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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #1

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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).

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 m3/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.

- 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.

- 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.

- 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).

- 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.

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

- 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.

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

- 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.?

- 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.

- 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.

- 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.

- 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.

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