Response to Reviewer #1:

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

Anonymous Referee #1

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General assessment

This ms describes the development of a process-based, integrated model of catchment hydrology and glacier dynamics and its application to a medium-sized catchment in the Canadian Rockies. The topic is timely and of great interest within the hydrology and water management communities given the potentially significant changes in streamflow that could occur as a result of ongoing climate warming and associated glacier retreat. The topic is, therefore, highly suitable for publication in HESS.

The study makes an important contribution that builds on previous research and advances the capability of models to simulate hydrologic response under transient land-cover conditions, particularly in the context of making projections under future climate scenarios. As reviewed by the authors, one earlier study (Stahl et al., 2008) had developed an integrated model of hydrology and glacier response, but the glacier response was simulated using volume-area scaling rather than a process-based model of glacier flow. Again as reviewed by the authors, Jost et al. (2012) used output from a physically based glacier dynamics model to update glacier hypsometry and coverage in a hydro- logic model. However, this parallel modelling approach has a number of shortcomings. Therefore, the integrated approach developed by Naz et al. represents an important "next step" in the modeling of the hydrologic impacts of climatic change and associated glacier response and deserves to be published in an international journal such as HESS following revision to address the specific points raised below.

We wish to thank Anonymous reviewer #1 for his/her comments and constructive criticism which have led to an improved manuscript. Below are specific answers to his/her comments.

Specific comments

1. The algorithms used to simulate glacier hydrology need to described more completely. Specific points include the following:

a. Was glacier melt treated like snowmelt and routed through a soil layer, or was it routed through multiple parallel reservoirs with different coefficients (e.g., Hock, 1999)?

If the former, what is the physical justification?

In the current configuration of the model, the glacier melt is treated like snow melt and is routed through the soil layer. Glacier melt routing through multiple parallel reservoirs is most often applied to an entire glacier area, delaying and releasing melt water at one point, the proglacial outlet stream of the glacier. We do not use such a formulation in our distributed melt modeling approach. To represent these processes in our distributed approach we would need to implement representations of vertical and horizontal meltwater transport in each grid cell with a glacier. These processes are important in the case of simulating discharge timing close to the outlet of the glaciers, but less so in watersheds where the glacier makes up a modest part of the total area. In the Bow River application, we evaluate the contribution of glacier melt for a large drainage area (420 km^2) with only 10% glacier cover and the representation of the above-mentioned processes will have considerably less influence on simulated streamflow at the outlet of the watershed than at the outlet of the glacier. In the revised MS, these issues are now explicitly described in Section 2.3.

b. What albedo values were used for glacier ice?

We used a constant albedo of 0.35 glacier ice. This value is now specified in the revised manuscript.

c. The model does not include a firn layer and, instead, converts snow-water equivalent in excess of 5 m to ice. Is there a physical rationale for this approach (e.g., why 5 m)?

After the manuscript was submitted, explicit calculation of snow densification due to overburden compaction was incorporated in the code. The second layer of the snowpack is now transferred to the ice layer when the calculated density exceeds a threshold of 850 kg/m^2. We now include a discussion of this, and results which reflect this change, in the revised manuscript.

To what extent might the lack of treatment of firn influence the hydrologic simulation, given the distinctive hydrologic characteristics of firn (e.g., albedo intermediate between snow and ice; hydraulic characteristics similar to a coarse sand aquifer) (e.g., Fountain, 1998; de Woul et al., 2006)?

The influence of melt water storage and movement in the firn layer on streamflow timing at broader scales is addressed in our response to your query c) above.

Neglecting the influence of the albedo of a firn layer on portioning energy into melt could make some difference to the calculation of melt rates in particular conditions: In the case where all snow that accumulated during a given water year melts, snow older than 1 year (firn) that has not reached ice density is still considered snow in the model. Melt will be underestimated as albedo calculated from the decay curves would be higher than 1 year old snow (firn). This scenario should be spatially limited to areas near the

ELA in average years, however would affect a larger area in anomalous years where the ELA increases in elevation. For these extreme years, inclusion of a firn layer would increase the accuracy of the simulation of melt on the glacier. In the revised MS, we now acknowledge the effects of our decision to ignore this effect, and note that we have opted for a simpler configuration of ice and snow layers that ignores this effect.

d. Was heat conduction into and out of glacier ice simulated? If so, was a two-layer approach similar to that used in the DHSVM snowpack model used?

