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The Cryosphere Discussions

Interactive comment on "Relative effect of slope and equilibrium line altitude on the retreat of Himalayan glaciers" *by* T. N. Venkatesh et al.

Anonymous Referee #2

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The paper intends to explain retreat rates of several Himalayan glaciers in terms of ELA changes and a term that combines geometric characteristics such as glacier length, glacier thickness, and glacier slope. 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. Frankly, I found the paper rather difficult to follow and truly understand, partly from what 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 simplified 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.

From a theoretical point of view, my main problems with the manuscript can be summarized as follows. These would need substantial clarification before the manuscript could be published in The Cryosphere:

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-)simplification and is insufficiently acknowledged/ discussed when the model is applied to 'real' glaciers. 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'. 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. 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 author's model. 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 flow. 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. 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. 7. Section 4.2. p. 2580, line 17. I don't 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. 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.

Other main comments include:

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 figures from the manuscript. 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 flank and a low-sloping debris-covered valley glacier. 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.

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.

Minor comments

1. p. 2574, line 17. What are ‘underbraces’. Is the right word used here? 2. p. 2576, line 9: ‘Cuffey and Patterson’ should be ‘Cuffey and Paterson’, also in other places and in the reference list. 3. p. 2582, line 6: ‘forces’ are not the right word here. ‘Processes’ seems more appropriate. 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. 5. Reference list: it is not necessary to mention the page number where the reference is cited in the main text. 6. Fig. 2: is the retreat rate expressed in horizontal distance? 7. Fig. 10: the scale bar should be expressed in km, not meters.

Interactive comment on The Cryosphere Discuss., 5, 2571, 2011.