Review of ‘Possible biases in scaling-based estimates of mountain-glacier contribution to the sea level’ by Banerjee et al.

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In this manuscript, Banerjee and colleagues simulate the evolution of 551 glaciers from the Ganga basin (Himalaya). They start from glaciers in steady state resembling present-day glaciers and force them with a stepwise change in equilibrium line altitude until they evolve into a new steady state over a 500-yr time period. These simulations are performed with a V-A scaling method, with a linear response model and with a 2-D flow model based on the shallow ice approximation (SIA). The authors find that the V-A scaling method, when subject to a time-invariant scaling, underestimates the modelled future loss compared to the simulations performed with the SIA model. From this finding, they suggest that relying on V-A scaling is problematic for studies focusing on the future sea-level contribution from glaciers as this contribution is thus likely to be underestimated.

Some of the ideas put forward in this manuscript are interesting, but the manuscript has several problems (some of which are major in my opinion). My main concerns relate to:

[1] **State-of-the art**. The authors compare the outcome of V-A scaling with results from a SIA model. V-A has indeed been used in some important regional- to global studies in the past (e.g. Marzeion et al., 2012; Radić et al., 2014) due its computational efficiency. Moreover, with spatial estimates of ice thickness lacking for individual glaciers at the time, V-A methods offered a good alternative to estimate the volume of a glacier (and its changes through time). However, increasing computational performance and new glacier-specific inventories on e.g. ice thickness (Huss & Farinotti, 2012; Farinotti et al., 2019) and mass balance (e.g. Brun et al., 2017; Braun et al., 2019; Dussaillant et al., 2019; Zemp et al., 2019), now allow for far more sophisticated methods to simulate the dynamic evolution of glaciers. This includes methods based on imposing observed geometry changes in which the glacier geometry is explicitly accounted for (e.g. Huss & Hock, 2015; Rounce et al., 2020a, 2020b) and more recently also flowline models in which glacier dynamics (i.e. mass transfer within a glacier) are included when projecting glacier changes at regional to global scales (Maussion et al., 2019; Zekollari et al., 2019). When reading this manuscript, it seems like V-A scaling is a state-of-the art approach, and that you compare it to something more sophisticated (2-D SIA model). This comparison would have been very relevant a few years ago, when V-A scaling was state-of-the art (I am for instance thinking about the excellent work realized by Surendra Adhikari during his PhD; see e.g. Adhikari & Marshall, 2012), but has, in my opinion, lost some of its interest by now. With the new glacier-specific ice thickness estimates and other information derived from remote sensing becoming widely available (outlines, surface topography, ice thickness derived from this), the importance of V-A scaling methods is now strongly reducing and is likely to continue doing so so in the near future (see e.g. discussion by Haeberli, 2016). I do therefore have some reservations whether the ‘The Cryosphere’ is the ideal medium to share these (somewhat outdated?) findings. This concern is furthermore strengthened by my doubts about the experimental setup and the validity of your main conclusions as elaborated in the following points.

[2] **The experimental setup**:

a. Comparing different methods and models is always quite complicated. This is especially the case when considering ‘real’ cases (glaciers with real geometries in your case). A study such as the one presented here would have greatly benefited from an idealized setup, which would have made comparisons more straightforward and allowed to disentangle differences between simulations obtained from V-A scaling and those relying on 2-D SIA modelling: see e.g. Leysinger Vieli and Gudmundsson (2004) and Adhikari and Marshall (2012). Here a ‘selection’ of glaciers is considered, due to some 'problems' occurring when considering all glaciers in the region (see point 2b), which makes it even questionable how representative these are for this given region. With idealized glacier geometries, you could have explored the effect of glacier size, surface slope,... on the discrepancies between V-A based results and SIA modelled results more carefully.

