Response to Reviewer #3:

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 #3: David Finger

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Summary of the manuscript: This manuscript presents the application of a dynamically coupled, spatially distributed hydrologic model (DHSVM-MODEL) and a dynamic glacier model (SIA-MODEL) to a glaciated Canadian study site (Upper Bow River). The model was calibrated by identifying five key model parameters (conductivity, exp. decrease, snow roughness, P-laps rate and T-laps rate) and adjusting them "one at a time" to observational datasets for all odd-years between 1979 and 2007. Model outputs were evaluated with observed discharge, SWE at two locations, glacier mass balances and glacier extent during even years between 1979 and 2007. The model results are used to quantify the effects of glacier retreat during the investigated modeling period on discharge patterns. Furthermore, contribution of glacier melt, snow melt and precipitation to the total runoff is estimated. The study concludes by assessing glacier melt contribution to total flow in the Bow River, quantifying temporal trends in the total runoff and identifying that glacier contribution is not yet decreasing due to retreating glaciers.

We wish to thank David Finger for his comments and constructive criticism which we believe has led to an improved manuscript. Below are specific answers to his comments.

The manuscript convincingly presents model set up, calibration and validation. In particular the mutli-variable validation can be highlighted, as discharge, SWE, mass balances and glacier extents are reproduced adequately by the model. While the presented topic has been discussed by many authors in recent years, I see two fundamental new contributions to the ongoing discussion of glacier retreat effects on runoff: i) a glacier dynamic model has been dynamically coupled (or integrated) in a fully distributed hydrological model which improves model performance and ii) the assessment of glacier contribution to runoff is of major importance as it may generate social, ecological and economic impacts in the downstream dry areas. Accordingly, I do think that this study is suitable for publication in HESS, following revisions addressing the specific points listed thereafter. Also, referring to the comment posted by reviewer 1, I also think that additional information is needed to clarify some modeling approach.

Major comments: 1) This may be one of the first studies where a dynamic glacier model has been coupled dynamically to a physically based hydrological model. In order to demonstrate the added value of this technique the results have to be compared to the results of a hydrological model with static glaciers and periodic updates of the glacier extents. In accordance with reviewer 1 and Dr. Schäfli, I also believe that a comparison without glacier model is of little interest, as the effects are obvious.

Yes, we agree that the results may be obvious but comparing with no glacier model run helps to estimate the glacier melt contribution to streamflow and effects of glacier disappearance on downstream water resources in future.

The interesting question is: What is the added value of the dynamic coupling of the two models compared to a model where glacier extents are updated periodically?

The integrated model, after validation during a period of observations, can be used to make predictions outside of the period of record, future or past, where glacier extent observations are not available. Having the mass and energy balance of the hydrologic model entirely connected to the glacier dynamics model, should provide the most accurate representation of their interdependent processes and also ensures mass conservation. Additionally, using a stand-alone glacier model to make future predication of the glacier extents may not be consistent in terms of physics/parameterizations for the hydrological and glaciological snow melt.

2) In the introduction it is briefly mentioned that discharge from glaciated catchments provide crucial water resources to the dry downstream areas in Canada. This statement is certainly true, but it could be discussed in more detail and the results of the study should be put into the context of potentially declining water resources in the downstream dry areas. What can be learned from the simulations? And is the model accuracy high enough to assess impacts on water availability for the downstream areas?

One reason to compare the model with and without the glacier was to get an estimate of the glacier melt contribution in the basin and to assess the impact of glacier disappearance on downstream water resources in future if this contribution is not available. This is now more specifically stated in the revised manuscript.

3) Structure of the manuscript: calibration and validation is a method, accordingly it should not be in the study site section; validation performance is a result, accordingly it should be in the result section. Although structure is a question of style, I would like to recommend the following structures: 1) Introduction, 2) Study site and data (incl. Fig. 2, 3, 4, 5), 3) Methods: incl. model setup, glacier thickness estimation, calibration and validation method (incl. Fig 1, 6 evt. 7) 4) Results: incl. model performance for calibration and validation (Fig. 8 - 12), 5) discussion and 6) conclusion.

