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

This timely paper potentially makes an important contribution to describing how glacier retreat and its variability are reflected in the glacier-fed streamflows and could be of a great interest to both glacio-hydrologists and water resources' planners and managers. However the modelling approach lacks clarity and this raises important questions that need to be answered before it should be published.

Some suggestions are presented to the authors for improving the paper.

General Feedback

1. The paper is mainly focused on the contribution of glacier melt to the streamflow. This has been presented in three components (based on the conclusion of the paper), average glacier melt contribution to the streamflow during summer, the contributions during warm years, and those during cold years. The glacier melt contributions during warm and cold years were presented considering the years - 1998 and 1999. 1998 is the warmest year and 1999 is the coldest year. Though the authors have mentioned 1998 as a dry year in the conclusion, this has not been discussed in the analysis. From the Fig 12, it is clear that 1999 is a wet year, at least compared to the year 1998 and compared to the average flow for the period from 1980 to 2007. According to Adam et al. (2009), the hydrological responses to changing climate in snow-dominated basins had significantly different sensitivities to precipitation and temperature changes. The study could be further strengthened with an additional analysis how glacier melt contributes to the streamflow in wet and dry years along with warm and cold years. The authors are also suggested to review the paper by Immerzeel et al. (2011), who studied the hydrological response to changing climate in Himalayas using an integrated cryospheric hydrological model. They combined glacier dynamism and hydrological processes to study the impact future changes of temperature and precipitation on glacier evolution and hydrological processes.

2. The authors should better define the contribution of the glacier-melt to streamflow and suggest how this could be verified/tested. The paper provided the contribution from glacierized area to the streamflow comparing two model outputs, with and without glacier. How does this compare to estimates of glacier wastage contributions to runoff, with wastage as defined by the recent US NRC report on Himalayan Glaciers? It is important to confirm that the model simulates a realistic alpine hydrology even when glacier coverage is removed.

3. The contributing runoff area of the glacier has been considered as 9% of the basin area. The same glacier will make less contribution in the streamflow when the basin area is more, and vice versa. However, the size of glacier also controls seasonal variation of melt-runoff (Hannah & Gurnell, 2001). Therefore, the contribution of glacier melt in runoff from a basin should be presented with the ratio of glacierized area to the total basin area along with the size of the glaciers.

4. The authors admitted the weakness of the model while capturing the daily variations of streamflow. The NSE for daily simulation is significantly lower than that for monthly simulations. Though the referees have indicated both monthly and daily NSE values are lower than expected for this study, the very low performance of the model could be improved by considering several significant hydrological processes that have been well investigated in the region but are not well represented (or at all) in the model. For example, redistribution and sublimation of SWE by blowing snow, albedo decay and variation in snow cover area during snowmelt period, could be considered for temporal and spatial variation of snow accumulation and ablation (Pomeroy et al., 1998). Sublimation losses from blowing snow in the region have been shown to be important to the alpine mass balance (MacDonald et al., 2010). Similarly, snow melt contributing area could have some influence on making more robust conclusion (DeBeer et al., 2010). Could the daily (and monthly) simulations be improved if these processes are considered? What about snow interception and sub-canopy snowmelt in the significant forested part of the basin (Ellis et al., 2010). What about variation in both long and shortwave radiation to slopes and the effects of remote terrain shading (DeBeer and Pomeroy, 2010; Marsh et al., 2012). The authors have adjusted albedo decay in the model (page 5027, lines 23-24). But, there is not clear indication of blowing snow considerations. The authors have emphasized that the majority of precipitation in the basin occurs in the form of snow, mainly in the higher altitudes of the basin. Therefore, it could improve the model performance if the above mentioned phases of snow transformation are considered in the coupled model.

5. Similarly, water flows are controlled by some other factors in the cold region, one of them being the infiltration into frozen soils (Gray et al., 2001). Was this represented in the model? What about river ice breakup? Groundwater dynamics?

6. The other important factor could be the meltwater flow in the glacier. The paper has not described how water is routed in the glacier. The contribution of glacier melt to the streamflow was presented as the difference between simulated flow with and without glacier. The delayed meltwater flow within a glacier along with surface melt rate variation is important for modeling outflow hydrograph from a glacierized basin (Arnold et al., 1998; Fountain & Walder, 1998; Munro, 2011). Therefore, modelling of meltwater

and rainwater flow via supraglacial, englacial, and subglacial could improve the model performance capturing the daily variation of streamflow. Agreeing with the reviewers, the detailed description of the hydrological model and parameters used are needed. The specific focus should be on how the glacier (as a landcover) was dealt in the model and how meltwater or precipitation (rain) is routed through the glacier.

