Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-260-AC1, 2018

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Multi-scale temporal variability in meltwater contributions in a tropical glacierized watershed" *by* Leila Saberi et al.

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Received and published: 1 November 2018

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1 Response to Reviewer 1

We would like to thank the reviewer for their time to review our paper. In this response, we have addressed all the reviewer's comments by providing clarifications and indicating how we will edit the manuscript. The reviewer's comments are copied here with italic font style.

This paper presents a detailed multi-method assessment of glacier melt and groundwater contribution to runoff for a small catchment in the tropics. The authors find significant contributions of melting to overall runoff using tracer studies, time series analysis and hydrological modelling. They also show that melt water can be a substantial contributor to groundwater discharge. This is an excellent study that presents a thorough analysis of field data and modelling leading to interesting conclusions. The manuscript is very well written, and methods and results are clearly described. The findings are also nicely presented. Overall, I absolutely recommend this paper for publication in HESS after some – mostly minor – issues have been resolved (see below).

We are encouraged by the reviewer's positive comments and will carefully address all issues raised.

More substantive comments:

Page 5, line 11: Is there an estimate how important glacier-derived runoff is for the larger catchment? A high importance (irrigation system) is implied here, but how does the glacier runoff volume relate to larger-scale effective precipitation? Given that the absolute runoff amounts in the Gavilan Machay basin are really small (in the order of 0.1 m^3/s) I doubt that this water (despite of originating from the headwaters) has a major significance lower downstream. This is also supported by the statement of page 12, line 6. The glaciers' importance for water resources in the region might need to be better put into context.

When extended downgradient to the Boca Toma diversion point, our mixing model analysis with HBCM predicts that surficicial runoff of meltwater contributes a range of 4-15% of the discharge to the irrigation system over 2012-2017, with the rest supplied by groundwater. While this melt contribution indeed seems to comprise a minor proportion, earlier investigation by La Frenierre (2014) on downstream water usage showed that farming communities cannot afford to lose any of the water; already, the irrigation system consistently fails to deliver its current allocations. Furthermore, if groundwater at Gavilan Machay also contains meltwater, as our simulations suggest, the actual total amount of meltwater contribution could be even higher than the 4-15% estimated for surficial runoff of meltwater. We will add this discussion in our revised manuscript. A lack of model input data outside of the Gavilan Machay sub-catchment prevented further extension of the model to Boca Toma.

Reference:

La Frenierre, J., 2014, "Assessing the Hydrologic Implications of Glacier Recession and the Potential for Water Resources Vulnerabilities in Volcán Chimborazo, Ecuador", PhD Dissertation, Ohio State University, 2014.

Page 9, line 23: The authors use a model that computes snow melt based on the energy balance. It is surprising to me that they nevertheless decided to implement an empirical, strongly simplified model for ice melt. This seems to be an unnecessary and also unphysical combination of approaches. Later, it is stated that a temperature index model is the only feasible approach given the limited data availability. However, if data are available to force an energy balance model for snow, it should also be applicable to glacier ice (just having a different albedo and surface roughness). More argumentation is required here, and possibly more insight into the energy balance scheme of Flux-PIHM.

We do not use energy balance calculations for glacier melt for two reasons. First, energy balance calculations of glacier melt would have to be coupled with the other energy balance calculations already in the Flux-PIHM model (for snow-melt, ET, sensible heat flux, and ground heat flux) because of both its role in the partitioning of incoming net radiation and its effect on surface temperature. However, adding this to Flux-PIHM requires intensive source-code modifications that are beyond the scope of this study. Second, an alternative approach of an approximate, uncoupled energy balance calculation of glacier-melt would be complicated by the lack of radiation input measurements in the study watershed. We currently use GLDAS data with Flux-PIHM for its energy balance calculations, but there is substantial uncertainty in applying the coarse-scale GLDAS radiation values over the steep mountainous watershed. Because of these difficulties, we chose to invoke the simpler temperature-index model and focused on constraining glacier-melt amounts based on discharge observations at the watershed outlet. We note that using coarse-scale GLDAS does introduce uncertainty into the current Flux-PIHM energy balance calculations, including for snowmelt. However, even without partitioning some of the incoming radiation for glacier melt in the model, our simulated snowmelt is a relatively small contribution of the total meltwater (15%), suggesting that precipitation limitations may make snowmelt calculations less sensitive to uncertainties in radiation inputs. We will better explain our choice of the temperature index model in the revised manuscript and acknowledge the corresponding uncertainties.