A Results section has been added in the revised manuscript to keep the methods and results separate.

4) The model uncertainty should be assessed. The authors used a "one at a time" calibration technique (more information on the calibration proceeding would be nice). The approach is adequate; all observational datasets are reproduced adequately. However, is this the only adequate optimum? Are there other adequate parameter sets leading to similar performance, but revealing different conclusions? Model uncertainty should be addressed. Especially when several datasets are used to calibrate a model, parameter sets can be adjusted to increase performance of one or another dataset leading to different optimums (e.g. see Finger et al. 2011, 2012).

In this study, we have used a similar approach to calibrate the model using different datasets such as discharge, glacier covers from Landsat images, glacier mass balance from Peyto glacier, and SWE from two stations. We evaluate the model performance by changing the calibrated parameters one by one, following first principles, and select the NS values that

reproduce the discharge, SWE, glacier and snow cover observations reasonably well. The model uncertainty to different parameter values is now briefly discussed in the calibration section of the revised paper.

I believe that if the four points mentioned above are addressed adequately, the study would improve significantly, making it a substantial contribution to the ongoing discussion about glacier retreat and its impacts on downstream water resources. Accordingly, I would also suggest a more focused title, e.g: Assessing the effects of glacier retreat on downstream water resources using a dynamic glacio-hydrologic model.

Specific recommendations: Abstract: Ln3: add county and state to the study site Ln 7- 9: not necessary here, might be deleted Ln 10: SWE of what? Mass balances or snow height? Ln13-14: why is uncertainty reduced? This was not convincingly shown: : :(see comment 1 above)

The abstract has been revised.

Introduction: Ln20: not only in Canada, but worldwide: : : (e.g. Gardner et al. 2013, Science)

The sentence has been revised.

Ln23: Why is the discharge crucial? And during which seasons? This should be addressed in more detail. The quantification of the glacier retreat in connection to downstream water availability could be, in my opinion, a main objective of the study.

The sentence has been revised. We agree that one of the objectives of the paper is to quantify the glacier contribution to the downstream water resources, however, the main objective of the paper is the development of a coupled model, validation the model method and estimation of model errors.

Pg5015, Ln 1: snow is not depleted either, as in some years mass balances are positive

The sentence has been revised for clarity.

Ln2: negative feedback should be explained

The sentence has been revised for clarity.

Ln6-9: needs a citation

A citation has been added.

Ln17: water supply for what? How much is required? Are the observed changes already crucial?

The sentence has been revised for clarity.

Ln19: why is our ability limited? Numerous studies exist: : :

The sentence in Ln 19-21 explained why our ability is limited.

Ln29: Why is the periodic update of the glacier extent a disadvantage? This issue should be

addressed better: : : e.g. model is driven with the same input data. See also recommendations below.

Our scheme includes explicit simulation of the glacier mass and energy balance and dynamically adjusts the glacier and non-glacier areas and volume depending on accumulation and ablation conditions at the monthly time step of the glacier dynamics model. This approach explicitly simulates the snowpack and glacier ice space-time distribution across the model domain. On intra and inter-annual timescales, the relationship between these two masses can play an important role on ice/snow melt dominated runoff (e.g. snow albedo feedback). The integrated model, if validated during a period of observations, can be used to make predictions outside of the period of record, future or past, where glacier extent observations are not available

The above points have been included in the revised manuscript.

Pg5016, Ln2: "on the other hand" needs a "On one hand"

Corrected.

Ln14: Why is it important to dynamically couple glacier and runoff models? There are numerous advantages, but they should be discussed: : :

The advantages are now briefly discussed in Pg 5016 and 5017.

Ln21: repetition of Line 11

These lines described two different studies.

Ln 28-29: The updating of ice extend can easily be implemented into any code; this reasoning seems week.

The sentence has been revised for clarity.

Pg 5017, Ln3: is a shorter time step necessary for glacier modeling? Glaciers evolve gradually, what is the added value of smaller time steps?

Pg 5017 Ln1- 6 have been revised for clarity.