7. Mean annual temperature increase is 0.8^{o}C per decade (Table 3), which is a high rate. It would be interesting to see how other temperature indexes are changing, for example, mean maximum and minimum, seasonal averages. The authors have found that there are not significant trends in annual and summer streamflow. They have attributed the downward trend in the annual and summer flows to glacier retreat and precipitation decrease. What about changes in precipitation phase along with the changes in evapotranspiration? They concluded that the glacier in the study site is still in a phase of increased summer flow contribution. This is presented as the contradiction to the previous studies as cited in this paper (page 5015). Could the higher rate of temperature increase be directly attributed to increasing melting rate of snow and glacier?

8. Finally, though the authors have claimed a comparison of model outputs between the glacier as static and dynamic states, there is not any analysis of this comparison in this paper. The paper is merely limited to a comparison of the model performances between model outputs with glacier (glacier evolution has been considered) and that without glacier. There is not any comparison how the model behaves differently while considering the glacier as static or dynamic.

Specific comments/observations

1. The net annual mass balance (Fig. 5) depicts the negative mass balance for the last three decades throughout the study area. The dominating factor for the net mass balance distribution is elevation of the basin rather than landuse, higher the elevations lower the negative mass balance. Any justification?

2. Modelled specific mass balance were under-estimating all the values when it is over -1000 m w.e.yr^{-1}, and over-estimating when the observed values were less than -1000 m w.e.yr^{-1}. Moreover, the positive mass balances (1996 and 2000) could not be signalled by the model outcomes. Though the average annual mass balance for the Peyto Glacier is well simulated (page 5029, lines 3-5), the annual variability could not be comparatively simulated well (Fig. 9b). The model output agreed best for the mass balance values for years 1990 and 1991. Those values were filled in with the average value calculated from the study period.

3. The precipitation variability with altitude has been considered as double the value measured at the climate station as stated in page 5022, section 3.1: ----- The mean annual precipitation at the Lake Louise weather station is about 600mm but is thought to be as high as 1200mm at higher elevations, mostly in the form of snow.---- The evidence for this consideration has not been presented. What backs up the precipitation gradient and what seasonal considerations were made to correct for it?

4. There is a confusing cross-referencing in the following statement (page 5024, lines 13-15): ---- To run the model at 3-hourly time step, the daily 1km NARR precipitation data extracted at the location of Lake Louise station were temporally disaggregated by equally 15 apportioning days to 3-hourly intervals (Fig. 2). ---- Fig. 2 provides the location of the climatic station, not the downscaled precipitation data. Fig. 2 should be cross-referenced to the earlier sentence in the paragraph (lines 6-8). ---- Climate daily data such as minimum, maximum, wind speed and precipitation are available at the Lake Louise station (elevation: 1524 m) for time period of 1915–2007.------

Tables and Figures:

The presentation of the tables and the figures in the paper could be further improved with the following considerations:

1. Table A2 can be removed. The dates for the Landsat images acquisition are clearly presented along with glacier extend in the Fig. 3. The cross-reference for the dates of acquisition of the images is also referred to Fig. 3 [see the figure description in Fig. 10].

2. The snow accumulation and melt model presentation in the Fig. 1 (c) could be significantly improved. It can either be enlarged with indexing the figure or presenting the model part of the figure separately as a new figure.

3. Latitudes and longitudes of the Fig. 2 should be presented with one decimal point. The scale of the figure has the value '01.53`, which needs to be corrected. Moreover, the locations of the glaciers (Bow and Peyto) could be marked in this figure.

4. The DEM classification index in the Fig. 4 (b) needs to be fixed; the bottom part of the index is covered by the Fig 4 (d).

5. Fig. 8 needs improvement with an enlarged display. The chart size should be enhanced by at least 1.5 times more than its current size (particularly the y-axis). Fig. 9 and Fig. 11 should also be enlarged in the similar way.

6. The units of Y-axis in Fig. 11 (a) and (b) could be made consistent by fixing the maximum discharge values to 60 m^3/s.

7. Unit for discharge could be made consistent with either [m^3/s] or [m^3/sec] (Fig. 11 and 12).

Type errors in the text

1. Page 5029, line 16; ‘Fig. 10’ should be replaced by ‘Fig. 11’.

2. Page 5029, line 25; ‘Fig. 11’ should be replaced by ‘Fig. 10’.

3. Page 5030, line 8; ‘form’ should be replaced by ‘from’.

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