Interactive comment on
“Numerical simulations of
glacier evolution performed using flow-line models
of varying complexity”
by
Antonija Rimac et al.

General comments :

My main concern with this paper comes from the fact that we do not really see what is the message the authors want to pass. The paper simply compares results of 2 models of varying complexities under various contexts with regard to forcing scenarios and glacier/bed geometries, but no real conclusions are drawn as to which model to use under which circumstances. This is actually what the modeling comunity expects to learn from such a paper.

In that respect, the first phrase of the abstract should read 'Results of two...' instead of 'The performance of two..' because no discussion in terms of performance is actually proposed. Doing so would require comparing results in terms of computer efficiency (CPU), easiness of coding and/or validity of the results. I admit this last aspect is difficult to properly assess without studying real cases, still, glacier behaviours in response to climatic scenarios have sufficiently been investigated to have an idea on what is realstic or not (for instance the response lag as a function of the size of the glacier, the bed slope....). Moreover, given the degree of sophistication and the proven robustness of the Elmer model as correctly mentionned in the manuscript (reference to Pattn et al., 2008) it seems to me acceptable to consider the FS model as a realistic reference to which SIA results should be compared and to consequently derive a criterion for the correctness of the SIA simulations. These aspects have to be (at least partially) addressed for the paper to gain in interest and in my view to deserve publication.

I also agrree with referee #1 on the fact that the SIA approach relies on an asymptotic theory based on the aspect ratio.... proper reference should be made to this theory which dates back to quite some time ago (eg, Hutter, 1983, Hindmarsh, 2004) and was considered again later within the isotropic/anisotropic duality and for newtonian rheologies (Mangeney and Califano, 1998) from which interesting results can also be exploited.

In any case, results should be discussed in terms of the aspect ratio (eg. Pattyn et al., 2008) bacause the validity of the SIA asymptotic theories (be it of the zeroth or second order) essentially relies on the value for this term which results mainly from the actual glacier geometry (typical thickness to length ratio, slope of the bedrock) and to a lesser degree from the intensity of the basal drag. This is the main reason why SIA has been abundantly used for modelilng grounded ice sheets for which this aspect ratio is generally small enough. One big issue for mountain glaciers modeling is whether or not the SIA is still applicable, and if so, under which conditions with regards to the glacier settings as given by its aspect ratio.

Moreover, as can be seen from the comments below, the paper as it is written suffers too many inconsistencies and errors which gives the unpleasant feeling of a botched job not properly checked before submission.

More specific comments follow :

- page3 line 8 : '..which depends on the local surface
slope and a ice thickness..' -> supress the 'a'

- page 5 line 16 : '...under a small period random forcing and under..' -> '..period of ramdom forcing ..' ?

- page 6 line 10 : 'After the equilibrium states of three…', 'equilibrium state' is not the proper term.. we are here talking about steady state of glacier geometries which is not a true equilibrium state -> please change

- Page 6 line 16 'For better clarity, the glacier length is shifted by ±1 km along the y-axis for the large and small glacier'. I am sorry but if the simulations start with zero volumes (assuming zero initial lengths) the shifts as represented on figure 1b is not correct.. I rather see a 2-km shift

- Results of the time dependent behaviour following the steadystate simulations (page 7 and figs 1 and 2) :
When results of fig 2 are commented, it is said : '...we can see that the rate of advance and retreat differs in our models with SIA producing shorter advance and retreat compared to FSM..' ... Honestly, when looking at the figure, I do not see any significant difference .. therefore the following conclusion does not hold anymore. This has to be changed.

- Eq. 11 : The formulation is not clear .. is it meant that TauV is the time after which the volume V reaches the proposed value after the equal sign ? If so , please state it differentlt, as it is written it is not a proper equation and it is anyhow confusing.

