Second round of review of 'Possible biases in scaling-based estimates of mountain-glacier contribution to the sea level' by Banerjee et al.

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Banerjee and colleagues have put a substantial effort in updating their manuscript in order to answer the issues raised by both reviewers. Through this, they have addressed some issues raised (e.g. more clarity on mass conservation, some unclear statements were removed and clarified). However, some important foundations of the story are still problematic.

- Scaling methods are losing significance now that methods arise in which the glacier geometry is explicitly accounted for. The authors state that they are not aware of any studies in which sea-level contribution is calculated based on flow models. OGGM was applied globally (Maussion et al., 2019), and was used to project the future contribution to sea level from all glaciers (Marzeion et al., 2020).
- The argument that an idealized setup is not useful to compare various methods does not hold in my opinion. To make claims about the suitability of scaling-based methods for sea level contributions based on Himalayan glaciers (or any mountain glaciers) does not really make sense. When it comes to sea-level contribution studies, one should focus on ice caps and big Arctic glaciers (i.e. not mountain glaciers), which contain the almost entirety of the worldwide glacier volume (see e.g. Table 1 in Farinotti et al., 2019). i.e. you investigate the effect of scaling-based methods on the future evolution of mountain glaciers: 'sea level' is out of the context here.
- In their answers Banerjee and colleagues suggest that there is a clear evidence of underestimation of glaciers relying on V-A scaling arguments in GlacierMIP (Hock et al., 2019). This is not very clear, and moreover, the problem with GlacierMIP is that the setup was very different for the various models (i.e. it is difficult to compare a V-A scaling and another type of model if the forcing is totally different). The good news is that this has been partly been solved in the second phase of GlacierMIP (Marzeion et al., 2020). Another advantage of this new study is that there are more models to compare and that the comparisons can also be made at the regional level. All the data is freely available. The authors would have to look into this, but I'm afraid that also here there is no clear sign of V-A scaling based methods vs. others.
- Some parts remain vague. It is for instance unclear how your algorithm failed for many glaciers, and now that you have performed an 'update' of your algorithm this is solved…
- It remains problematic to see that the model was not calibrated for individual glaciers. The argument 'to avoid the associated computational cost' is not a very solid one… Such models are computationally cheap to run and given the relatively limited sample of glaciers considered (within the framework of regional- to global studies), this should not be a problem. By having a realistic geometry, the velocities will automatically also be relatively close to the observations (and the argument 'Tuning the rate factor to fit the thickness may not be a good idea, as it may lead to unrealistically small glacier velocities, and thus, unrealistic response properties' does therefore not hold).
- The conclusions are in the end still based on a comparison of the evolution between steady states (which boils down to comparing steady states). The lack of real transient analyses makes it difficult to support any claims related to validity of transient models (which are used for sea level rise studies).

In conclusion, I am still not convinced by the statement that V-A scaling methods are likely to underestimate the future sea level contribution from glaciers. And even with a good modelling setup and clear presentation of your results, I do not think that any conclusions on sea level contribution validity of different models can be obtained from a study on Himalayan glaciers (or any other mountain glaciers, given the limited total volume of ice stored in these ice bodies).

References

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