

In “Numerical simulations of Gurenhekou Glacier on the Tibetan Plateau using a full-Stokes ice dynamical model” Zhao et al. use an ice dynamical model to simulate the evolution of a small glacier on the Tibetan Plateau. The model is thermomechanically-coupled, solves the Stokes equations, and is forced by parametrized temperature and climatic mass balance. Prognostic simulations are carried out over the next 50 years for two warming scenarios. The study concludes that Gurenhekou Glacier will probably disappear by 2100, and a similar fate is proposed for 95 % of all glaciers in the area.

The manuscript is well written and organized, reads fluently, and is within the scope of The Cryosphere. The ice flow model used here is very well established, open-source, and some of the co-authors have an excellent track record in glacier modeling. Thus I have a lot of confidence in all numerical modeling aspects of the manuscript.

I am concerned that the study is imbalanced. The numerical setup is very sophisticated. Prognostic simulations of a small valley glacier are performed with a state-of-the-art Stokes model, following the spirit of, e. g., Juvet et al. (2009); Zwinger and Moore (2009); Huss et al. (2010). This is in stark contrast to the treatment of the boundary forcing, namely the climatic mass balance. The climatic mass balance is parametrized as a function of elevation and equilibrium line altitude obtained with an energy balance model of a glacier 25 km away. Some readers may not be familiar with the local climate, and its variability. Why is Gurenhekou glacier expected to exhibit a climatic mass balance distribution similar to Xibu glacier? As a test the parametrization is compared to observed surface elevation changes and stake measurements. From Figure 3 one expects a large uncertainty in the climatic mass balance, but this uncertainty, and potential consequences on projected glacier retreat, are not addressed in this paper. Surface elevation changes are the result of climatic mass balance and flux divergence. Given relatively small ice thicknesses and surface speeds, it is conceivable that the uncertainty in climatic mass balance is larger than the flux divergence term. The authors already give us a hint: “This is because the volume loss mostly depends on the surface mass balance” (p. 158, l. 7–8). Also the sentence “... test this hypothesis using the observed change of the glacier surface elevation and the stake mass balance measurement” (p. 150, l. 8–10) implicitly assumes the flux divergence to be small, right? So, is the glacier just down-wasting, and ice dynamics plays an insignificant role?

To make this clear, I am not questioning the validity of the parametrization itself. But, if possible the uncertainty in climatic mass balance should be quantified. Admittedly this may be difficult. Yet, at the very least, a discussion of how uncertainties translate into uncertainties in projections of glacier mass (volume) changes is needed. Reporting projections of volume change rates with two significant digits but no error bars seems hardly justifiable. That is, results should be discussed in the context of uncertainties associated with climatic mass balance.

Of course, nothing is wrong with cracking a nut with a sledgehammer, but I am under the impression that, with some additional efforts, more substantial conclusions could be reached. If I may offer a suggestion: I would test the hypothesis that a simple melt model is sufficient to project changes in the evolution of Gurenhekou over the next

50 years within the uncertainties arising from the climatic mass balance forcing. Such an exercise seems easy, as all necessary ingredients are already available, including the numerical simulations. Possible rejection of the hypothesis would be a strong argument why a sophisticated ice flow model is needed. On the other hand, if the hypothesis is accepted, then this study may serve as a kind of benchmark for small glaciers on the Tibetan Plateau, paving the way to an assessment of regional glacier change.

Is quite possible I am missing something. If so, please help me to see what I am missing. In summary, the study may be publishable if the above concerns can be addressed satisfactorily.

A word on naming convention: In my review I use “climatic mass balance” instead of “surface mass balance”, meaning the sum of the surface and internal mass balances as defined in (Cogley et al., 2011). Because firn processes are not considered in this study, I think it is the climatic mass balance that appears in the surface kinematical equation.

