Replies to comments of `Reviewer 2`

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1. *While the introduction of the latter terms appears a novel way to describe glacier advance and retreat, I am not quite convinced the paper advances our understanding and/or comes up with a usable model for the right reason. I believe are theoretical misconceptions on behalf of the authors, and partly because of a clumsy explanation in parts.*

   The conclusion that a steep glacier is likely to retreat less in horizontal distance than a glacier with a low slope for the same environmental forcing is in fact trivial, as acknowledged by the authors. Others have shown that under very schematic conditions (a slab of ice of equal width and uniform thickness, and a constant balance gradient), an expression for equilibrium glacier length can be derived in terms of glacier altitude above ELA, bed slope, and glacier thickness (Oerlemans, 2008). In fact, this idealized model is even simplied further in this manuscript before it is applied to a set of Himalayan glaciers with very different characteristics. The authors ought to look back to verify whether their assumptions still make for a valid model.

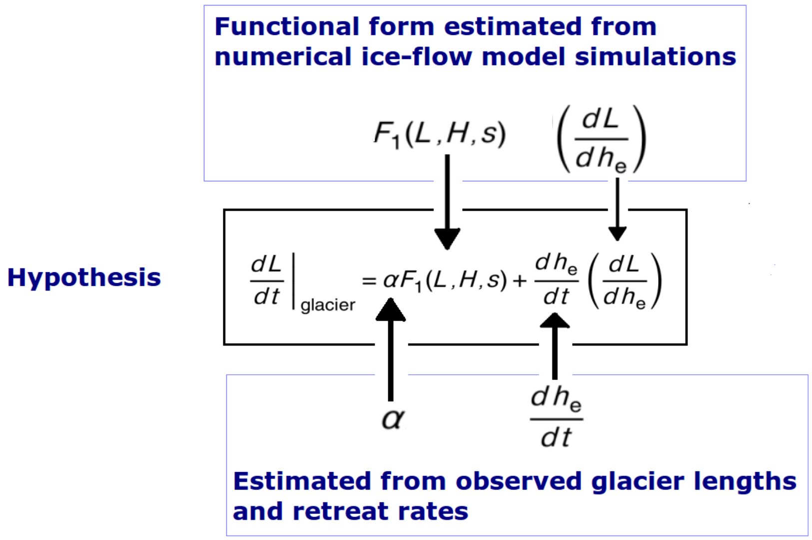
   We thank the reviewer for a very detailed review and the large number of comments. These comments are probably due to viewing our paper from a detailed ice-dynamics modeling perspective.

   The approach we have taken in the paper is as follows. We have used two different models of varying degrees of approximation for studying glaciers. They are

   (a) A numerical ice-flow model
   Which is valid on small time-scales (annual) and where the area-altitude distribution is taken into account and

   (b) An empirical model proposed for the overall advance/retreat wherein two factors are considered.
   This model is for the longer time-scale and the hypothesis is that gross geometric and climatic parameters such as mean slope and rate of change of ELA determine the overall dynamics.

   Detailed simulations with the ice-flow model under idealized conditions are used to determine the functional form of the empirical model which is expected to mimic the main processes. The coefficients which multiply the terms are then determined by using observations for a set of glaciers. In this way, the functional form obtained from the idealized simulations are related to the real world. To make the approach clear, a schematic view, is shown in the figure below. The empirical model is then used to predict the retreat rates for other glaciers. We believe that the ability of the model to explain many Himalayan glaciers supports the validity of our approach.



We are aware of the limitations in the empirical model. Many aspects of the problem, such as role of debris cover, variation of slope along the length of the glacier, variation of width etc are not included in the model. The premise is that some of these effects are indirectly accounted for, when the empirical constants are estimated from observations. This is essentially an engineering/empirical approach. We have tried to see how much insight can be gained even with these assumptions.

Additional text and a diagram will be added to the revised manuscript, to make these points clearer.

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2. *1. The theoretical development of their model ignores the area-elevation distribution of the glacier by considering glaciers of equal width. This is an important (over)simplication and is insufciently acknowledged/ discussed when the model is applied to real glaciers.*

The area-elevation distribution is crucial while doing numerical ice-flow modelling. We consider this to be of a secondary effect on the large-scale (for the empirical model).