b. Several arbitrary steps and decisions are made in the manuscript. A few examples of decisions that are hard to understand / seem not well funded:
   - l. 181-182: you exclude glaciers with a large change in area over the 500-year time period? Why? This seems arbitrary, but you must have a reason for this. Moreover, how this this influence your results? This makes the sample less representative...
   - l. 182-183: why do you exclude glaciers with long response times? Again, this makes your sample less representative (you probably exclude a certain type of glaciers, likely those that are gently sloping: see e.g. Haeberli & Hoelzle, 1995). Is this because these glaciers are not in steady state after 500 years? If so, you should simply run your experiments for longer and not exclude these glaciers.
   - Figure 1: you show '200 randomly chosen glaciers': why? Should show them all!
   - l.249-250: 'In fig. 2b, about 30 data points,...were not included in the fit': why? You mention something about possibly creating a bias in the linear fit in the next sentence, but I do not see where this would result from / what the problem could be.

c. The Setup of your SIA model is not fully clear.
   - You mention that for > 100 cases 'our algorithm for finding a steady-state similar to present extent did not converge or the final steady state glacier geometry was not realistic': how is this possible? How can a simple SIA solution not 'converge' to steady state (in fact, even analytical solutions may exist that do not even require running the SIA model to find the steady state: see e.g. Jouvet & Bueler, 2012)? And what do you consider 'not being realistic'? Which boundary conditions did you use to ensure mass conservation (e.g. to ensure specific ice-free regions do not become ice-covered)? You mention that mass conservation was monitored (l. 162-163): but how do you do this (this is not so straightforward to do...)? Did you check that the integrated SMB over your glacier is zero for the steady states (which it should be)? Would also be good if you could consider some benchmark experiments (e.g. Jarosch et al., 2013) to make sure your model is mass conserving.
   - Why do you randomly pick the values for the rate factor in Glen's flow law (not 'Glenn' + add a reference to the original studies, e.g. Glen, 1955)? The value of the rate factor will have a large influence on the local ice thickness and on thus the glacier volume. By picking this randomly: could be 'off' quite a lot from the 'reference/observed' volume of the glacier. Why do you not match this to the reference volume from every glacier that you have from Kraaijenbrink et al. (2017)? Is this also not problematic when working with single values for c and γ later in your analyses for all glaciers (e.g. for the best fits): you make some glaciers too thin and some too thick.

d. Lack of in-depth analyses. Often you seem to be perplexed by some findings yourself and leave important questions unanswered, which is unsatisfying for the reader. This questions the thoroughness of your approach, e.g.:
   - l. 186-187: '...we did not do a detailed glacier-by-glacier analysis of the reason behind the failure of the algorithm'... Well, you should do this! May be something intrinsically wrong with your setup (e.g. in terms of mass conservation, boundary conditions; see 2c). If this is the case, this is likely to have direct consequences for your results and for some of your conclusions...
   - l. 247-248: 'We do not have a clear explanation of this effect as yet': ...

- l.256: '**Again**, we do not have a theoretical argument for such a power-law behavior and did not explore this further here': …
- l.304-305: '…, it remains to be investigated if the results described here depend on the regional characteristics of glaciers to some extent': …

[3] The **main conclusion** drawn your manuscript, and which also appears in the title, is that using V-A scaling methods (with 'time-invariant scaling') are likely to underestimate the future sea level contribution from glaciers.

a. I am not sure that the material you presented is convincing enough to support this statement and that the experimental setup is adequate (see previous point).

b. Another major concern that I have is: if this would be the case: why do we not see this when comparing outcomes of V-A scaling estimates compared to more sophisticated methods relying on retreat parameterizations (Huss & Hock, 2015) or flowline models (Maussion et al., 2019)? The first phase of the GlacierMIP project (Hock et al., 2019), in which future large-scale glacier simulations from the literature were compared, did not reveal a tendency for V-A scaling methods to underestimate the contribution to sea-level rise (SLR). Also in the second phase of the GlacierMIP experiments, in which several ice dynamic (vs. V-A) were included and in which coordinated experiments were performed, no clear tendency can be seen when considering V-A scaling vs. methods in which the glacier geometry (and in some cases also ice dynamics) are explicitly considered. From the material at hand, I would rather tend to believe the outcomes from GlacierMIP than the main conclusions put forward here when it comes to the implications of using V-A scaling for future sea level projections.