- page 7 line 18 : ' Unlike in Leysinger-Vieli and Gudmundsson (2004), whose full-Stokes model did not show consistent lag or lead in comparison to their SIA model, here, the response time in FSM is longer than the response time in SIA for all glaciers.' Why is there no attempt to discuss this contradiction with previous results ? This illustrates my more general comments according to which there is an obvious lack of discussion of the proposed results. This way of achieving the paragraph is abrupt and frustrating for the reader.

- page 7 line 29 'The opposite is true for glacier volume that reacts faster in FSM compared to SIA. Also, as already seen, it is notable that glacier volume and length amplitudes are not equal in the two models.' .. again, I find these conclusions extremely drastic when actually looking at the correponding figures. I am sorry to say that the lag between SIA and FSM is almost insignificant on figure 3a. (it is confirmed by phase differences of 53.1 and 56.5 degrees for FSM and SIA in table 3). As for the amplitudes , especially for volumes, they are almost the same ... I see a max difference of 0.2 km3 for volumes of around 10 km3....it makes a 2 % difference at the most ! not enough to assert that they are different in my view.

- Page 8 line 3 : ' Also, the phase lag increases with increasing frequency of the climatic forcing, possibly as a result of grid discretization since when a grid resolution is increased from 100 m to 20 m the phase lag decreases for almost 50%'

This is not a satisfactory explanation... if the authors suspect a model artefact, they should then try to solve for it. In the present case, before drawing any dubious conclusions, a sensitivity test on the grid size should be carried out until a convergence in the obtained phase lag is obtained. This is compulsory for the modeled time lag to become reliable and suitable for any form of interpretation. Either these simulations on the time lag as presented here should be removed or new simulations with an optimized grid size are proposed instead.

- page 8 line 11 : 'First, a uniform noise with a standard deviation of 100 m is superposed on E. Second, a forcing function derived form the Central European mean annual temperature record

(Luterbacher et al., 2004) is used' . It is absolutely necessary to show these two forcing curves especially when later the response of the glacier to the varibility in these fields is commented.

- page 8 line 19 'This is not the case for glacier volume evolution, where SIA lags FSM (see e.g., years 760-840 on Fig. 4d).' Sorry again... not observable from the figure ..moreover fig 4d does not represent glacier volume evolution ... has the paper been checked before submission ?

- page 8 line 19 : Additionally, it is notable that glacier length evolution in
FSM does not respond to some smaller short-term variability (e.g., year 800-900 and 1100-1200 on Fig. 4b)' ... what is the short term variability .. that of the forcing ? we have no means to assess it.

- page 8 line 20 : ' Figures 4c and 4d indicate a periodic reaction of glacier length and volume to year-to-year variability in the temperature record. The power spectrum (not shown) also shows high energy at a frequency of 250 years that seems to reflect on the glacier evolution.' This is of no use if we do not see how the temperature record changes through time.

Page 9 line 1 : 'In Sect. 4.1 the glacier width W is kept uniform and it can be excluded from the equation', I do not see how the fact that W is uniform implies giving it up in Eq. 12

page 9 line 4 : Paragraph title says 'Constant bed slope and exponential change in glacier bed width' whereas the corresponding figure 5 talks about a bed with a uniform width…… → please check on that

- page 10 line 17 : 'Here, and in the following sections, the force balance components shown in Fig. 6d include the varying bed width. Applying this varying width leads to increase in the normal stress (compression) where the width decreases, and to decrease in normal stress (extension) where the width increases.' : This attempt to explain results of fig. 6d is a bit misleading. Indeed, warying widths imply changes in the normal stress , but the quantity as represented in the figure is the stress gradient which is not the same. This is confirmed by the fact that on the figure we do not see any obvious link between the stress gradient and the glacier width. The interpretation (if there is one of interest) should then be reconsidered or removed as it is confusing in its present state.

- Figure 7 : There is an inconsistency between the caption and the figure. (b) is the vertically averaged horizontal velocity and (c) is the basal shear stress.

