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

M. Schäfer et al.

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Dear editor,

First of all we would like to apologize for the late response. During the finalizing of this letter we encountered an email problem - (no possible email contact between M. Schaefer in Canada and the two authors in France) which took several weeks to be identified.

Regarding the two reviewer comments and the short comment to our manuscript, we agree with most but not all of the objections. Our response is detailed below in this letter.

Overall, we think that the manuscript has been written at an early stage of the overall

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Interactive Discussion

Discussion Paper



Interactive
Comment

study. This first set of simulations gave some interesting, but not necessarily new, results and above all gave indications about what would be important to include in a second set of simulations. We are aware of this and all three comments point it out as well. This study was done within the framework of M. Schaefer's PhD and M. Schaefer is no longer at the LGGE having started another position on another subject. That is the reason why it is currently not possible to redo or define new simulations.

To answer your question about what we consider new and substantial, the following aspects are the most important ones of our manuscript:

We are using 3D experiments - in contrast to the 2D experiments of the many other published comparisons. It might be that 3D experiments lead to the same conclusions as the 2D comparisons. Nevertheless we think it is important to actually do 3D experiments - especially as a step in moving towards real case simulations.

Two of the three synthetic geometries we have chosen are close to real case geometries. Working with synthetic geometries has advantages: synthetic geometries are easier to implement, normally have better convergence behavior and allow modification of bedrock slopes and/or aspect ratios for comparison purposes.

We present a detailed comparison of the different configurations: simulations without and then with mass balance, diagnostic and prognostic runs, dependency on bedrock slope and aspect ratio, effects on velocities, but also effects on geometry,...

Fig. 13 and the corresponding discussion gives an insight into comparing not only final states but also the actual evolution in time. The Figure emphasizes the interest to do such a comparison in a latter step.

Our experiments have been chosen for the following reasons:

The spheric glacier has been used in a similar comparison paper (Le Meur et al., 2004). As this study uses two of our three models, it was a natural choice for us to include this geometry to compare our results with those of this publication.

The conic glacier is the geometry that was used in a project on the Cotopaxi glacier in Ecuador.

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The valley glacier geometry is defined as close as possible to a typical mountain glacier while keeping the advantages of a synthetic geometry, i.e. the possibility to change easily bedrock slope and aspect ratio.

Regarding the choice of the initial geometry for the transient simulation, there are two main alternatives:

- (1) one can start from the same geometry for all the models or
- (2) one can start from steady-state geometries obtained for the same given constant forcing, these steady state geometries being different for all models.

In the latter case, since we are not starting from the same configuration, the comparison of the transient phase from two steady states is more complicated. This is why we chose the former alternative even though there is no good choice of what should be the initial surface.

Why the initial surface from an SIA steady state should be more suitable for the other models than a completely arbitrary surface? We agree that in making this choice the initial geometry might be far from a steady state geometry. But it is the same for all the models.

Regarding the mass balance altitude feedback: we agree that it would have been better to include it even if, by doing so, it complicates a comparison based only on mechanical considerations (mass conservation and momentum conservation). Nevertheless, we don't think that our simulations are meaningless by keeping the mass balance constant in time (see also our answer to the first editor review). In 2D the mass balance prescribes completely the position of the snout for a given problem. This is not the case in 3D. For our geometries one can argue (as we are doing in the manuscript) that they are close to 2D geometries because of their symmetry. Therefore, it is to be expected that the snout positions will be in agreement, which is what we have observed. The final geometries are not, however, only defined by the snout positions and we observe important differences in the surface geometry over the whole surface in both the 2D and the 3D cases.

TCD

2, S436–S444, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



In what follows, we would like to reply point-by-point to the different objections made, first to what are in our eyes fundamental points and then to minor points. We have not touched on the purely technical corrections at this stage.

Fundamental points:

- *Why we did not use a real case glacier (comment by Oerlemans)*

We have indeed done a comparison on a real case glacier (St. Sorlin glacier in France). We chose not to publish these results as there is an important disagreement between our simulations and the significant data available from fieldwork - even with a Full Stokes model. We have to emphasize that the data available for the St. Sorlin glacier is a higher resolution data set than typically available for similar glaciers. We hypothesize that the origin of this disagreement is in the sliding part of the model. The sliding law is not yet correctly understood and thus is not correctly taken into account in the simulation. We suspect that a space and time dependent sliding law would be necessary. We think that at this point in time the glacier of St. Sorlin is probably not a suitable glacier for such a comparison regarding this sliding problem.

Coming back to the question of why we did not include a real case glacier in this paper, our simulations showed us that in real case glacier simulations there are probably currently too many poorly understood aspects. This is true for the St. Sorlin glacier that we modeled. Knowing that even the best available model disagrees with data, we don't think that it is currently meaningful to conduct an inter-comparison experiment on a real case like the St. Sorlin glacier.

- *Climate-glacier issue (mentioned by Oerlemans)*

Certainly the climate-glacier issue is much more important than simply understanding the deformation part of the evolution of a glacier. Even if there are other aspects, possible more significant ones, we still need to understand this small part of the evolution of a glacier. The purpose of our manuscript was not to ad-

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2, S436–S444, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



dress the entire climate-glacier issue but only to study the deformation part and to compare it, for a given mass balance distribution, to the mass-balance part.

