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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 ad-

vances 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 hydrologic 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.

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?

b. What albedo values were used for glacier ice?

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)? 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)?

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?

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?

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. 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.

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.

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.

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. 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.

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.

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.

Technical points

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

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

References

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