- Last paragraph section 4.2 : The proposed interpretation of figure 7 is by far too vague and approximative :

-'At the length of about 3000 m, or just upstream of the bed bump...', The distance of 3000 m matches the bump top not its upsream part -> please be more precise

-'Above and just downstream of the bump, the driving stress reaches its maximum due
to the large surface slope' .. when mentioning 'above and just downstream of the bump', are the authors trying to explain the double peak in the basal shear stress ? There is obviously a singularity here in the SIA derived basal stress which can not be solely explained by the surface slope as stated. If it were the case, the surface slope should undergo a similar double peak and the local minimum in basal stress should not correspond to approximatively the maximum in surface slope as on the

figure. One possible explanation could reside in the double dependency of the SIA expression of the driving stress (proportional to both the ice thickness and the surface slope), although I am a bit dubious on that in the present case owing to the strong singularity in the shear stress. I would rather suspect a deficiency in the SIA representation in this specific context.

This brings me to a more general comment :

We are probably here addressing the most interesting part of the paper, namely the local degradation of the aspect ratio due to a singularity in the basal topography. This aspect has already been abundantly addressed in the literature and authors should reconsider their interpretation by referring to appropriate papers. Above all, authors should interpret their results in terms of the aspect ratio (as also suggested by reviewer 1). Here we obviously face a situation where locally fast varying slopes degrade the aspect ratio making SIA representations strongly deviate from reality. This is typically the kind of situation where FSM approaches should make up for the SIA deficiencies. This aspect is fundamental but is obviously missing here and a significant rewriting of the paper should then be considered.

- Fig 8 : again a confusion similar to that of fig 7 in the caption. Making copy-paste captions from one figure to the other can be a good way of saving time provided a minimum check is done.

- Ice fall result interpretation : Here also, although maybe to a lesser degree, the bedrock perturbation leads to changes in the SIA and FSM representations which should be interpreted similarly as suggested above.

- page 12 line 24 : 'For a constant bed slope and a uniform glacier width, the sliding velocity is higher in SIA compared to FSM in the area of high ablation. This supports our conclusion that in SIA glaciers flow faster to the area of higher ablation, which in turn makes the volume response in SIA slower compared to FSM'
I do not understand this statement... if with the SIA glaciers flow faster to areas of higher ablation, it induces a higher turn over which in my view (unless I am wrong) should lead to faster response times at least when the equilibrium altitude increases. The simulation is for a constant bed slope and a uniform glacier width, corresponding to figure 2 and Table 2 where inferred volume response times are smaller with SIA compared to FSM .... how can authors talk about 'volume response in SIA slower compared to FSM' then ?

- Page 12 line 30 : 'This does not necessarily mean that the highest surface velocity simulated using SIA of about 250 m year −1 is overestimated. Wangensteen et al. (2006) showed that the highest surface velocity measured on Nigardsbreen (Norway) can reach 489 m year −1 at the main ice-fall, which is almost twice as high than the highest velocity obtained in this study'
This comparison seems to me irrelevant as an attempt to explain the high velocities of the SIA model. Comparing a real case glacier with a theoretical glacier as proposed in this study does not make sense as the settings differ notably. These high velocities should rather be interpreted as the deficiency of the SIA approach in the present case. Many previous studies have shown that for mountain glaciers, if SIA representations remain usually quite realistic for volume changes, on the other hand they often yield irrealistic representations of the velocity field as correctly stated page 2 line 16.

References

Hutter, K.: Theoretical Glaciology: material science of ice and the mechanics of glaciers 30 and ice sheets., D. Reidel Publishing Company, Terra Scientific Publishing Company (ISBN90277 1473 8), 1983.

Mangeney, A. and Califano, F (1998) The shallow ice approximation for anistropic ice: formulation and limits, J. Geophys. Res. 103(B1) 691-705

Hindmarsh, R.C. A. 2004 : A numerical comparison of approximations to the Stokes equations used in ice sheet and glacier modeling, JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 109, F01012, doi:10.1029/2003JF000065, 2004