

Interactive comment on “Future high-mountain hydrology: a new parameterization of glacier retreat” by M. Huss et al.

Anonymous Referee #2

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General comments

This paper proposes a new strategy to compute the evolution of glacier surface elevation from climate data. The method appears to be promising as it is based on detailed data set collected in Swiss glaciers over the last 100 years. It is also simple enough to be applied for many glaciers. The manuscript is well written and oriented towards the interests of the journal readers. The model description is easy to follow for non-glaciologists and runoff calculation is a good example of application for hydrological study. Future projection of hydrology in a glacierized basin will be improved by including this kind of simple but robust glacier model.

Because of the reasons above, this paper will make an important contribution to the journal, and the hydrological and glaciological communities. Before the manuscript is

C308

accepted for the publication, I hope the authors to improve the manuscript by addressing the concerns and questions listed below.

Specific comments

1. Presentation of the relationship between the elevation range and ice thickness change (Δh -parameterization)

I recommend the author to show how variable the h - Δh relationship obtained for the 34 glaciers based on measurements. Because only means of relationships are presented in Figure 3b, readers cannot know how representative the "generalized" parameterization is. In fact, the relationship for Rhonegletscher (Figure 3a) is substantially different from the mean for medium valley glaciers (Figure 3b). The best is to include all the specific relationships in Figure 3b. If it is difficult, showing the variation range in Figure 3b by drawing a grey band is helpful. It is also recommended to include curves which represent the equations used for the modelling (equations in the box in Figure 3b).

2. How to call the 3-D finite element model

The results of Δh -parameterization are compared to those obtained by a 3-D finite element model which couples ice flow and mass balance models. The author uses the term "ice flow model" for the latter and it is confusing as the model includes mass balance part as well. For example in page 354, line 21–24, it took some time for me to understand how the mass balance was treated in the model. I looked into the paper by Jouvét et al. (2009) to find the term for their model. In the conclusion, they describe as "The simulation of Rhonegletscher has been performed by combining an ice flow model with a mass balance model. This combined glacier mass balance and ice-flow model allows us to", but these are probably too long to use in the paper. What about using "3-D finite-element glacier model"?

3. "Validation" of the Δh -parameterization

The author claims that the proposed method is validated by comparing the results to

C309

those of the 3-D finite element model. Is it really possible to validate the method in this way? First, solving Stokes equations does not promise that the model results accurately capture the glacier dynamics. Especially in this kind of temperate valley glaciers, basal boundary conditions and ice mechanics (rate factor, stress exponent) are very difficult to constrain. In my opinion, the proposed parameterization might predict the future more accurately than the sophisticated finite element approach. Second, the 3-D FEM model was tuned to reproduce the observed changes in Rhonegletscher (page 6435, 5.1. in Jouvét et al., 2009). The Δh -parameterization was derived from the same observation. Isn't it evident before the experiments to get similar results? In short, validation of a model has to be done by observational facts but not by other models. In that sense, "validation" is correctly used in page 353, line 16 and 20, and page 354, line 25.

I do not criticize the idea to compare the two models. I agree that the performance of the parameterization was assessed by comparing the results to those by 3-D FEM model and it was confirmed that the parameterization is as good as the sophisticated FEM model. To avoid misuse of the method in the future, however, I suggest the author to be modest in his words.

4. Rhonegletscher modelling

This comment is related to the points 1 and 3 listed above. It is useful if Rhonegletscher is modelled with the generalized parameterization derived from observations in medium sized glaciers. Comparison of the result to 3-D model is more useful to assess the performance of the proposed approach, and the difference from the parameterization specific to Rhonegletscher provides important information. Detailed presentation is not necessary, but showing the result of this additional experiment in Figure 9 is of great value.

Technical corrections

page 346, line 13: "In case studies" » "As case studies"?