Page 9, Eq. 1: Given that relatively large parts (those experiencing the highest melt rates) of the glaciers are covered by supraglacial debris, I wonder how the model distinguishes between ice melting over these regions in comparison to clean ice.

We reported measurements of a slower ablation rate (0.54 to 0.87 m/yr) for the insulated debris-covered ice compared to a faster rate (3.4 m/yr) for the clean ice (p. 14, Lines 23-25), which indeed support debris-dependent melt conditions. However, these were only a handful of ablation measurements over different time periods, which were not sufficient to constrain separate melt factors for debris-covered and clean ice. We thus elect to use an effective melt factor over all glacierized areas (below the equilibrium line altitude) to model the bulk rate.

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Discussion paper

Figure 4: In my print-out (but not in the online pdf version!) there are ugly black squares around panels c and d, mostly covering the axis text. Please carefully check the figure data. Obviously, these issues only arise for particular printer drivers but make the figure almost unreadable. The same observation has also been made for Figure 8 (black squares left of the glacier snout in all panels).

We will update these figures in the manuscript. Thank you for pointing this out.

Page 20, line 12: Tackling the problem using different complementary approaches is highly beneficial. However, after reading the results section I somewhat missed a synthesis (figure) of the findings from the three different methodologies. For example, Fig. 3 and Fig 5 c/d could be combined to permit a direct comparison of findings based on tracers and based on the hydrological model which might also be helpful in discussing drawbacks and potentials of the individual methods.

Thank you for the suggestion. We will update Fig. 5d (showing model results) by shading the interval of % Melt Contribution estimated with the mixing model (from Fig. 3a) in order to facilitate comparisons between methods. Adding the discharge estimates from the mixing model (from Fig. 3b) to Fig. 5c would likely make the plot too busy, since it already has 5 different lines. The discharge information is summarized in Fig. 5d, so we think that adding the mixing model results to Fig. 5d should suffice. We will also edit the caption/text to remind the reader that the results represented by the mixing model somewhat differ from that of the watershed model, because the mixing model results included 5 discrete sampling times that were distinct from the simulation period, and the mixing model does not consider meltwater in groundwater.

Additional detailed comments:

Page 2, line 11: normally, references are ordered with the year of appearance but not here.

Thank you for pointing this out. We will update the order of references in the manuscript.

Page 3, line 5: please shortly mention the physical reason (energy balance) why higher humidity leads to more ablation – this might not be immediately clear to the reader.

We will add the following explanation to the manuscript: "Harpold and Brooks (2018) showed that increasing humidity enhances ablation rate by increasing net longwave radiation and condensation."

Page 5, line 19: Are there observations of recent glacier retreat in this region? Just to round up the story.

We will revise the following sentences by adding the underlined explanation to page 5, line 1-4: "Records since 1980 indicate that, consistent with the rest of the tropical Andes, temperatures have warmed 0.11$^{\circ}C$/decade around Volcń Chimborazo (Vuille et al., 2008; La Frenierre and Mark, 2017). This likely caused a 21% reduction in ice surface area and 180m increase in the mean minimum elevation of clean ice between 1986 and 2013 (La Frenierre and Mark, 2017)".

Page 5, line 27: precipitation gradients were determined with stations at 3900 and 4500 m a.s.l., respectively. Will this elevation difference be enough to capture / estimate precipitation over the higher reaches of Chimborazo, i.e. between 5000 to 6200 m a.s.l.?

Previous research in a glacierized mountainous watershed by Wang et al. (2016) found that the elevation-precipitation relationship is piecewise linear, with precipitation increasing with altitude below the elevation of maximum precipitation (EMP) and decreasing with altitude above the EMP. Such results support our application of a negative linear lapse rate calculated from our two stations – both located in the lower part of the watershed – to the higher elevation portions of the watershed. However, we should and will explicitly acknowledge that this assumes the EMP to be located below our watershed, which could lead to errors in the precipitation if the EMP is actually within the watershed above the lowest weather station. We will point out the need for denser monitoring to better constrain the EMP and precipitation lapse rate.

