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Interactive comment on "Estimating the avalanche contribution to the mass balance of debris covered glaciers" *by* A. Banerjee and R. Shankar

Anonymous Referee #3

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General comments:

The comparison of glaciological mass balance and geodetic mass balance of the Hamtah Glacier over the last decade has revealed a large discrepancy. The topic of this paper is to assess the avalanche contribution in the accumulation zone which could be the cause of the underestimation of the glaciological mass balance. The authors use a flowline model to calculate the surface velocities and the snout fluctuations from surface mass balance. From this model, they adjust the surface mass balance in the accumulation zone to reproduce the observed surface velocities and snout fluctuations. The authors conclude that the glaciological mass balance has been underestimated (i.e., is too negative). More generally, they conclude that the accumulation area ratio is very low for the debris covered glaciers surrounded by high and steep head walls and thus

nourished by avalanches.

However, I believe this paper is not ready for publication and it suffers from numerous shortcomings:

1°) In this paper, the glaciological measurements are not described and it is difficult to know exactly how the result of these measurements can be biased. In addition, these glaciological data cannot been analyzed and criticized here given that the Geological Survey of India (GSI) reports or other restricted reports are not available for general scientific community. The only description of mass balance data is provided in Figure 2a. The authors should describe accurately the data (from acknowledgements it seems the authors contributed to the field measurements in 2013) and mention how and where the glaciological mass balance measurements have been performed. A detailed map with the ablation stakes and the drilling cores is necessary. This map should also show the limit of the debris covered surface area. The modelling experiments and analysis are based on front fluctuations over the last 50 years. I believe that these front fluctuations should be shown in a Figure given these data have not yet been published in a peer-reviewed journal. 2°) The authors use a very simplified ice-flow model to assess the surface mass balance in the accumulation zone and very few data are used to calibrate the model. The bedrock topography is unknown. The bedrock slope is assumed to be 0.1 without any justification. The choices of fd and fs are not justified. Many assumptions have been done. It is difficult to assess the performance of this model and the quantitative results of the model could be questioned. Consequently, in order to assess the relevance of the results, the authors should study the sensitivity of the modelling results to the assumptions and the uncertainties relative to the bedrock slope, the factors fs and fd, the initial thicknesses... 3°) The main conclusion of this paper, relative to the avalanche contribution assessment, is based on the assumption that the glacier is not very far from a steady state. To estimate the avalanche contribution, the authors stated that "the steady state length is likely to be about a km less than its present length" However, the analysis which supports this assessment (p. 648

, l.4-21) is very fragile: the authors use the equation 5.25 p.61, of Oerlemans (2001) to obtain the sensitivity of length to temperature. It is obviously a rough approximation. The authors should study the consequences of the large uncertainty of this approximation. Moreover, I do not understand why the authors do not study the climate sensitivity and the response time directly from their model.

4°) The methodology is unclear and can be questioned. Given that the bedrock topography is unknown, the initial thicknesses have been inferred from the observed velocities (coming from remote sensing observations) and Equation 2. Consequently, the initial calculated fluxes of the model result from observed ice flow velocities only. Providing that the obtained results are very sensitive to the ice fluxes, the authors should study the impact of the uncertainty relative to Equation 2 formulation.

5°) This paper highlights the importance to perform surface mass balance measurements over the entire surface area of the glaciers. As many other studies, the authors show that the glaciological measurements can be biased and should be systematically compared/calibrated with geodetic method. I believe the authors should stress these two points.

6°) The term “avalanche contribution” can be questioned given that the present study points out an over-accumulation of snow in the accumulation zone but the present study does not prove here that the snow accumulation comes from avalanche. It could come from wind redistribution. The text and the title should be changed. The main conclusion is that the measured glaciological mass balance is likely underestimated.

