Replies to the referee comment by Anonymous Reviewer#3

We thank the anonymous reviewer for his/her kind review of our manuscript. His/her specific comments (in blue) and our corresponding responses are given below.

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.

We have clarified our stand regarding the glaciological mass balance data in our initial response: "We have described all the available information regarding the glaciological mass balance data and that is all that has been used as model input (data source: Vincent et al, 2013 and the referred .gif image from GSI website). We would like to clarify that we have not participated in the mass balance measurement programme by GSI and do not have access to their raw data. While it would be better to have the details of the measurement available for scrutiny, we work with whatever is available. We believe that any major flaw in the glaciological mass balance data used would have shown up as inconsistencies with other available data (e.g. velocity profile, recent retreat rates, and geodetic mass balance data) even in the simplified model simulations. Our work also provides a possible solution to the issue of reported mismatch with geodetic data of Vincent et al (2013), who discusses the GSI data on net balance of Hamtah glacier in some detail."

Regarding snout fluctuation data, we have just used the total retreat during the period 1960-2010 and the average retreat rate in 2000-2010 which have been clearly specified. We also cite relevant reference where the data is taken from.

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 ...
We have clarified each of the above issues in detail in our initial response and also provided the details of our modeling procedure and sensitivity tests performed. These information would be provided as supplementary material to the revised manuscript. We believe these details clearly demonstrate the robustness of our model results.

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.
These issues have been answered in detail in sect. 4.2.1-2 of our initial response document. We are aware of possible large uncertainty in the estimated net retreat to the steady state corresponding to present mass balance, and this has been taken into account in estimating the uncertainties in our calculated avalanche strength. The relevant information would be provided in the revised manuscript and additional supplementary material.
The lack of knowledge of past steady state profile and long term trends of mass balance profile prevents us from using the flowline model to estimate the climate sensitivity.

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.
The effect of various choices of f_s, f_d and initial thickness profiles have been discussed in detail in our initial response (sect. 4.2) and it has been shown that our results are robust and does not involve any undesirable fine tuning of

the parameters of the model. We have also clarified the basis for the choice of bedrock geometry in the document.

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.

In case of strongly avalanche fed glaciers, mass balance measurements over the entire surface area of the glaciers are not feasible because of hazardous field conditions. In such situations systematic inconsistency between glaciological and geodetic mass balance may show up. In this discussion paper we have shown for the first time that this can be remedied by estimation of the avalanche strength through flow line modeling. Here we are not trying to reestablish the well accepted need for systematic comparison of glaciological and geodetic mass balance measurements as pointed out by the reviewer.

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.

As we have argued in our initial response document, the estimated "over-acumulation" is mainly due to extensive avalanche activities in Hamtah glacier. This claim is supported by 1) the presence of a high, wide, steep and large ice-free headwall, 2) the presence of huge avalanche cones, and 3) the extensive debris cover. Another pointer to dominant avalanche activity is the relatively low (about 4600m) ELA of the glacier. Therefore we do not agree that either our main conclusion or the title needs to be modified. Though we agree that wind redistribution, statistical fluctuations contribution etc may be contributing to some extent as well.

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

Causal association between high headwall, strong avalanche activity, and presence of debris cover is well established in the literature.

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

The sentence would be reworded in the revised draft.

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

The information is given in p. 645, l. 3.

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

The information would be provided in the revised draft.

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
Appropriate changes would be made in the revised manuscript.

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"
We would rename the section.

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.
The issue with glaciological mass balance measurement has already been discussed and our point of view made clear in the initial response document.
Snout fluctuation data and temporal variability of net mass balance data are available in the cited references.

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.
As is clearly mentioned in the draft, average retreat reported retreat rate for the period 1960 to 2010 is 20m/yr and the recent retreat rate is 16m/yr. Our model state has a recent retreat rate of 12.5m/yr.