This aspect will be further emphasized in the revised manuscript.

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3. *2. p. 2574, line 20. Shear stress in fact depends on surface slope and ice thickness, not on bed slope as erroneously stated; basal slip and sliding are the same, the authors probably mean basal sliding and internal deformation.*

In a numerical ice-flow model the shear stress depends on the surface slope. In the context of our empirical model, we consider the only the external factors such as mean slope to play a role. The ice-thickness is in response to the overall climatic conditions and the geometry.

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4. *3. p. 2575. I assume the authors mean by negligible mass balance a zero mass balance. That is indeed a fully hypothetical situation because it would not allow to form a glacier in the first place.*

   This fully hypothetical situation of zero mass balance, is one of the new ideas in the paper. The tendency of a block of ice to deform and the resulting frontal velocity is estimated under these conditions. In our view, this is a novel way of understanding the functional form of one of the aspects of the problem. It is not our contention that a glacier would form under these conditions.

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5. *4. I am not sure the authors really understand the full implications of eq. 1. It states that a stationary glacier is in dynamical equilibrium when on all places on the glacier there is a balance between the specific balance and the divergence of the horizontal ice volume flux. A glacier front advances because the (negative) flux divergence exceeds the ablation. In my understanding, this is not directly linked to glacier length or mean slope as used in the authors model.*

   The implications of eq. 1 for a dynamic equilibrium are clear. If one is doing simulations with an ice-flow model, the length is determined from the calculations. The initial conditions in this case are the distributions of ice-thickness with distance and the detailed height - area distributions. For the empirical model however, the glacier length is representative of the state of the glacier and the mean slope represents the geometry.

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6. *5. p. 2575, lines 12-13. In my conception, ice flow is determined by surface slope and ice thickness, not by mean slope and length. A longer glacier may flow more vigorously because it is thicker and more ice needs to be transported for the same balance gradient, but glacier length by itself does not control ice ow. Even when a glacier front retreats, there may still be sliding and ice deformation at the tongue, contrary to what this sentence seems to imply.*

   The comment is valid if one is doing simulations with an ice-flow model. In such modelling, details of the processes at the glacier front such as sliding and ice deformation are considered. In the context of our empirical model however, it is only the gross advance/retreat which is calculated. We propose that the front behaviour on a climatic time-scale can be looked at in this way.

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7. *6. Section 4.1. To derive the function F1 the authors look at the rate of frontal advance after 1 year in a numerical experiment in which a block of ice of variable length and thickness starts to flow for a given bed slope. Frankly, from my experience with numerical ice flow modeling this seems rather nonsensical. The authors are probably*

discovering the artefacts of the numerical scheme at a glacier margin rather than anything else. It is unclear to me (in a physical sense) how glacier length can have an influence on how the glacier front initially responds.

We find that the front velocities derived for a block of ice *with zero mass balance* are quite robust and not numerical artefacts. The main reason is that by considering a constant slope bed and zero mass-balance, sources of noise (irregular geometry, variations in the source terms) are eliminated. Also, a predictor-corrector scheme which is formally second-order accurate has been used.

The reviewers experience is probably with modelling of real glaciers, where, the bed-slope and fluctuating mass-balance makes the front movement fluctuate on a short time-scale.

The way in which the initial length of the block of ice determines the subsequent front behaviour is shown in figures 4, 5 and 6. The physical basis is as follows: thickness of the ice-block causes the movement and that there is a correlation between the initial length and thickness. Since length is easier to measure and available for a large number of glaciers, we have used it.

As pointed out later, references to figure 5 and 6 are missing in this section. References to these figures will be added.

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8. *7. Section 4.2. p. 2580, line 17. I dont agree that the term 2Hm/s is small for large L. From first principles (Nye-Vialov solution) Hm depends on the root of L and therefore is always important. In fact, this relation is restated by their own eq. 11. Moreover, eq. 9 only applies for a uniform width and it is doubtful whether that assumption is valid for comparing Himalayan glaciers. Likewise, I believe the authors should make an effort to show the theoretical ground for eq. 10. It would have made more sense to substitute eq. 11 into eq. 9 before taking the derivative in eq. 10.*

The statement should have been $2H_m/(sL)$ is small for large L, since it varies as $1/\sqrt{L}$. Dropping of the H_m term in comparison with h_e is thus justified. The statement will be changed in the revised manuscript.