c. You draw your main conclusion (that the loss from V-A scaling with time-invariant scaling is underestimated vs. SIA) from two steady states: an initial one and a final one. You present your results like transient results (e.g. in plots, when describing response times, in section 4.1. describing that *c* is time-dependent and decreases with time, in section 4.4.,….etc.), but in the end, **it boils down to the fact that the volume of the final steady state with time-invariant V-A scaling is 'too large'** (compared to the SIA). Due to this, the transient volume loss when evolving to this steady state is underestimated (always with respect to SIA results). **The main question that you thus need to address is: why is the V-A scaled final steady state too big?** I am not an expert in V-A scaling, but I would find it surprising that this issue has not been addressed in other V-A scaling studies and that no solutions to this problem have been formulated. In the end, from my understanding, what happens is that many glaciers that reduce in size lose their lowest part, which are often the most gently sloping parts of the glacier and where the highest ice thickness is thus found (in most ice thickness reconstructions this clearly appears, where in the end, a large part of the reconstruction results from the negative correlation between the surface slope and the local ice thickness; see Farinotti et al., 2017). It is thus to be expected that the V-A scaling that you use to create the initial steady state does not hold for the final one. This is something that would need to be explored in more detail, and for which studies in which the volume scaling also uses information from other glacier characteristics (e.g. the glacier slope) could be useful (Grinsted, 2013; Zekollari & Huybrechts, 2015; see e.g. Fig. 9a in the latter, which summarizes the main point made here).

[4] **Unclarities** in the manuscript. I found the text difficult to follow and quite often had to re-read sentences several times before being able to grasp their meaning. A few examples include:

a. l. 8-9: '..and validate them with results from scaling-based simulation of the ensemble of glacier'

b. l.84-85: '...are then empirically extended in order to obtain accurate parameterisations the linear-response properties of the SIA-simulated glaciers'
c. l.86-87: 'The linear-response model the long-term total shrinkage of glaciers as predicted by the scaling-based method (Radić et al., 2007), and the linear-response model are compared with the corresponding response'
d. ....etc. See also comments on specific sections below.

This makes it tedious to go through the manuscript. Furthermore, there a substantial number of grammatical errors, some of which (but not all) have already been pointed out by the first reviewer. Also, many figures cannot be interpreted/read independently, without having to refer to the caption. It would be good if all essential information (e.g. meaning of colors used, R^2 values, equations,...etc.) could be directly included in the figure.

Some other **comments for specific sections** (non-exhaustive list and not focusing on grammatical errors)

- 1. Introduction: 'methods solving the dynamical ice-flow equations' → 'numerical cost of such a computation on a global scale is prohibitive': well is not really the case anymore. In general: would be good to acknowledge regional- to global studies in which ice flow is explicitly accounted for (Clarke et al., 2015; Maussion et al., 2019; Zekollari et al., 2019).
- 1.2. Motivation for the present study: difficult to follow the first paragraph: be more specific when you refer to *c* and γ and do not continuously mix with other terminology 'time invariant scaling-based parameterisation', '...given the known violation of the time-invariant scaling assumption'.
- 2. Quite abstract and thus very difficult to go through for someone who is not an expert in V-A scaling. Could make it less technical by for instance adding some additional information that links the various parts.
- 3.1.: 2-dimensional SIA model:
    - l.152-154: where did you get the ice thickness from? From Kraaijenbrink et al. (2017) directly? As the ice thickness is quite crucial in your story (it determines the volume...), why did you not consider the consensus estimate of Farinotti et al. (2019), which is freely available?
    - SIA: refer to the original work by Hutter (1983) also.
    - You neglect basal sliding (l. 161). Justification? Could refer to other studies where this is done, like e.g. Gudmundsson (1999) and Clarke et al. (2015).
    - l.168-178: this is related to the SMB, which you apply in all cases (i.e. also for the linear-response model and the V-A scaling, right?). Not sure this section is correctly placed here in the '2-dimensional SIA model' section.
    - l.183: through several exclusion you keep 68% of the initial glaciers... How much does this represent in terms of glacier volume and glacier area when compared to the total glacier sample?
    - l.188-196: you explain some simplifications related to debris cover, avalanche and sliding have been made and that this may influence your results. Well, you have made much larger simplifications than this: e.g. linear SMB profiles with strongly imposed max. SMB, steady state assumptions for glaciers,... → not even worth mentioning these more detailed simplifications in my opinion. With all these simplifications, would have been better to opt for idealized setup likely (see main comment 2a).
    - l. 194: 'These simplifications do not weaken our study': not sure you can judge on this yourself...
- Section 3.2.:
    - l.204: 'was fixed at... because...': don't understand the causality (i.e. link between cause and consequence).