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.
The report is freely available online.
(http://www.serb.gov.in/pdfs/Publications/Chhota-Shigrii.pdf)

p. 645, l. 15-16: the authors should mention the period of measurements, both for glaciological mass balance and geodetic mass balance. The information of periods of measurements would be included in the revised draft.

p. 646, l. 18: it would be helpful to show the velocity map
We have provided a plot of velocity profile along the flowline (Fig. 3) that has been used in our modeling.

p. 646, l. 18-20: the authors should give the uncertainties relative to the surface velocities measurements
It was shown as vertical bar in Fig.3 of the draft. It would be mentioned in the figure caption.

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

The information would be provided in the revised draft.

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.
Rationale behind this choice of bedrock slope has been discussed in the initial response (section 4.2.3) and would be included in the supplementary material.

p. 647, l.2: fs and fd are given without any reference
The choice of fs and fd has been discussed in detail in the initial response (section 4.2.4) and would be included in the revised draft/supplementary material.

p. 647, l.4: are there some evidences that sliding is the dominant mechanism of flow ?
No direct estimation of fs, fd is available for Hamtah glacier. We use them as free tuning parameters. But our result is robust against large variation of fs and fd as has been discussed in the initial response (section 4.2.4). This would be clarified in the revised draft and details would be provided in the supplementary material.

p. 647, l.17: how could the glacier upper part move below the ELA ?
Such a situation may arise if top of the bedrock is lower than ELA as is the case for our model of Hamtah. For example similar situation is discussed by Oerlemans (2001).

p. 647, l.20: "model mass balance": not clear, given that the authors said previously that the mass balance come from measurements.
The model mass balance is a piecewise linear approximation of the GSI data as depicted if figure 3 of our draft.

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)
We agree with reviewer's comment.

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.
We have included the possibility of a large variation (70%) of this possible retreat magnitude to take care of the uncertainty of our estimation of steady state length.

p. 648, l.2: "present climatic conditions" is vague.
We mean to imply present or recent mass balance conditions. It would be clarified in the revised draft.

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.

As is clarified in the initial response document (sect. 4.1.2), "no information on any past steady state thickness profile and subsequent time dependent mass balance profiles of Hamtah are available. This prevents us from modelling the detailed retreat data and from using the model to determine of climate sensitivity of the glacier".

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

This information would be provided in the revised draft.

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

As mentioned in the draft "Its response time is expected to be of the order of the ratio of its length to the typical velocity magnitude (Oerlemans, 2001) i.e. 250 yr". Here the typical velocity taken to be 20m/yr and length of the glacier is about 5 km.

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

This has already been discussed above.

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

This simplification is justified because average value of dh/dx, i.e. the ratio of ice thickness to the length of the glacier, is 100/5000, which is small compared to average bedrock slope. This approximation is analogous to that of ignoring the ice thickness feedback.

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).

As has been discussed before we remedy this by allowing a wide variation (70%) of the possible net retreat.

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

This has been discussed in our initial response and would be discussed in the revised draft/supplementary material. Basically the uncertainty is coming from our lack of knowledge of the steady state.

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.

The typo would be corrected.

p. 651, l. 7: references should be written properly. References: Oerlemans 2001: pages 646-648 do not exist.

These are hyperlink to the appropriate pages in the draft as per TCD styles, and are not pages of the book referred to.

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

As we have clarified above, map of details of stakes and pits are not available with us.

Figure 2: caption: Please mention the period of measurements

Corrections would be made.

A figure with the snout fluctuations is needed

Since we do not use the details of snout fluctuation data, and since this is already available in the referred literature (Pandey et al. (2010)), we choose not to include the figure here.

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.

Relevant information would be provided in the caption.

The time label 0yr correspond to the current state inferred from velocity profile data. The steady state corresponding to present mass balance is similar to the state after 300 yrs.

The sharp peak is caused by narrowing of the channel at around 3.1 km mark as shown in fig. 2. The sharpness is an artifact of our approximate piecewise linear width distribution profile. In the measured velocity profile, the peak is visible as a more diffuse one.

A figure with the modelled thicknesses is required.

It has been provided in the initial response document and would be included in the supplementary material.