C310

page 347, line 5: "have" » the anticipated disappearance "has"

page 347, line 14–20: Ice flow models and ice flow-mass balance coupled models are mixed in the references. The references (Greuell, 1992; Oerlemans, 1997; Sugiyama et al., 2007) deal with ice flow-mass balance coupled models, which are in the same category as (Vieli et al., 1997; Wallinga and van de Wal, 1997). The references (Hubbard et al., 1998; Gudmundsson, 1999) studies ice flow only, whereas (Jouvét et al., 2008, 2009) couples ice flow and mass balance models.

page 347, line 28: Here and everywhere, "proposed by (Huss et al., 2008b)" » "proposed by Huss et al. (2008b)"

page 348, line 6–7: "... the glacier length is approximated with the surface elevation ..." » Are you talking about Δh -parameterization? Isn't it relate the longitudinal position along the glacier (not glacier length) with the surface elevation?

page 348, line 24–26: Any references for the radio-echo soundings?

page 348, line 26: Remove "on".

page 349, line 16: "changes" » "change"

page 349, line 16–17: "all spatial ... ice flow dynamics" » what about "surface mass balance at each point is compensated by ice flow."?

page 349, line 21–22: "but shows ... mountain glaciers." » Why?

page 350, line 2: "retrieved" » "derived"?

page 350, line 6: "h- Δh function" » " Δh vs. h function" is used in the same page, line 21. Please be consistent.

page 350, line 9–10: "The DEMs ... one decade" » Isn't it dependent on the magnitude of the elevation change and DEM accuracy?

page 350, line 14–18: It should be stated that these periods given as examples are for

C311

glaciers in the Alps.

page 350, line 20–22: "Thus, it is proposed ..." » I understand that all the parameterizations were carried out with the data over the 20th century. If it is correct, this sentence is very confusing.

page 250, line 18: "delayed response of ice flow" » "delayed response of glacier geometry"? Ice flow does not respond to a change in climatic forcing and its response to a change in geometry does not delay.

page 351, line 1–3: What about "In order to investigate geometry changes of unmeasured glaciers, generalized Δh - parameterizations were derived for different glacier size classes from observations in Swiss glaciers.

page 351, line 5–9: What is the period taken for the parameterization? How many samples for each size class?

page 351, line 12–13: "the curvature .. smaller" » What about "the elevation dependence of Δh is more uniform over the glacier."

page 351, line 21–22: "obtained for ... easily available data sets" » It may be true for Swiss glaciers, but not for glaciers in other regions!

page 352, line 10–11: "It is assumed ... immediately" » This sounds strange because the redistribution of ice mass delays in nature and the Δh parameterization takes into account this delay.

page 353, line 23–24: "reasonably simulates" » "reasonably well simulates"

page 354: Can you briefly describe how the rate factor A and basal sliding are treated in the model?

page 356, line 16–17: Please rewrite this sentence. It is not clear.

page 356, line 24: "assumed to reproduce these" » What are "these"?

C312

page 356, line 26: "large Alpine valley glaciers" » Do you mean "large valley glaciers in the Alps" or "large alpine valley glaciers"?

page 356, line 27: "loose" » "lose"

page 357, line 3–4: Isn't it because ice flow near the glacier terminus is difficult to model?

page 357, line 23: "good" » "better"?

page 357, line 23-25: "This is explained ...": This sentence is not clear.

page 357, line 26–28: " The parameterization ..." » What about "The ice-covered areas agree each other within 3% throughout the entire modelling period."?

page 358, line 1: "less than" » "the difference of"

page 358, line 22: "next decades" is not very accurate as it happens 40–60 years later for Scenario 2 and 3, and does not happen for Scenario 1. It also contradicts to the first sentence of the next paragraph.

page 359, line 7–8: "...with significantly reduced storage capacity...": Do you mean that risk of floods increases because snow-covered area over a glacier decreases resulting in less water storage in snow layers? It is not clear for me.

page 361, line 2–4: It is not clear why AAR-method is suitable for these conditions.

page 361, line 14: "looses" » "loses"

page 363, line 26: "Changes in ..." » I agree with this. It will be very interesting if the parameterization is performed for different time period in the past for Rhonegletscher.

page 364, line 5–14: "We find ..." » This should be described later in the conclusion as the main subject of the paper is Δh -parameterization rather than the modelling results.

Table 2, 3, Figure 5 and 14: Captions of these tables and figures start with "Validation of ...", which are not correct. They should be "Comparison of ...".

C313