General comments

- “full Stokes”: in the recent glacier and ice sheet modeling literature, both terms, “full Stokes” and Stokes models, are used to describe models solving the Stokes equations. While “full Stokes” is often used, it remains to sound very odd to me. Doesn’t it imply that there is something like “half Stokes”? Of course the meaning of “full Stokes” is clear to the reader: The model does not solve an approximation to the Stokes equations. I acknowledge that “full Stokes” vs. Stokes is a matter of personal taste, but “full Stokes” is not used in computational fluid dynamics outside of glaciology.

Specific comments

p. 146, l. 6; p. 147, l. 29; and p. 155, l. 7 “dynamical evolution”. Please remove “dynamical”. “Dynamic evolution” refers to changes in ice dynamics (i.e. flow patterns, distribution of stresses, etc.). From the context I infer that you mean the glacier’s response to environmental forcing, hence “The evolution of the glacier...” seems both sufficient and more adequate.

p. 146, l. 16 It might be an oversight on my side, but I cannot find statement or reference within the main text that supports “Although Tibetan glaciers are not particularly sensitive to climate warming”

p. 147, l. 14–21 In a flow-line study comparing the Stokes stress balance and the Shallow Ice Approximation, Leysinger Vieli and Gudmundsson (2004) conclude that length and ice thickness changes are well reproduced with the Shallow Ice Approximation, at least in the case of insignificant basal sliding. Maybe the above reference could be added here.

p. 149, l. 4 What are the errors in ice thickness estimates along the radar lines, and how does the sparsity of radar measurements and gridding-related errors translate into uncertainties in the DEM (i.e. ice volume). It is conceivable that the sensitivity of the prognostic simulations to uncertainties in initial ice volume estimates is small compared to the uncertainties in the climatic mass balance. If so, this might be worth a statement.

p. 153, l. 12 changes with time...

p. 153, l. 15–16 This is a repetition of p. 147, l. 22 and p. 152, l. 3. I suggest to remove it, and add the website of ELMER to p. 147, l. 22.

p. 153, l. 19–20 I am not sure I understand what is meant with "..., which then defines a mass balance pattern that is constant over time". Could you clarify? It defines the surface mass balance that balances, both locally and globally, the flux divergence such that the surface elevation change is zero everywhere, or something like that, right?

p. 155, l. 12–13 remove "every year" unless it implies a yearly time step in the surface kinematical equation. If so, please clarify.

p. 159, l. 3–26 This is an interesting discussion.

References

- Cogley, J. C., L. A. Rasmussen, A. Arendt, A. Bauder, R. J. Braithwaite, P. Jansson, G. Kaser, M. Möller, M. Nicholson, and M. Zemp (2011). Glossary of Glacier Mass Balance and Related Terms. Technical report, UNESCO-IHP, Paris.
- Huss, M., G. Jouviet, D. Farinotti, and A. Bauder (2010). Future high-mountain hydrology: a new parameterization of glacier retreat. *Hydrology and Earth System Sciences*, **14**(5), 815–829. doi:10.5194/hess-14-815-2010. URL <http://www.hydrol-earth-syst-sci.net/14/815/2010/>.

Jouvet, G., M. Huss, H. Blatter, M. Picasso, and J. Rappaz (2009). Numerical simulation of Rhonegletscher from 1874 to 2100. *Journal of Computational Physics*, **228**(17), 6426–6439. doi:10.1016/j.jcp.2009.05.033. URL <http://linkinghub.elsevier.com/retrieve/pii/S002199910900285X>.

Leysinger Vieli, G. J.-M. C. and G. H. Gudmundsson (2004). On estimating length fluctuations of glaciers caused by changes in climatic forcing. *J. Geophys. Res.*, **109**(F1), 1–14. doi:10.1029/2003JF000027. URL <http://www.agu.org/pubs/crossref/2004/2003JF000027.shtml>.

Zwinger, T. and J. C. Moore (2009). The Cryosphere Diagnostic and prognostic simulations with a full Stokes model accounting for superimposed ice of Midtre Lovénbreen, Svalbard. *The Cryosphere*, **1967**, 217–229.