Response to Reviewer #2:

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

Referee #2: B. Schaefli

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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.

We wish to thank Dr. Bettina Schaefli for her comments and constructive criticism which we believe has led to an improved manuscript. Below are specific answers to her comments.

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).

See our response on lower NS values to Reveiwer #1 comment-5.

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).

In the revised MS, we now have updated Figure 11 to show the daily comparisons for a few selected years.

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.

See our response to Reviewer #1 comment-2.

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).

Uncertainties in meteorological forcings and how they are distributed with elevation is now discussed briefly in the revised manuscript to clarify our use of temperature and precipitation lapse rates in calibration.

- "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 ?

When the snowpack has melted, the glacier ice melts at a rate determined by calculating the surface energy balance in the same manner as the snowpack, assuming the surface temperature is zero C and surface albedo is 0.35. This is now clarified in the revised manuscript.

- "(..) 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?

Comparison of integrated model results with static ice and prescribed glacier extent is now included in the revised manuscript which clarifies this statement.

- What method was used to derive the stream network?

A description of the method used to derive the stream network is now included in the revised manuscript. Basically, it is based on D-4 flow direction algorithm and flow accumulation files.

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

The same parameters as for bedrock were used. We have now corrected this in the revised manuscript.

- "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?

The specification of soil depth is effectively a calibrated parameter because its actual value and distribution across the landscape is unknown. We do however use an approach that spatially distributes it based on local slope (determined from the DEM), upstream source area, and elevation (see Westrick, 1999 for details). This has been now clarified in the revised manuscript.

Westrick, K. 1999. Soil depth calculation “aml” [online]. Available from www.hydro.washington.edu/Lettenmaier/Models/DHSVM/index.shtml [accessed January 2011].

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

In the spinup run, the glacier model initially accumulates ice on the bedrock until the mean elevation of the ice is greater or equal to the mean elevation in the study area and then it starts flowing down the slope, which is why we see the bump in the ice thickness at the start of simulation.

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

Simulation results are after the albedo adjustment. This is clarified in the revised manuscript.

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

Table 1 has been revised as per reviewer suggestion.

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

Corrected.

References

Schaefli, B., and Gupta, H.: Do Nash values have value?, Hydrological Processes, 21,