        - Figure 1: SIA-derived volumes are scaled by a factor 10: why? Does not really make sense and unclear when just looking at the figure without reading the caption… Axes should be correct in the figure and not only for a part of the data you show.. Also illustrates the unclarity in the figures mentioned in main comment 4 (problem that figures cannot be interpreted without referring to their caption).
- Section 3.3.:
    - l.208-210: complicated way to say that you consider e-folding time scales. Would reformulate this and add references for this to e.g. Leysinger Vieli & Gudmundsson (2004).
- Section 4.1.:
    - l.225: $V=cA^{1.286}$: not sure I understand. Does this statement apply for the initial and/or final steady state volumes? And can all the volumes be described with this single relationship? Is the fact that quite different rate factors are used not a problem for this (see main comment, point 2c)?
    - l.227 + l.230 + l.232: here you mention that *c* is time dependent. Not sure you can say that it is time dependent: simply results from the fact that final steady state volume for V-A scaling is 'overestimated' (vs. SIA). As a result the evolution to this steady state is different. See main comment 3c for this.
    - l.235-237: relates to main comment 3c again. If you do not modify the V-A scaling, then problems will arise when considering the same glacier that is much smaller in a warmer climate (when rising the ELA in your case): you typically lose the lower parts where most volume is and volume will thus be 'overestimated'. Is this not accounted for in some way in future glacier evolutions based on V-A scaling? As a part of this discussion, studies in which V-A scaling is extended with other glacier characteristics (such as the surface slope; Grinsted, 2013; Zekollari & Huybrechts, 2015) would be good to include. Such relationships which could prove to remain valid over time, even without changing scaling and exponents.
- Section 4.2.:
    - l. 243: 'This is exactly what is seen in Fig. 2b, which shows…': I cannot directly see this…
    - l.244: 'change in c to the tune of ~13%': what does this mean?
    - l.255: 'The above figure': will depend where your figure comes in final manuscript…
- Section 4.3:
    - I was wondering what the point is that you want to make with this section? It is known from literature that volume responds faster than area (e.g. Oerlemans, 2001; Leysinger Vieli & Gudmundsson, 2004).
    - l.260-264: relationship between volume and area response times. How does this compare to the relationship others have found in the literature?
- Section 4.4.:
    - l.271-272: 'with most of the changes taking place during the first couple of centuries': this is not a result/finding.. This directly results from the e-folding time-scale when forcing a steady state glacier with an instantaneous forcing in SMB.
    - l.273: 'underestimates the long-term change': not about reaction/response. This is direct consequence of fact that final steady state volume is too large (see main comment 3c)
    - l.279-280: '…suggests that there might be significant negative biases of mountain glacier contribution to sea-level rise as computed by scaling-based methods' (+ section 4.5, l.300-302): well, do not see this in GlacierMIP phase 1+2… Is a very strong statement to make and should be sure that it is well-founded.
- Section 4.5:

- l.296-297: 'More detailed studies that relaxes some of the above mentioned assumptions are needed…': not sure what you mean by this. Would also make sense that you dig into this: e.g. by focusing on real transient response vs. comparing two steady states (what you do now and then translate into an analysis of the transient response resulting from this: see main comment 3c).
- l.299: 'intruding more scatter in the fits': what does this mean?

o Summary and Conclusions:

- l.309-310: scale factor reduces over time. Well, not sure the time dimension is adequate here. Boils down to having a final steady state that would require a smaller value for *c*: see main comment 3c.
- l.324: computational efficiency. OK, still important, but is not really a limitation anymore, due to which V-A scaling becomes less important (and also driven by the release of new datasets with regional- to global spatial coverage at individual glacier level: see main comment 1).
- Code availability: for which models is the code available? Seems to suggest that the SIA code is not available. Not sure if this fully agrees with the policies of The Cryosphere: see www.the-cryosphere.net/about/data_policy.html

References

Adhikari, S., & Marshall, S. J. (2012). Glacier volume-area relation for high-order mechanics and transient glacier states. *Geophysical Research Letters*, *39*, L16505. https://doi.org/10.1029/2012GL052712

Braun, M. H., Malz, P., Sommer, C., Farias-Barahona, D., Sauter, T., Casassa, G., et al. (2019). Constraining glacier elevation and mass changes in South America. *Nature Climate Change*, *9*, 130–136. https://doi.org/10.1038/s41558-018-0375-7

Brun, F., Berthier, E., Wagnon, P., Kääb, A., & Treichler, D. (2017). A spatially resolved estimate of High Mountain Asia glacier mass balances from 2000 to 2016. *Nature Geoscience*, *10*, 668–673. https://doi.org/10.1038/NGEO2999

Clarke, G. K. C., Jarosch, A. H., Anslow, F. S., Radić, V., & Menounos, B. (2015). Projected deglaciation of western Canada in the twenty-first century. *Nature Geoscience*, *8*, 372–377. https://doi.org/10.1038/ngeo2407

Dussaillant, I., Berthier, E., Brun, F., Masiokas, M., Hugonnet, R., Favier, V., et al. (2019). Two decades of glacier mass loss along the Andes. *Nature Geoscience*. https://doi.org/10.1038/s41561-019-0432-5

Farinotti, D., Brinkerhoff, D. J., Clarke, G. K. C., Fürst, J. J., Frey, H., Gantayat, P., et al. (2017). How accurate are estimates of glacier ice thickness? Results from ITMIX, the Ice Thickness Models Intercomparison eXperiment. *The Cryosphere*, *11*, 949–970. https://doi.org/10.5194/tc-11-949-2017

Farinotti, D., Huss, M., Fürst, J. J., Landmann, J., Machguth, H., Maussion, F., & Pandit, A. (2019). A consensus estimate for the ice thickness distribution of all glaciers on Earth. *Nature Geoscience*. https://doi.org/10.1038/s41561-019-0300-3

Glen, J. W. (1955). The Creep of Polycrystalline Ice. *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Sciences*, *228*(1175), 519–538. https://doi.org/10.1098/rspa.1955.0066

Grinsted, A. (2013). An estimate of global glacier volume. *The Cryosphere*, *7*(1), 141–151. https://doi.org/10.5194/tc-7-141-2013

Gudmundsson, G. H. (1999). A three-dimensional numerical model of the confluence area of Unteraargletscher, Bernese Alps, Switzerland. *Journal of Glaciology*, *45*(150), 219–230. https://doi.org/10.3189/002214399793377086

Haeberli, W. (2016). Brief communication: On area- and slope-related thickness estimates and volume calculations for unmeasured glaciers. *The Cryosphere Discussions*, (January), 1–18. https://doi.org/10.5194/tc-2015-222

Haeberli, W., & Hoelzle, M. (1995). Application of inventory data for estimating characteristics of and regional climate-change effects on mountain glaciers: a pilot study with the European Alps. *Annals of Glaciology*, *21*, 206–212. https://doi.org/10.3189/S0260305500015834

Hock, R., Bliss, A., Marzeion, B., Giesen, R. H., Hirabayashi, Y., Huss, M., et al. (2019). GlacierMIP – A model intercomparison of global-scale glacier mass-balance models and projections. *Journal of Glaciology*. https://doi.org/10.1017/jog.2019.22

Huss, M., & Farinotti, D. (2012). Distributed ice thickness and volume of all glaciers around the globe. *Journal of Geophysical Research: Earth Surface*, *117*(4), F04010. https://doi.org/10.1029/2012JF002523

Huss, M., & Hock, R. (2015). A new model for global glacier change and sea-level rise. *Frontiers in Earth Science*, *3*, 1–22. https://doi.org/10.3389/feart.2015.00054

Hutter, K. (1983). *Theoretical Glaciology*. Dordrecht: Reidel Publ. Co.

