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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Modeling the effect of glacier recession on streamflow response using a coupled glacio-hydrological model" *by* B. S. Naz et al.

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I have read this paper with interest and I fully agree with reviewer 1 that this manuscript is suitable for publication in HESS after revisions.

Following reviewer 1, I think that the paper should present more details on the hydrological model. I was furthermore also surprised by the relatively low model performance (in terms of Nash values) for streamflow. Nash values of 0.7 for daily and 0.8 for monthly (which is the easiest time step to simulate) are rather low for glacier-influenced hydrologic regimes (i.e. for strong annual cycles), see a discussion in (Schaefli and Gupta,

2007). Given that the model is fully distributed, I understand of course that automatic calibration is limited (which would give better results) but it would nevertheless be worth commenting on this; the shown monthly streamflow simulations (Fig. 11) might give a feeling for the overall quality of the simulations but it would be nice to have also 2-3 years of daily observed and simulated data to judge how well the model predicts daily flow during periods with glacier melt (Fig. 11 could show monthly and daily but only for the validation period or observed could be added to Fig. 12). Finally, just as reviewer 1, I think that comparing the model performance with and without glacier model is not interesting, a comparison to a static glacier model would give interesting new results.

Additional detailed comments:

- In the introduction it is stated that " However, in these studies glaciers were represented as static deep snowpacks (..). This approach could result in distortion of the parameters that control snow accumulation and glacier melt (..)." I was surprised to see this comment for a physically-based modeling approach since I would have expected such a distorsion only for calibrated models using a temperature-index approach. Later on, I understood that the model uses calibrated meteorological parameters (lapse rates). I think it is quite important to emphasize that the model is physically-based but that it has a strong degree of meteo calibration (which compensates for imperfect descriptions of the snow / ice accumulation and melt process).

- "When the snow has completely melted, the ice (..) [is] melting at a rate determined using a modification of the energy balance approach incorporated in DHSVM." What type of modification ?

- "(..) the integrated model was better able to capture how climate variations cause changes in glacier cover and streamflow dynamics." Does the paper actually show this?

- What method was used to derive the stream network?

- "Soil properties similar to those of bare rock class were used for the glacierized areas.": Similar or the same parameters?

- "Soil depth information was derived from the DEM (Fig. 4e) based on local slope (determined from the DEM), upstream source area, and elevation.": used method, reference?

- Figure 7: what explains the strange shape of the ice thickness increase (just out of curiosity)?

- Figure 8: does it show the simulation results before or after albedo adjustment? From the text it is not clear.

- Table 1 mentions snow roughness as calibration parameter, the text discusses only albedo; should be completed; several units are missing

- The sketch of Fig. 1c should be vector-based to zoom in.

References

Schaefli, B., and Gupta, H.: Do Nash values have value?, Hydrological Processes, 21, 2075-2080, 10.1002/hyp.6825, 2007.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5013, 2013.