Reference:

Wang, X., Sun, L., Zhang, Y., and Luo, Y. (2016). “Rationalization of altitudinal precipitation profiles in a data-scarce glacierized watershed simulation in the Karakoram.” Water, 8(5), 186.

Page 5, line 28: It is a drawback for melt model validation that the ablation stakes are only installed over a very limited elevation range (i.e. not permitting to capture elevation gradients in glacier melt), and – as it seems – only over the debris-covered parts of the glaciers. This should be stated.

The ablation stakes were installed in clean ice. We will make sure to explain this in the Methods section (it is currently only mentioned later in the Results section, p. 14 Line 24) and acknowledge that the ablation stakes do not represent debris-covered ice, which was too difficult to drill into. Recognizing the limited representation of the ablation stakes, we do not directly use their measurements in the model but instead only use them as a high-end point of comparison for our calibrated glacier-melt model. Later in Section 4.3.1 Calibration Results, we explain that our calibrated average glacier melt rate (below the equilibrium line altitude) is lower than the ablation stake measurements in faster-melting clean ice and higher than the mass balance measurements for the slower-melting debris-covered ice (p. 14, lines 21-25).

Figure 7: I like the analysis of the coherences and it allows interesting conclusions to be drawn. However, it would be helpful if the term “coherence” would be better introduced, making it clearer how it was computed and what it potentially shows.

We present results for magnitude squared coherence (MSC), which can be thought of as the square of the correlation (between 0 and 1) between two variables at a certain frequency. Thus, coherencies between precipitation and discharge and between temperature and discharge indicate how strongly each of the climatic signals relate to discharge at a certain time scale. Looking at different time scales helps to distinguish whether these relationships may occur through fast surficial processes or slower subsurface processes, and whether discharge is more sensitive to certain climate forcings at particular time frequencies. MSC is defined as:

$$C_{xy} = \frac{|S_{xy}(f)|^2}{S_{xx}(f)S_{yy}(f)} \quad (1)$$

where, $S_{xx}(f)$ and $S_{yy}(f)$ are auto-spectral densities of variables x and y, respectively, and $S_{xy}(f)$ is the cross spectral density of x and y. This explanation will be added to the manuscript.

Page 19, line 12: Highly interesting finding. In how far could these 18% meltwater contribution to groundwater runoff be generalized to other catchments (different sizes, geology etc.)? Have there been other studies coming up with similar estimates or is this the first time this has been quantified? May be something for the conclusion section.

Past studies have examined the overall role of groundwater in glacierized watersheds and have found it to contribute up to 80% of total stream discharge (Clow et al., 2003; Liu et al., 2004; Huth et al., 2004; Hood et al., 2006; Baraer et al., 2009; Andermann et al., 2012; Baraer et al., 2015; Pohl et al., 2015; Somers et al., 2016; Harrington et al., 2018). A smaller number of studies have also identified a component of meltwater in the groundwater (Favier et al., 2008; Lowry et al., 2010; Minaya, 2016; Baraer et al., 2015; Harrington et al., 2018), but to our knowledge, our work is the first to quantify this component. Generalizing our results to other glacierized watersheds depends on a number of geologic and climatic factors. The importance of meltwater contributions to streamflow through groundwater depends first on the presence of groundwater pathways. These typically are most prominent with the presence of fractures in young volcanic bedrock (Tague et al., 2008; Frisbee et al., 2011; Markovich et al., 2016) – like Chimborazo – and sometimes even crystalline bedrock (Tague et al., 2009; Andermann et al., 2012; Pohl et al., 2015). Morainic deposits (Favier et al., 2008, Minaya, 2016, Somers et al., 2016) and alpine meadow soils (Loheide et al., 2009; Lowry et al., 2010;

Gordon et al., 2015) have also proved to be effective groundwater units below glaciers and snowpacks. Even in settings that may have limited groundwater networks extending throughout the watershed, talus slopes can serve as localized areas of meltwater recharge (Clow et al., 2003; Baraer et al. 2015; Harrington et al., 2018). In the groundwater, the proportion of precipitation versus meltwater depends on watershed size and climate. Well-established discharge-watershed area relationships for non-glacierized watersheds (Dunne and Leopold, 1978) lead to predictions of increased precipitation contribution in larger watersheds (with similar glacierized areas). More arid settings may be expected to have a higher proportion of glacier-melt due to overall less precipitation inputs to the watershed, although our results indicate a possible interaction between glacial melt contributions and precipitation, where rainfall boosts melt contributions through both the transfer of heat to glaciers and through antecedent moisture conditions that facilitate meltwater recharge.