Ln7: why is the integration for the named objective necessary? Others have used periodic updates of the glacier extent.

See our response on the disadvantages of periodic updates of the glacier extents.

The added value of dynamic coupling should be emphasized in the entire ms.

This is now briefly discussed in the introduction section. Pg 5016, 5017.

Ln16: please give full name of the model and only thereafter abbreviations.

Corrected.

Pg5019, Eq1-4: define index x,y

Corrected.

Ln15: m is an "empirical" exponent

Corrected.

Pg5020, Ln17: if Jarosch et al. (2013) provided a more robust method, why not use it?

Pg 5020 Lines 16 – 17 are revised as follows:

Our scheme exploits flux limiters by upwinding ice thickness H in Eq. (4)
where as the scheme proposed by Jarosch et al. (2013) applies upwinding to both H and $r_{xy}S$
and is somewhat more robust than ours in its handling of ice flux but it uses an explicit
numerical scheme that in certain situations demands unacceptably small time steps to
maintain stability.

Pg5022, Ln2: this relies on the assumption that the glaciers are in steady state; however, the
entire study discusses the dynamics of the glaciers; so an argument should be listed why this
approach is still valid.

We do not fully understand this comment and think it is based on a misunderstanding.
Equation 6 simply describes how we treat the situation when surface melting reduces the ice
thickness to zero. We do not allow the ice surface elevation to be lower than the bed
elevation.

Pg5022, Ln1, Eq6: define index i,j,t (also at pg 5019, Ln7)

Corrected.

Pg5024, Ln 29: Structure: move description of Fig 4f to the description of the other panels of
Fig 4.

Corrected.

Pg5025, Ln1-6: why is it realistic to assume that glaciers are steady state under a given
climate? I understand that you used this to obtain glacier thickness, but the assumption should
be somehow justified.

Our first objective was to start the hydrological modeling with an initial areal distribution of
glacier ice that closely approximated the observed distribution; our second objective was to
start from a state that was mechanically consistent so that initial transient adjustments were
avoided. A steady-state initial condition is the simplest assumption that meets these
requirements. Starting the model from an out-of-balance but mechanically acceptable state
would be justified if we had access to reliable quantitative mass balance records but would
have a negligible effect on the hydrological results.

Pg5026, Ln2: Fig 6a and b should come before 6c

Corrected.

Pg5026, Ln19: calibration and evaluation is rather method, accordingly it should not be in the study site chapter

The calibration and evaluation are now moved to "methods" section.

Pg5027, Ln2: We also used MODIS data to calibrate our models (see Finger et al. 2011), so I am absolutely in favor of this. Nevertheless, the MODIS data are not mentioned again. What happened to the MODIS data?

We did compare with MODIS snow cover data and in the revised manuscript, we now mention these comparisons, but state that these are not included in the results because of questions as to the accuracy of the MODIS algorithms (especially having to do with the relatively coarse spatial resolution, and questions as to the accuracy of treatment of partial cover within a grid cell (see Ritgger et al., 2013).

Rittger, K., Painter, T. H., & Dozier, J. (2013). Assessment of methods for mapping snow cover from MODIS. Advances in Water Resources, 51, 367-380.

Ln12-17: the calibration procedure needs more details: how was each optimum found? How were the different observational data sets weighted? Or was only the Nash-value optimized?

See our response above to comment #4.

Ln18: How about the melt parameters? Did they not need to be calibrated?

The snow and glacier melt is calculated using full energy and mass balance approach and does not require calibration.

Pg5029, Ln14: a NS value of 0.7 may seem low for a glaciated catchment (see comments of reviewer 1), however, given that the model reproduces all other observational datasets well, a NS value of 0.7 seems adequate. We discuss this issue in Finger et al. (2011).

See our response to Reviewer #1 comment #5.

Ln16: hydrographs are illustrated in Fig 11! As this is mentioned here for the first time I suggest switching Fig 10 and 11. (see also comments above).

Corrected.

Pg5030, Ln 15-19: can you give an estimate of the model uncertainty regarding the glacier contribution? What does this imply for the water availability in the downstream dry areas?