We are aware that equation 9 applies for uniform width. Our whole approach in the paper has been to use the simplest possible model. We are of the opinion that the changes due to variation of width would change the value of the coefficient multiplying this term which is determined from observed rates of retreat. As our results show, even with these assumptions, the model can be applied to Himalayan glaciers.

The theoretical basis for equation 10 lies in the Figure 8, equation 9 and the text. Since only the functional form is of interest to us, inclusion of second order effects is unnecessary.

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9. *8. Discussion: p. 2582, lines 19-21. The authors suggest that the advance due to gravity being comparable to the ablation term is the main reason for a stationary front, whereas more precisely it is the flux divergence term that is decisive.*

In the context of our empirical model, Table 4 and figure 11 clearly show the balance of the two processes: terms 1 (gravity term) and 2 (ablation term) on the RHS of equation 13 for Zemu and Gangotri.

Other main comments include:

10. *1. A reference to Figures 5, 6, and 13 is missing from the text. Possibly their discussion was removed from the text but the authors overlooked to remove the gures from the manuscript.*

Reference to figure 13 is present on page 2582, line 26.

The discussion corresponding to figures 5 and 6 is present on page 2579, lines 15 – 20.

References to figures 5 and 6 will be added.

11. *2. I am surprised to find the Khumbu glacier in the control set. I thought the Khumbu glacier retreats so slowly because it is covered by thick debris and therefore thins at a similar rate all along its valley part. Also, which part of the Khumbu glacier is considered to calculate the mean slope: only the valley part downstream the steep ice fall or the whole glacier down from the South Col to the glacier tongue? This is not trivial since the Khumbu glacier consists of two distinct parts: a steep clean glacier along the mountain ank and a low-sloping debris-covered valley glacier.*

For the control set, glaciers spanning a range of retreat rates and lengths have been used.

The complete Khumbu glacier is used to calculate the mean-slope. Many of the glaciers considered in this study, have a steep initial portion followed by a more gentle slope. Accounting for the variation of slope could be one of the possible refinements to the model.

12. *3. Looking at the sample of control glaciers shown in Fig. 10 the assumption of the same environmental changes seems worrying. The glacier selection covers a distance of more than 1000 km over which climatic conditions are expected to show more variation than assumed by the authors.*

By same environmental changes, we mean the large-scale changes, such as global temperature rise. We are aware that climatic conditions are different across the glaciers considered. Our premise is that on a climate time-scale, to the first order, the ELA changes resulting from global climate change are of similar magnitude. In this investigation, we wanted to show that if ELA changes are similar, glacier retreat will be different, depending upon geo-morphological parameters.

13. *While generally reasonably well formulated, the paper would also need input from a textual editor to improve the language. Most importantly, articles are misplaced or missing, and the should be changed by a or vice versa at many places. I am not in a position now to send a scan of an annotated manuscript with all required corrections.*

Another round of proof-reading to improve the language and style will be done.

Minor comments

14. *1. p. 2574, line 17. What are underbraces. Is the right word used here?*

It will be changed to 'underbrace'. The LaTeX command to generate the symbol is

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\underbrace
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15. *2. p. 2576, line 9: Cuffey and Patterson should be Cuffey and Paterson, also in other places and in the reference list.*

It will be changed in the revised manuscript.

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16. *3. p. 2582, line 6: forces are not the right word here. Processes seems more appropriate.*

It will be changed in the revised manuscript.

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17. *4. p. 2582, line 7-9; it is unclear to me how the effect of debris cover is implicitly taken into account in their model.*

While there is no explicit inclusion of debris cover, the value of dh_e/dt is estimated from the control set. The effect of debris cover would indirectly be reflected in the value of dh_e/dt.

This is a limitation of our approach. Additional explanation, regarding this, will be added to the revised manuscript.

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18. *5. Reference list: it is not necessary to mention the page number where the reference is cited in the main text.*

The page number is automatically generated by the typesetting system and has not been included by us.

19. *6. Fig. 2: is the retreat rate expressed in horizontal distance?*

It is expressed in distance along the glacier.

20. *7. Fig. 10: the scale bar should be expressed in km, not meters.*

The scale bar will be changed.