In the current model configuration, the glaciers are assumed to be isothermal (at 0 degrees), which arguably is a reasonable assumption for temperate glaciers. Inclusion of a multilayer representation of ice heat conduction and storage would make the model more applicable to a broader range of climatic regions. We now clarified this in the revised MS.

e. In areas exposed by glacier retreat, how were characteristics such as soil depth and hydraulic parameters specified? What value was used for soil moisture at the time of ice disappearance?

The soil type, depth, and hydraulic properties do not change upon ice disappearance. The continuous computation of soil moisture does not change whether the glacier is present or not. We have clarified this in the revised text.

2. This work is essentially a proof of concept. The paper would be a stronger contribution if the authors took the work a step further. For example, the authors could consider exploring the error associated with assuming static glacier cover. It is unlikely that fully coupled models will be used in operational forecasting in the near future and that conventional models that assume static glacier cover will continue to be used. The authors could use their model to explore how prediction errors evolve through time as the glacier area and hypsometry evolve away from the static representation used in model set-up. See also comment 4, below.

In the revised manuscript we now include a comparison of model simulations using static vs. dynamic glacier representation. However, we don't see much difference due to the fact that we are running the model for a relatively short period of historical record, and inclusion of dynamics may not have much influence over a couple of decades (depending on the amount of retreat). The motivation for the model is not, however, simply to reconstruct past observations, but rather to be able to evaluate the effect of long-term glacier changes on streamflow, in particular in a warming climate. In such cases, it's not possible to prescribe the glacier extent, and we need a model structure that will do this. We now briefly discuss this in introduction section of the revised MS that should make our objectives more clear to the reader.

Another issue that could be explored is the sensitivity of glacier changes to the specification of the sub-glacial topography. It would be valuable and informative for researchers following up on this work if the authors could perform simulations using alternative sub-glacial topographies generated by different plausible approaches.

For our model simulations we used the Clarke et al., 2012 approach to estimate the glacial bed topography. Based on our stand-alone glacier model runs initialized with alternative sub-glacial topographies (e.g. estimated using surface slope from DEM and assuming a gravitational driving stress of about 10^5 Pa for all glaciers), the ice volume and ice areas are fairly comparable and did not effect the hydrologic responses for our historic simulation time period. However, uncertainties in ice thickness using such simple approach might be more important for long term simulation (future climate) to accurately simulate glacier recession. We did not include this analysis in our paper, as it is not the focus of our paper.

3. While the authors appropriately highlight a number of limitations associated with modeling approaches that use external information to update glacier cover during a model run, they should also provide some consideration of the limitations of this integrated modeling approach. For example, despite the use of physically based algorithms, the authors still had to resort to calibration to achieve reasonable streamflow predictions – but the long run times did not allow for sufficient runs to explore the effects of parameter uncertainty on streamflow predictions. Another potential issue when applying this approach to diagnose historic contributions of ice melt to streamflow is the errors in predicted glacier area, which would result in biased estimates (e.g., Figure 10). For that type of application, it is arguably more appropriate to use externally prescribed glacier coverages based on mapping products.

See above our response to comment #2. We agree that using prescribed glacier area in additional simulations and comparing the modeled streamflow with simulations based on the integrated dynamic model will be useful, and such simulations are reported in the revised paper.

It should be noted, however, that we have evaluated the coupling of the models in predicting historical glacier recession by comparing with the Landsat ice extent estimates, and these comparisons indicate some confidence that the model can be used for future predictions of recession.

4. The authors highlight the fact that the streamflow simulations were substantially improved by inclusion of the glacier routines (e.g., Figure 12). This is not a surprising result given the amount of glacier cover in the catchment. A more interesting and informative effort would be to compare streamflow simulations with dynamic and static glaciers. The authors claim that the dynamic glacier representation allows better streamflow prediction than simulations based on a static glacier (p. 5033, line 8-10), but I

could find no supporting evidence for that statement in the ms, such as a comparison of model runs with static and dynamic glacier representations.

The manuscript has been revised to show this comparison.

5. Based on the literature, it does not appear to be difficult to achieve Nash-Sutcliffe efficiencies in excess of 0.8 in glacier-fed catchments at a daily resolution. However, the model performance in this application fell short of this benchmark. It would be constructive for the authors to consider more carefully the nature and sources of streamflow prediction error. For example, in the discussion, the authors attribute the underestimation of late-summer flow to an underprediction of ice extent. They then state that this error is decreased later in the melt season due to a mass balance-elevation feedback. The evidence for this feedback is unclear; it is not obvious in the pattern of prediction errors shown in Figure 11.

There are several possible reasons why the NS values of our simulations are lower than those of some previous modeling efforts in glaciated catchments.