In the revised manuscript, the uncertainty in the glacier contribution is now partially assessed by running the model with observed glacier extents.

Pg5031, Ln9-12: this is what we expect, glaciers must have a significant effect on runoff. But it would be very interesting to compare a static update of the glacier extent with the dynamic coupling? This would provide new insights to modeling effects of glacier retreat on runoff.

See our responses to Reviewer #1 comment #2 and #3.

Pg5032, Ln9-11: Could you estimate how this uncertainty affects uncertainty on glacier contribution to stream flow?

The uncertainty in the mass balance field from using degree day factor of 3.9 mmd^{-1} ∘C^{-1} in the glacier model spin-up run was somehow removed by tuning the mass balance field for the spin-up run to estimate a reasonable representation of the glacier extent in the study area.

Ln22-25: This is indeed very valuable and an important input to the ongoing discussion of glacier contribution to stream flow.

Thank you for your encouragement.

Pg5033, Ln 7: better than what?

This statement is somewhat vague, and has been removed in the revised manuscript.

Ln13: average over what period

The sentence is revised to include the time period.

Point2: What is the trend of precipitation? Can you quantify this hypothesis?

The trend value is given in Table 3.

Point3: This is a very interesting point, which should be linked to downstream socio-ecological impacts. What does this mean for the dry downstream areas?

We now comment on this briefly in the revised manuscript.

Comments to the Figures: Figure 1: a mixture of proceeding steps, data sets, model results and images are presented in a flow chart. This should be made consistent. Suggestion: put only datasets or model products in text boxes, label arrow with proceeding steps; the final results should be at the bottom, not in the center of the figure.

We now have simplified the Figure 1 in the revised manuscript.

Figure 2: make details on the map visible also in black and white printouts. Include river network in the figure.

Corrected.

Figure 3: Figure 3 and 10 present the same data; one is redundant. (Figure 10 and 11 are wrongly labeled, see comment above)

Figure 3 is now removed in the revised manuscript. The labels have been corrected for Figures 10 and 11.

Figure 4: include in all panels the river network.

Corrected.

Figure 5: I would not illustrate mass balances on areas outside the glacier extent; this is confusing; also include key location presented in Fig 1, this helps the reader recognize the study site.

The dynamic model spin-up scenario is run on the entire domain, not just the glacier areas. This is why the entire mass balance forcing field is shown. This is now clarified in the manuscript. The key location has been updated.

Figure 6: add "w.eq" to the units. I suggest using the same extent in figure 5 and 6.

Corrected

Figure 7: a) % of what? Total watershed? b) add "w.eq" to units

Corrected

Figure 8: a) change the scale to = to 650 as SWE never exceeds 650.

Corrected

Figure 10: should be Figure 11 (see comment above)

Corrected

Figure 11: should be Figure 10 (see comment above)

Corrected

Figure 12: include observed flow in plot b and c.

Corrected

Recommended references:
Finger, D., F. Pellicciotti, M. Konz, S. Rimkus, and P. Burlando (2011), The value of glacier mass balance, satellite snow cover images, and hourly discharge for improving the performance of a physically based distributed hydrological model, Water Resources Research, 47, doi: W07519, 10.1029/2010wr009824.

Finger, D., G. Heinrich, A. Gobiet, and A. Bauder (2012), Projections of future water resources and their uncertainty in a glacierized catchment in the Swiss Alps and the subsequent effects on hydropower production during the 21st century, Water Resources Research, 48, doi: 10.1029/2011wr010733, W02521.

Gardner, A. S., G. Moholdt, J. G. Cogley, B. Wouters, A. A. Arendt, J. Wahr, E. Berthier,

R. Hock, W. T. Pfeffer, G. Kaser, S. R. M. Ligtenberg, T. Bolch, M. J. Sharp, J. O. Hagen,

M. R. van den Broeke, and F. Paul (2013), A reconciled estimate of glacier contributions to sea level rise: 2003 to 2009, Science (New York, N.Y.), 340(6134)