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Discussions



# *Interactive comment on* "Modelling runoff from a Himalayan debris-covered glacier" *by* K. Fujita and A. Sakai

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The role of debris covered glaciers in the run-off generation of glacier covered drainage basins is an important scientific question. There exist many studies which investigated physical processes involved in ice melt underneath a supra-glacial debris cover. In order to calculate total melt in a drainage basin, based on limited input parameters, requires a more generalized approach. Many debris covered glaciers are situated in remote areas and measurements of debris thickness are extremely difficult, if not impossible, by ground based methods. Fujita and Sakai used a different approach to quantify the total run-off and separate the individual contributions in the drainage basin of the Tsho Rolpa glacial lake. Based on earlier ideas about debris cover mapping from



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space and the concept of thermal resistance they provide a detailed description of the individual hydrological components and the uncertainties involved. This manuscript is a valuable contribution to the ongoing efforts of evaluating the impact of supra-glacial debris cover.

The paper is well written and shows a strong insight of the authors with respect to debris covered glaciers. The results are well described and give new, instructive insights in the role of debris covered glaciers in the catchment hydrology. Still, there are some weaknesses which could be improved in order enhance the quality of the manuscript.

Structure: I feel that quite a large part of the analysis is presented in the Discussion part. This would be better transferred into the Results part (all the experiments for sensitivity).

Excess melt water: this expression is not clearly defined. This makes it sometimes difficult to assess the impact in the water balance. There are different definitions of excess melt water in the literature, e.g. the difference between melt on a clean glacier and the debris covered case, or excess with respect to balanced conditions. From a definition later in the manuscript it seems that excess is the additional water discharge compared with ice free terrain.

Temporal continuity of surface conditions: the authors claim that they implemented possible variations in surface conditions. This however, is only a very rough all or nothing approach which has already been applied by other authors (most recently e.g. Juen et al., The Cryosphere, 2014). There is no mechanism included which allows a realistic temporal evolution of the debris cover. Even though such comparisons are very instructive, it needs to be made clear that the results are just the two possible extremes.

Melt at the calving front: There is no mentioning of potential mass loss and enhanced melt at the front of Trambau glacier, which is in contact with Tsho Rolpa. Melt and ice berg calving (with subsequent melt in the lake) could be a considerable amount,



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strongly dependent on the water temperature. Was this calving cliff balance included in the sensitivity calculation in section 4.2?

Steep slope terrain (steeper than 30°) is classified as ice free: In a way this is a sensible classification, even though many of these slopes will be ice covered, but contribute to the glacier mass balance by avalanching. The reasoning behind this classification should be mentioned in the text.

A separate table of used variables would make it easier to follow the method development, because you could omit the units and the absolute values in the text.

Assumptions in defining the conductive heat flux: Even though researchers dealing with debris covered glaciers are well aware of these assumptions, there should be at least a short discussion about the consequences and limitations.

In my opinion it is unfortunate to assign "R" as a variable name to the radiative heat fluxes and the thermal resistance, because they are fundamentally different physical parameters.

P. 2447, l. 3f: This definition seems a bit too complicated. Maybe it is easier to define "All fluxes are positive towards the ground.", because G_d is also positive from the debris surface downward.

P. 2447, l. 4ff: This sentence is rather confusing. Probably turbulent heat fluxes are small at high altitudes because there is only limited mass flux in the low density air.

P. 2450, l. 4f: If you apply an iterative method, you need to specify you initial conditions, i.e. what temperature depth distribution to you start with?

P. 2459/2451: I do not understand the reasons behind the definition of the maximum water content and the bulk coefficient. There must be a reason why to use these numbers.

P. 2453, l. 4f: the albedo of glacier ice varies considerably in dependence of air humidity



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and wind. The range should at least be mentioned in the text.

P. 2453, bucket model: This is a usual way of defining the water balance of the ground storage. However, here the different parameters are just assumed, without any reasoning. Are these values based on experience in other water sheds, or derived from ensemble runs, or other sources?

P. 2455, l. 20f: This sentence is confusing.

P. 2456, Distributions of thermal resistance and albedo: The given uncertainty is actually the variability based on temporally distributed data. However, each of the variables involved (Eqn. 5) has a certain error range (the energy fluxes from the gridded data and the surface temperature from the remotely sensed raster values).

P. 2457/2458: It is surprising that the relative contribution of debris-free ice and ice-free terrain is approximately equal. The sentence at the end/beginning of page 2457/2458 is a bit confusing with respect to evaporation influencing the run-off height.

Section 4.1: This section only derives the uncertainties due to scatter and temporal variability. It does not include potential systematic errors in the input parameters (e.g. radiative fluxes, surface temperature). This should be stated clearly.

Section 4.2: This result implies that the mean thickness of the debris over the entire glacier tongue is still enhancing ice melt and is therefore rather small. Is there any explanation for that, given that the glacier tongue below the ice fall shows a complete and dense debris coverage?

P. 2461: The statement of temperature sensitivity is 23 times higher than precipitation sensitivity is a bit misleading. It should clearly be noted that this is just a comparison of natural variability under the current conditions. Referring to a different time span, this could already be very different.

P. 2462: It should be stated that for the long term effects of variations in precipitation, there is only a static glacier and a static debris cover involved in the estimate.



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