In many places, the explanations are confusing or missing

Specific comments :

p. 642, l. 1: why this restriction to debris covered glaciers?

p. 643, l.6 : some words are missing

p. 643, l.19: the authors should provide the % area covered with debris.

p. 643, l. 25-27: the authors should specify the measurements period.

p. 644, l. 3-12: it is not usual to find results in Introduction. I believe that this paragraph should be moved in Results or Conclusions

p.644, l.21: the name of the section “Hamtah glacier” is curious. I believed that it should be deleted and the section 2 should start directly with “Available data”

p. 645 l.1 -21: as said in general comments, the authors should describe properly the glaciological mass balance measurements and the snout fluctuations measurements. A map is needed. A figure with snout fluctuation is needed too. Moreover, it could be useful to know the temporal variability of surface mass balance.

p.645, l.10: the authors mention the measurements period relative to the retreat of 20 m/yr. The retreat is -125 m between 2000 and 2010, and -1.2 km between 1963 and 2010. Here it is not clear.

p. 645, l. 14: The value of the AAR0 provided in Wagnon et al., 2007 is 0.71. Citing an internal report is not necessary here, given that the cited report is unreachable.

p. 645, l. 15-16: the authors should mention the period of measurements, both for glaciological mass balance and geodetic mass balance.

p. 646, l. 18: it would be helpful to show the velocity map

p. 646, l. 18-20: the authors should give the uncertainties relative to the surface velocities measurements

p. 646, l.22: please, give the period on which mass balance have been extracted.

p. 646, l. 24: the authors assume a constant bedrock slope of 0.1. The authors should analyze the consequences of the uncertainties relative to this constant bedrock slope.

p. 647, l.2: fs anf fd are given without any reference

p. 647, l.4: are there some evidences that sliding is the dominant mechanism of flow ?

p. 647, l.17: how could the glacier upper part move below the ELA ?

p. 647, l.20: “model mass balance”: not clear, given that the authors said previously that the mass balance come from measurements.

p. 647, l.21: “this exercise shows..”: this conclusion is not surprising given that mass balance is positive only in the extreme upper part of the glacier (z=4650 m)

p. 648, l.2-3: the conclusion is not convincing. I do not believe that the presence of stagnant area proves that the glacier is unlikely to retreat more.

p. 648, l.2: “present climatic conditions” is vague.

p. 648, l.4-21: I do not understand why the sensitivity is not obtained from the model. Moreover, the authors assume here that accumulation does not influence the behavior of the glacier. They assume that air temperature only influences the mass balance, without any justification.

p. 648, l.11: the authors should mention which equation of Oerlemans (2001) they used ?

p. 648, l.14: the authors should explain exactly how they calculated 250 years.

p. 649, l.2-3: do the authors have any evidence that the main contribution of ice flow comes from the sliding velocity ?

p.649, l.8: what is the consequence of the assumption dh/dx=0 ?

p. 649, l.14-17: the conclusion is based on the assessment that the glacier would reach a steady state after a retreat of 1 km. The analysis which supports this assessment is not convincing (see general comments).

p. 649, l.22: “+/-0.1 “: the authors should explain how they obtained the uncertainty on the avalanche contribution

p.650, l. 1-2: From the previous explanations, I understand that the simulations are

been calibrated from (i) the retreat over the last decade (ii) an assumed steady state after a retreat of 1km. However, here, the authors mentioned a “modelled steady state” at the beginning of the simulation. It is very confusing.

p. 651, l. 7: references should be written properly.

References: Oerlemans 2001: pages 646-648 do not exist.

Figures: A map with debris covered part and mass balance measurements is needed

Figure 2: caption: Please mention the period of measurements

A figure with the snout fluctuations is needed

Figure 3: Please mention where is the top and the bottom of the glacier. We assume that 0km corresponds to the top. Does the modelled velocity profile at 0yr correspond to a steady state ? What is the cause of the sharp peak of velocity at 3.1 km ? Please explain the meaning of red vertical bars.

A figure with the modelled thicknesses is required.

Interactive comment on The Cryosphere Discuss., 8, 641, 2014.