

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

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We would like to thank the reviewer for his/her comments that were very helpful to improve the initial manuscript.

Below, all comments of the reviewer are given (in italic), and discussed (normal type style), and, where applicable, we suggest a new version of the text (in quotation marks).

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

C644

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).

Done. See comments and changes in the response to Reviewer #1.

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 iceflow model allows us to", but these are probably too long to use in the paper. What about using "3-D finite-element glacier model"?

In general, we agree with the reviewer. However, we would like to stress the flow dynamics part of the model by calling it "ice flow model". The description of the ice flow dynamics is the only difference compared to the glacier change calculation framework implemented in GERM, which features the Δh -parameterization. Therefore, we still call the "3D finite element ice flow and surface mass balance model" simply "ice flow model", but provide some more details in the model description (see below), and at other locations in the text.

"A three dimensional finite element ice flow model coupled to a surface mass balance

C645

model is used to simulate the flow dynamics of Rhonegletscher.”

3. "Validation" of the Δh -parameterization

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

The reviewer raises two points:

- 1) Validation is the wrong wording for the comparison of two models. We have substituted "validation" with "comparison", where differences between model results are analyzed. The comparison of the ice flow model with observed glacier geometry change in the past, and the comparison of measured and simulated runoff are still termed 'validation'.
- 2) The ice flow model was calibrated with observations in the past; the parameteriza-

C646

tion was derived from the same data. The crucial difference between the two methods is, however, that the ice flow model physically describes the processes of glacier flow and mass redistribution, whereas the parameterization lumps all these processes into one simple empirical function. The comparison presented in this paper shows, that also with this high degree of simplification, the parameterization provides results similar to those of the ice flow model. This is not evident at all from the beginning! The good agreement over a period of 90 years with strong climate change shows that the simplifications are still valid under different conditions than in the 20th century. A simplified approach can thus reasonably well replace complex ice flow modelling for the calculation of future glacier change in assessments on a regional scale (e.g. hydrology, global sea level rise).

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.

This comment goes into the same direction as comment 2 by Reviewer #1 – how different is the Δh -parameterization derived for an individual glacier from the generalized function for its size class? As the paper already includes a large number of different model runs (2 glaciers, 2 methods, 3 scenarios) we would not like to add even more lines to the figures in order to avoid confusion. The statistical analysis about the spatial transferability of the parameterization which is added to the Discussion section should suffice to respond on this comment.

C647

The technical corrections and suggestions by the reviewer were very helpful and were adopted as proposed. In the following only points are discussed that required larger changes and additional commenting.

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 (Vielé et al., 1997; Wallinga and van de Wal, 1997). The references (Hubbard et al., 1998; Gudmundsson, 1999) studies ice flow only, whereas (Jouvet et al., 2008, 2009) couples ice flow and mass balance models.

The ordering of the references follows (i) the separation between *flowline models* and *3D models*, and (ii) their application to *past* or *future*. In order to be fully consistent, we removed Jouvet et al. (2009) from the list.

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

"For application of the Δh -parameterization in practice, the position along the central flowline of the glacier (as shown on the abscissa of Figure 1b) is approximated with the surface elevation, assuming these variables to be strongly correlated."

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

This statement is removed.

C648

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

We agree and have changed the text accordingly.

"The DEMs should be apart for a sufficiently long time period to show elevation changes that are significantly higher than the DEM uncertainty."

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.

The statement is reformulated and is now disconnected from case (1), which has been provided as an example.

"Thus, it is proposed to derive the Δh - h function for periods between successive DEMs characterized by persistent glacier mass loss."

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.

We did not change the sentence. Generalized Δh - parameterizations cannot be used to "investigate" geometry changes of unmeasured glaciers. In the best case, they can "approximate" them. Furthermore, it is important here to emphasize the unavailability of repeated glacier change observations that is likely for many glaciers the parameterization might be applied to.

page 351, line 5: What is the period taken for the parameterization? How many sam-

C649

ples for each size class?

"For 34 glaciers for which repeated DEMs over the 20th century are available (Bauder et al., 2007; Huss et al., in press) glacier-specific Δh -parameterizations are derived (Fig. 3c). The functions were averaged in three size classes with n members – large valley glaciers (area $A > 20 \text{ km}^2$, $n=8$), medium-sized valley glaciers ($5 \text{ km}^2 < A < 20 \text{ km}^2$, $n=12$) and small glaciers ($A < 5 \text{ km}^2$, $n=14$). Most parameterizations were derived for the period between the 1930s and present; some functions only refer to the last two decades."

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!

The required input data are not only available for the Swiss Alps, but potentially also for remote regions of the world: Relatively accurate DEMs can be obtained from the Shuttle Radar Topography Mission (SRTM) or from ASTER; an increasing number of mountain ranges worldwide is covered with glacier inventories providing glacier outlines (see GLIMS data base). We added some details in the text to elaborate on this important point.

"Once the Δh -parameterization is established, its application for the calculation of future glacier extent requires the following field data input, which can be potentially obtained for a large number of glaciers, also in remote regions of the world, from readily available data sets (e.g. DEMs from the Shuttle Radar Topography Mission (SRTM); glacier inventories):"

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.

C650

"It is assumed that the redistribution of the annual surface accumulation by ice flow is instantaneous."

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

"Given the initial shape of the glacier, the ice flow velocity u is obtained by solving a 3D nonlinear Stokes problem derived from a regularized version of Glen's law with a temperature independent rate factor (Jouvet et al., 2008, 2009). A nonlinear sliding law is prescribed along a given part of the ice-bedrock interface. Both rate factor and sliding coefficient minimize the root-mean-square error of the difference between observed and simulated surface velocities. Then, glacier geometry is updated each year by computing the volume fraction of ice φ , which satisfies the transport equation ..."

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

"We apply the specific Δh -parameterization for Rhonegletscher (Fig. 3a) and simulate the evolution of Silvrettagletscher using the approximation (Eq. 1) derived for the sample of small alpine glaciers (Fig. 3b)."

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

"The flow model is assumed to reproduce glacier geometry changes ... "

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

This may certainly contribute to the misfit. However, the ice flow model performed well

C651

in reproducing length and surface elevation changes at the glacier tongue over a 130 year period (see Fig. 5, or Jouvet et al., 2009). The errors are too important to just attribute them to uncertainties in the ice flow model. Given the high degree of simplification in the Δh -parameterization it would be speculative to claim that it performs even better than a 3D finite element ice flow model in predicting glacier retreat (although this is not impossible). We did not change the sentence.

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

"For this scenario ice flow dynamics are similar as in the period for which the Δh -parameterization was derived. This might explain the good performance. "

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.*

"... indicate that this transition will take place until the end of the 21st century."

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 to me.*

Changed. See corresponding comment in the response to Reviewer #1.

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

"For predicting glacier area over a time step that is much longer than the glacier response time (e.g. Hoelzle et al., 2003) the 'AAR-method' may yield suitable results."

C652

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.*

We agree with the reviewer regarding the main subject of the paper. Our conclusions are structured into a results summary including the glacier evolution scenarios, followed by two paragraphs on the suitability of the parameterization. Thus, we provide the main findings of the study in the very end of the paper, which is, in our opinion, most appropriate for the 'take-home message'.

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 ...".*

We substituted "validation" with "comparison" except for Figure 5, where the model results are compared to field observations.

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