Jarosch, A. H., Schoof, C. G., & Anslow, F. S. (2013). Restoring mass conservation to shallow ice flow models over complex terrain. *The Cryosphere*, *7*, 229–240. https://doi.org/10.5194/tc-7-229-2013

Jouvet, G., & Bueler, E. (2012). Steady, shallow ice sheets as obstacle problems: well-posedness and finite element approximation. *Journal of Applied Mathematics*, *72*(4), 1292–1314. https://doi.org/10.1137/110856654

Kraaijenbrink, P. D. A., Bierkens, M. F. P., Lutz, A. F., & Immerzeel, W. W. (2017). Impact of a global temperature rise of 1.5 degrees Celsius on Asia's glaciers. *Nature*, *549*, 257–260. https://doi.org/10.1038/nature23878

Leysinger Vieli, G. J.-M. C., & Gudmundsson, G. H. (2004). On estimating length fluctuations of glaciers caused by changes in climatic forcing. *Journal of Geophysical Research*, *109*(F01007), F01007. https://doi.org/10.1029/2003JF000027

Marzeion, B., Jarosch, A. H., & Hofer, M. (2012). Past and future sea-level change from the surface mass balance of glaciers. *The Cryosphere*, *6*(6), 1295–1322. https://doi.org/10.5194/tc-6-1295-2012

Maussion, F., Butenko, A., Champollion, N., Dusch, M., Eis, J., Fourteau, K., et al. (2019). The Open Global Glacier Model (OGGM) v1.1. *Geoscientific Model Development*, *12*, 909–931. https://doi.org/10.5194/gmd-12-909-2019

Oerlemans, J. (2001). *Glaciers and climate change*. Dordrecht: A. A. Balkema.

Radić, V., Bliss, A., Beedlow, A. C., Hock, R., Miles, E., & Cogley, J. G. (2014). Regional and global projections of twenty-first century glacier mass changes in response to climate scenarios from global climate models. *Climate Dynamics*, *42*(1–2), 37–58. https://doi.org/10.1007/s00382-013-1719-7

Radić, V., Hock, R., & Oerlemans, J. (2007). Volume-area scaling vs flowline modelling in glacier volume projections. *Annals of Glaciology*, *46*, 234–240. https://doi.org/10.3189/172756407782871288

Rounce, D., Hock, R., & Shean, D. (2020a). Glacier mass change in High Mountain Asia through 2100 using the open-source Python Glacier Evolution Model (PyGEM). *Frontiers in Earth Science*, *7*, 331. https://doi.org/10.3389/feart.2019.00331

Rounce, D. R., Khurana, T., Short, M., Hock, R., Shean, D., & Brinkerhoff, D. J. (2020b). Quantifying parameter uncertainty in a large-scale glacier evolution model with a Bayesian model: Application to High Mountain Asia. *Journal of Glaciology*.

Zekollari, H., Huss, M., & Farinotti, D. (2019). Modelling the future evolution of glaciers in the European Alps under the EURO-CORDEX RCM ensemble. *The Cryosphere*, *13*, 1125–1146. https://doi.org/10.5194/tc-13-1125-2019

Zekollari, H., & Huybrechts, P. (2015). On the climate-geometry imbalance, response time and volume-area scaling of an alpine glacier: insights from a 3-D flow model applied to Vadret da Morteratsch, Switzerland. *Annals of Glaciology*, *56*(70), 51–62. https://doi.org/10.3189/2015AoG70A921

Zemp, M., Huss, M., Thibert, E., Eckert, N., McNabb, R., Huber, J., et al. (2019). Global glacier mass balances and their contributions to sea-level rise from 1961 to 2016. *Nature*, *568*, 368–386. https://doi.org/10.1038/s41586-019-1071-0