(1) First, using predicted glacier cover rather than prescribed glacier cover likely impacts the streamflow predictions to some extent. For example, late summer flow is underpredicted which is likely related to under-estimated ice extent. As previously discussed, the comparisons of model results with prescribed glacier extents should help to identify the role of simulated glacier extent in streamflow prediction. If using prescribed glacier extents does not explain this deficiency in the simulation of low flows, it is likely that baseflow decay in the soil layer is not well represented, which can be easily altered and recalibrated (we have a non-standard version of DHSVM which has this capability).
(2) Year-round discharge observations for the Bow River are only available before 1987. After 1987, observations are only available during the melt season (March-September in most years). In temperate high elevation regions streamflow activity during the winter is benign and most models are able to capture it easily. This should be noted when comparing the NS values of this study, to others who compared with year round observations, as simulating the less variable low flow outside of the melt season can greatly improve evaluation metrics.

(3) When making comparisons with other studies, the nature of the models being compared should also be considered. Models of a more empirical nature (eg Jost et al. 2012) may be better suited to calibration and simulation of historical flows as they often require less computational time and include many empirical parameters for site specific calibration. Models rooted in physical processes (e.g. our model, and Finger et al. 2011), sacrifice flexibility in calibration to historical flows, however may be more robust for future prediction under climatic and environmental change as most physical processes are explicitly simulated. That being said, our model performance falls short of Finger et al. 2011, and we are exploring reasons, as discussed above.

(4) The error in late summer flow due to underprediction of the predicated ice extents is decreased at some extent because of our use of the dynamic model which transports ice from higher to lower elevation by simulating the feedback between mass balance and glacier geometry/elevation changes resulting from retreat or advance of the glacier.

The above points are all discussed in the revised manuscript.

An alternative possibility is error in simulating snow dynamics. Figure 12a indicates that streamflow tends to be over-predicted in June and early July and under-predicted in July and August. This pattern could reflect an over-prediction of snow accumulation, which would result in higher summer flow contributions from unglacierized parts of the catchment and a suppression of glacier melt contributions later in the summer due to the later disappearance of the higher-albedo snow.

We agree. To reduce this error the model was calibrated by tuning the empirical parameters of the snow albedo curves. We selected the best parameters based on streamflow and SWE comparison with observations.

6. I do not believe that the authors have accurately characterized the current state of hydrologic modelling in some of their statements in the introduction. Two specific points follow.

a. On p. 5015, line 17-19, the authors claim that we have a limited ability to predict runoff in partially glacierized basins. On the contrary, there is a vast body of literature demonstrating that existing catchment models can simulate streamflow in partially glacierized catchments with Nash-Sutcliffe efficiencies well in excess of 0.8. A number of these models were also constrained to reproduce glacier mass balance, glacier snowlines or integrated glacier volume loss to help ensure that snow- and ice-melt contributions were properly simulated. These models are currently used with apparently reasonable success by a number of agencies around the world for operational forecasting and water resource assessments.

See our response to comment#5 on comparison of NS values between different models. The introduction section in the revised MS has been modified to focus on the model's applicability to future conditions, specifically prediction of recession and glacier melt outside of the observed record, which is where many of the current models may fall short.

b. On p. 5015, line 27-29, the authors state that snowmelt-runoff models such as HBV require snow-covered area to be prescribed. That is not true. Models like HBV and many other conceptual-parametric snowmelt-runoff models (e.g, PREVAH) simulate the evolution of snowpack water equivalent in a semi-distributed fashion; they do not require external information on snow-covered area, although that type of information has been used in calibration and testing.

This correction has been made in the revised manuscript.

Technical points

7. p. 5015, line13. comma splice: "headwaters, however ..."

Corrected.

8. p. 5032, line 23. insert "is" to follow "but also"

Corrected.

References

Clarke, G. K., Anslow, F. S., Jarosch, A. H., Radic, V., Menounos, B., Bolch, T., and Berthier, E.: Ice volume and subglacial topography for western Canadian glaciers from mass balance fields, thinning rates, and a bed stress model, J. Climate, doi:10.1175/JCLI-D-12-00513.1, 2012.

de Woul, M., Hock, R., Braun, M., Thorsteinsson, T., Johannesson, T., Halldorsdottir, S. 2006. Firn layer impact on glacial runoff: a case study at Hofsjoekull, Iceland. Hydrological Processes 20: 2171-2185.

Fountain, A.G., 1996. Effect of snow and firn hydrology on the physical and chemical characteristics of glacial runoff. Hydrological Processes 10: 509-521.

Hock, R., 1999. A distributed temperature index ice and snow melt model including potential direct solar radiation. Journal of Glaciology 45(149), 101-111.