass-balance - there is only a sentence (page 578, line 7) saying: ’In all simulations a good agreement for the snout position was observed (...), but not necessarily for the entire glacier geometry’. Say what is different for the glacier geometry. This is important and if there is a difference then calculations with a mass-balance-altitude feedback needs to be included too.

The re-advancement of the SIA valley glacier is just due to readjustments of a non-steady-state geometry (page 578, line 15). What do you mean by sentence (line 16) ’In some situations the overestimation of deformation of the SIA can overcome the effect of a mass-balance field and completely change the behaviour.’?Won’t it retreat?

Comparison of CPU time - this might be very useful for modellers but one has to be clear. E.g. ’does not increase linearly’ (page 579, line 6) o.k. but how does it increase then? This section would need to be clearer. Say what the ’0.06s’ is (page 579, line 14) is it computing time? On line 22 is the computing time for one time step? Or for the whole calculations? Clarify!

Figure 1 can be left out as it doesn’t add much to the understanding.

Figure 3 - the slopes look very steep, mention that scaling is exaggerated or use same scaling for x and y-axis.

Figure 9 - How do you get the vertical velocity of the SIA?

Figure 11 seems to show some dependency on the grid size. This is especially seen for the thick lines (zero mass-balance), where the snout position seems to depend on the grid. The x-axis of Figure 11 has not been properly labelled (is it meters?). Is the upper boundary condition for a flattened half-sphere realistic? And why is the FS upper boundary approx. 1 grid size lower than the upper boundary of the other models?

Figure 12 - it seems that the calculations shown are only for the labelled slope (0.3, 0.4, etc), therefore one shouldn't plot lines but rather the measured points.

Figure 13 - how much is the absolute difference compared to the velocity of the FS model? The same for the absolute surface difference - needs to be compared to the surface of the FS. This graph relative to velocity/to the surface would be an interesting graph.

Figure 14 - are the 3 models compared at the same time or the same deformation (figure states time, text deformation (page 576, line 24))? I think it's rather unlikely that the 3 different models have the same deformation after the same elapsed time, especially for the steep (thick) glacier.

14a - what are peaks at margin of the FS model? Why?

14b - why wiggles towards margins of HO/FS? Do they get less with time? Are these due to instability (grid size)? - In the shown case it makes no sense at all as the deformations are still so very high. If one would get such wiggles one should try to explain them. In order to test the dependency on grid orientation, it would be interesting to see a model run with the grid axis not parallel to the main flow (do the marginal wiggles look the same?).

TECHNICAL COMMENTS:

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Page 569, line 6: replace ’numeric’ with ’numerics’.

Page 569, line 7: ’we refer the reader to’, instead of ’the reader can refer to’.

Page 559, line 2: replace ’such us’ with ’such as’.

Mass-balance equation has a typo in equation 46 (the ’square’ stands alone).

Page 578, line11: replace ’experiences’ with ’experiments’.

Page 579, line 2: replace ’have been chose’ with ’have been chosen’.

Figure 5: units missing.

Figure 6: units of x-axis missing.

Figure 8: units of x-axis missing.

Figure 9: units missing.

Figure 12: Y-axis units are missing.

Figure 14: X-axis units are missing.

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