- *Simulation time (comment by Leysinger Vieli)*

Leysinger Vieli argues that the simulation times have been too short, especially in the case of the valley glacier. We are in agreement, but the simulations would have been too long. It was not possible to make them longer for practical reasons. And doing full-Stokes or higher-order modelling on 3D geometry is still a challenging issue today. Given the time constraints, we could not define a smaller geometry representing the same type glacier and keep all important features. The 65% of thickness criterion is only a compromise.

In some cases, the spheric glacier with zero mass balance is an example, there is no meaningful steady state surface. Thus we had to stop the simulation after a chosen simulation duration. In all possible - physically and technically - cases we chose steady state simulations.

- *Time evolution (comment by Leysinger Vieli)*

It would have indeed been interesting to compare the different models during the whole simulation time and not only the final states. We suggest it for the next stage of work. Nevertheless Fig. 13 and the discussion about the negative feedback gives an initial insight in this question.

- *Use of the Full Stokes model as truth and presentation of models (comment by Lüthi)*

We do not see what else besides an already validated Full Stokes model we could take as "truth" in such a comparison. We also disagree on the different suggestions as to our model presentation (complete model description including element types in the FS case, approximation orders, size of matrices, description of solvers and preconditioners and performance on geometries with analytical solutions for all models). These points have already been presented explicitly

TCD

2, S436–S444, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



in other papers and our preference is to keep the manuscript concise and refer the reader to the different model description papers. We also think that it is not necessary to validate and re-present simulations where analytical solutions exist in each inter-comparison study. Once a model has been validated it is, in our eyes, not necessary to validate it each time it is applied.

- *SIA velocities are observed to be higher than FS velocities (comment by Lüthi)*
For a diagnostic simulation, the fact that the SIA overestimates the local velocity is a 'classical' result as shown by the ISMIP-HOM inter-comparison for example. The case is more complex for a transient simulation because of the concurrence between higher velocity and thinner ice-thickness. This concurrence leads to smaller differences between SIA and FS velocities.
- *CPU time (comment by Lüthi)*

Our CPU time study could have certainly been done in a more detailed or different way, as stated in the manuscript we only wanted to give a rough estimation. We don't think that "*CPU time is not a good measure of the performance of a code*". Especially in real case simulations the CPU time is often a limiting factor. We are talking about a factor of 10000, several orders of magnitude, and not only a difference within the same order of magnitude between the different models.

Minor points:

- We agree that the manuscript could be improved by rewriting parts of the abstract, introduction, conclusions and by putting some of the equations in an appendix as proposed by the reviewers. This includes other paragraphs such as the discussion about the negative feedback and the CPU time. Regarding the presentation of our results, we could indeed give more quantified and detailed results and include some other figures. Some 3D figures could be included. This was not done mainly to keep the paper short and concise.

TCD

2, S436–S444, 2008

Interactive
Comment

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Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

- *Sliding*

We agree to omit the paragraph about sliding as sliding is not yet very well modeled. We note that this paragraph was added in response to a question/suggestion in the first editor review. Overall we were mostly interested in the deformation part, the only part among deformation, sliding and mass balance which is fundamentally different from one model to another.

- *Longitudinal Coupling (comment from Oerlemans)*

We agree that what we wrote is only correct for a diagnostic simulation. For a transient simulation, the mass conservation induces non-local effects. What we call the negative feedback is the fact that the high overestimation of the velocity given by the SIA for diagnostic comparison is reduced in the case of transient simulation because the SIA leads to a lower elevation of the upper free surface and smaller slopes of this surface.

- *Horizontal shear stresses (comment from Oerlemans)*

There is a misunderstanding on this point because we are not discussing how horizontal shear stress can be introduced by a shape factor from the theory of Nye in a 2D flow line model. We are discussing the fact that in a 3D model the SIA neglects the horizontal shear stress. This can explain part of the difference obtained using different models. We agree that the use of a shape factor is certainly a good compromise when using a 2D flow line model, even if it might not be well-adapted for every case of bed section geometries.

- *p578 Line 16 (comment from Leysinger Vieli)*

"In some situations the overestimation of deformation of the SIA can overcome the effect of a mass-balance field and completely change the behavior. Won't it retreat?"

It will retreat. Our point is only that it first advances and then retreats, in contrast to the other models where there is no initial advancing period.

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Interactive Discussion

Discussion Paper



Interactive
Comment

- *Fig. 9 (comment from LeysingerVieli)*

The vertical velocities in the SIA model are calculated by analytical derivation and integration of the continuity equation and the horizontal velocities.

- *Definition of the valley glacier geometry (comment from Lüthi)*

We do not feel that the chosen geometry is that complicated. The formulas presented are a superposition of a linear slope and two parabolic functions. They represent a valley glacier emerging from a cirque.

- *p578 l3-5 (Lüthi)*

"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."

There is a steady state for a valley glacier geometry. But a steady state was not achieved in our simulations as the simulation time would have been too long. We chose the same simulation times as in our simulation without mass balance. The motivation for this choice is that the contribution of deformation to the glacier change should be equal in all models in this case.

- *comment on p580 l22 (Lüthi)*

"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 inter-comparison?"

The SIA is used because this model is fast and easy to implement. From the inter-comparison we learn in which cases we can use the SIA without too much error. We also show that the SIA is often not that bad even for geometries with higher aspect ratios - especially in prognostic simulations and if we are interested only in the geometry.

Best regards,

Martina Schäfer (for the authors)

S443

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