

Replies to the referee comment by Lindsey Nicholson

We thank Lindsey Nicholson for her kind review of our manuscript. Her specific
5 comments (in blue) and our corresponding responses are given below.

Please provide a location map for the glacier.
It shall be included in the revised draft.

10 Is it possible to include some information on the glaciological mass balance de-
termination - how many stakes and stake distribution over the glacier surface
for example? In the GIF referenced there appear to be two colours in the upper
ablation zone that do not have a corresponding value in the legend - are they
actually the same yellow and red categories as in the lower terminus? If not
15 how did you deal with them?

Unfortunately details of glaciological mass balance measurements are not avail-
able with us. This issue has been discussed in section 4.1.1 our initial response
to referees' comments.

Regarding the colormap of the referred GIF, we assume that they are the same
20 yellow and red.

Please briefly detail the source data used for the geodetic mass balance in Vin-
cent et al., 2013.

This would be included in the revised draft.

25 Can you detail on the range of avalanche inputs you tested and add a description
of your quantified criteria for an acceptable avalanche contribution according to
criteria (a) and (b) - I mean what do you consider 'compatible' (as in (a)) and
how close to 1km difference do the actual and steady-state lengths need to be
30 to be acceptable in your analysis?

These issues have been clarified in section 4.2.2 of our initial response to refer-
ees' comments. We allow a range of net retreat values (300-1700 m) and this
corresponds to an avalanche input range of 1.2-1.4 m w. e./yr. This information
would be included in the revised draft and details would be provided in the form
35 of supplementary material.

You neatly find that avalanche contribution corresponds to the mismatch be-
tween the glaciological and geodetic mass balance, but you do not discuss the

discrepancy between your modeled mass balance -0.2 m w.e./y and the previously determined geodetic mass balance of -0.45 m w.e./y. Do you have a comment on this?

Our estimated net mass balance -0.2 ± 0.1 m w.e./yr and the available geodetic measurement gives -0.45 ± 0.16 m w.e./yr. Given the simplifications in our model, we do not expect a more precise match and our estimate is to be considered as a ball park value. Our estimated errorbar does not include the possible uncertainty in glaciological mass balance itself as it is not available, and there are simplifications involved in the modeling whose contribution to uncertainty have not been taken in to account. Therefore strictly speaking our error estimate may be an underestimate and the difference between the two results is not unexpected. We would clarify the issue in the revised draft.

Specific comments:

P642/L19 Reference should read Nicholson and Benn, 2006

P643/L4 An extra 'the'

P643/L28 Change we conjecture to we hypothesise

P644/L3 I think this paragraph belongs in the conclusions of the paper

P644/L13 This is a matter of personal taste, but I do not think this paragraph is necessary.

P645/L18 I suggest adding the italics in: "the negative magnitude of the net glaciological balance is relatively large compared to negative net mass balance"

P646/L15 It is not clear here what is meant by 'both inputs', perhaps better to say: "But to obtain a particular non steady state, both inputs of some past thickness profile and the time dependent mass balance function are necessary." Appropriate changes would be made in the revised draft.

P646/L24 Can you comment on how well the simple bedrock is likely to match reality here? Is there any information on that? Did you follow an optimization procedure to determine the bedrock slope and highest elevation of the bedrock? P647/L2 Could you explain what guided these choices of f_s and f_d ?

Since no data is available on the bedrock profile we assume it to be linear. Our bedrock slope and highest elevation of bedrock are chosen to reproduce the actual highest surface elevation and the elevation range of the glacier. Also we do check that for a range of f_s and f_d values our conclusion do not change much. These details have already been provided in our initial response document (sect. 4.2.3-4) and would be included in the revised draft/supplementary information.

P647/L6 Change smoothening to smoothing .. and also later in the paper Appropriate changes would be made in the revised draft.

P647/L26 I'm not clear how this part of the argument relates to avalanche inputs, it appears to refer more to the role of debris cover in slowing terminus retreat

We would rephrase the statement clarifying that Hamtah is a thickly debris

covered glacier.

P649/L14 Change till to until

5 P649/L15 Might be better to say the ‘prescribed’ specific mass balance profile, rather than the ‘current’ one, as you are here describing the optimization procedure. Also I suggest the following change for the same reason: We then tune the avalanche contribution to the mass balance profile such that (a) the retreat rate is compatible with the observations and (b) the glacier length differs from the previously determined steadystate length by about 1 km.

10 P649/L23 Change satisfy to satisfies

P650/L1 Add italics “robust as it is tied to the”

P650/L23 In the discussion you state that avalanche inputs take longer to affect the dynamics? It sounds like an unsubstantiated statement as its written now, but I think you refer to the model behavior observed at the beginning of section 2.5, so try and make this more explicit.

15 P651/L5 I suggest starting this sentence with “At first glance, this may suggest ...” and remove the following paragraph break.

P651/L7 Should read: “Benn and Lehmkuhl (2000) have argued ...”

P651/L10 Should read “of Scherler et al. (2011b) show ...”

20 Appropriate changes would be made in the revised draft.