We thank the reviewer for this comment, which prompts us to better highlight our new contribution and its potential implications elsewhere.

New References (other references are in the original reference list for the manuscript):

Dunne, T., and Leopold, L. B. (1978). Water in environmental planning. Macmillan.

Frisbee, M. D., Phillips, F. M., Campbell, A. R., Liu, F., and Sanchez, S. A. (2011). “Streamflow generation in a large, alpine watershed in the southern Rocky Mountains of Colorado: Is streamflow generation simply the aggregation of hillslope runoff responses?.” Water Resources Research, 47(6).

Harrington, J. S., Mozil, A., Hayashi, M., and Bentley, L. R. (2018). “Groundwater flow and storage processes in an inactive rock glacier.” Hydrological Processes. Hood, J. L., Roy, J. W., and Hayashi, M. (2006). Importance of groundwater in the water balance of an alpine headwater lake. Geophysical Research Letters, 33(13).

Huth, A. K., Leydecker, A., Sickman, J. O., and Bales, R. C. (2004). “A twoâĂŘcomponent hydrograph separation for three highâĂŘelevation catchments in the Sierra Nevada, California." Hydrological Processes, 18(9), 1721-1733.

Pohl, E., Knoche, M., Gloaguen, R., Andermann, C., and Krause, P. (2015). "Sensitivity analysis and implications for surface processes from a hydrological modelling approach in the Gunt catchment, high Pamir Mountains." Earth Surface Dynamics, 3(3), 333-362.

Somers, L. D., Gordon, R. P., McKenzie, J. M., Lautz, L. K., Wigmore, O., Glose, A., ... and Condom, T. (2016). "Quantifying groundwater–surface water interactions in a proglacial valley, Cordillera Blanca, Peru." Hydrological Processes, 30(17), 2915-2929.

Page 22, line 22: I do not agree that runoff after glacier disappearance decreases by the current amount of melt contribution. As much as I understand, melt computed by the model includes both ice and snow melt. Whereas glacier ice melt is zero after the glacier has disappeared, snow melt is likely to remain a significant component of runoff or would be replaced by liquid precipitation in the case that the zero degree line remains above the top of Chimborazo all the time. Therefore, I would expect a significantly smaller runoff reduction for the catchment in the far future than implied here.

The reviewer's comment prompts us to make one clarification and also qualify our statement about the runoff change after the glaciers disappear. First, we clarify that the model scenario we called "No Melt" should have been called "No glacier melt", and the scenario we called "With melt" should have been called "With glacier melt" - we will correct this naming scheme in the manuscript. Both model scenarios include the same snowmelt amount, because they use the same meteorological inputs to Flux-PIHM. Flux-PIHM simulates snowmelt based on precipitation and temperature inputs, while glacier melt is simulated externally (using the temperature-index model) and then added as another water source to Flux-PIHM. Thus, our calculation of change between the two scenarios isolates the effect of having glacier melt versus no glacier melt.

Although by design our simulation scenarios aim to separate out glacier-melt and snowmelt contributions, we acknowledge that we lack constraints on their individual amounts, which means that our calibrated glacier-melt contribution could incorporate precipitation-sourced meltwater not fully accounted for in Flux-PIHM's snowmelt scheme. Further, mixing model estimates of meltwater contribution uses meltwater from the glacier tongue, which may include snowmelt and melt of freshly accumulated ice. Thus, we will qualify our statement about the decrease in future runoff post-glaciers (under the same precipitation conditions): we will say that the estimated amount of current meltwater provides an upper limit, and the reduction could be less depending on snowmelt.