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Hydrology and Earth System Sciences Discussions

Interactive comment on "Simulation of high mountainous discharge: how much information do we need?" *by* B. Schaefli and M. Huss

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Response to anonymous reviewer 1

We would like to thank the reviewer for her/his interest in our paper and the detailed review, which will certainly help us sharpening our paper. The overall comment is articulated around three points, which we will answer hereafter before addressing the comments on the paper structure, language and some additional detailed comments.

1) Title not reflective of the content

We agree with the comment that the title is misleading since it might imply an exhaustive treatment of the "how much" question and it hides the important focus of the paper

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on the question on how to integrate different types of data. We will change the title to account for this comment and to properly reflect the content of the paper (including namely an explicit reference to the use of point glacier mass balance data).

2) No exhaustive treatment of the "how much question" and no new results

We can understand that the "lack of more detailed analyses on the value of different"amounts of data is a bit disappointing" given the misleading title. Our paper suggests a method to extract information about the water balance from the data at hand and explores (i) whether point accumulation / melt samples contain extractable information and (ii) how many sources of information about the water balance we need to reproduce it reliably. We do not address the question of how long the corresponding records do have to be. This question is certainly also interesting and it is addressed in other papers (e.g. Konz and Seibert, 2010).

The reviewer suggests that we could come to more interesting conclusions if we were to complete a more systematic treatment of the record length question. To our point of view, a systematic investigation of the record length question has no absolute value: such results are not transposable to other catchments since the absolute results (e.g. 'we need 5 years of data') is intimately related to the ratio between information and disinformation content of the data, i.e. to the quality of the input / output data. In exchange, proposing a methodology for such a systematic investigation, just as Konz and Seibert (2010) did, is certainly very interesting but was never the focus of our paper.

We wanted to suggest a method to actually extract and combine information from available data. The main methodological contribution is the systematic integration of point observations to update the model parameters.

Contrary to what the reviewer suggests, our main conclusion is rather new: as we state in the conclusion, it is often assumed that some glacier-wide annual mass balance estimations, in addition to observed discharge, is good enough to obtain reliable estimates

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of the different water balance terms (e.g. Konz and Seibert, 2010; Schaefli et al., 2005; Stahl et al, 2008). In the submitted manuscript we show evidence that information on the seasonal mass balance is a pre-requisite to reliably calibrate a hydrological model. This conclusion is important for climate change impact studies.

Our paper also shows how to proceed to update a simple model structure in a step-wise manner to include new data types - rather than switching immediately to a more complex model under the assumption that a good description of natural variability (as reflected in point observations) needs more complex models. We believe that this systematic analysis is valuable since many available studies suggest methods to increase the flexibility of the underlying concept i.e. to extend the degree-day approach, see e.g. Hock, 1999) without a systematic analysis of why the more flexible model works better and if it works better for the right reasons.

3) Model does not include firn

Our model does not consider the transition from snow to firn. From a model point of view, accounting for firn would add three additional degrees-of-freedom (an additional degree-day factor, an additional water retention capacity and a melt-water runoff transformation coefficient).

The use of a firn seems a priori having the potential to greatly increase the flexibility of the model. However, during the development of the initial version of the used model (Schaefli et al., 2005), we noticed that adding firn cannot improve the model performance. This is due to the fact that only in extremely hot years, significant firn areas could become directly exposed to the melting. In most years, the relative portion of firn at the glacier surface is low. This is in strong contrast with adding three new parameters which would be very difficult to identify.

Of course, in the presence of information about the different melt components (e.g. through isotope studies) for individual melt seasons, a differentiation between the components could become feasible and necessary.

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Paper structure

The reviewer suggests to better separate the methods from the results. We were confronted with the classical problem that you need the results to explain the method and vice-versa. We believe that we found a good compromise; a strict separation between methods and results is not always possible and we believe that your solution follows the natural flow of ideas. The results section does in fact not present new methods, only options for model modification which have been presented in other papers. We will, however, add a sentence in the methods part to justify this choice.

Language

The reviewer makes the very general statement that the language could be improved and in particular, that past and present tense should be used more consequently. We will carefully check our paper to make it easier to read.

Detailed suggestions:

- Yes, eq. 1 only applies to the glacier area, otherwise, the ice melt factor is zero; thanks for pointing this out;

- We will include a brief description of the rest of the model.

- Reviewer comment: *Section 2.3 In the last sentence you state that annual discharge has been used to validate estimates for precip, but how can you do this in a catchment with a glacier?* Answer: This comment refers to the model of Huss et al. 2008 about which we say "Total annual discharge observations are used to validate the estimates for catchment precipitation" We will rephrase this sentence since Huss et al., 2008 actually used the discharge observations to qualitatively assess their estimate of total precipitation on the non-glacier part of the catchment. What is important is that they did not use discharge observations to calibrate their glacier module.

- Eq 4: thanks for pointing out the mistake (the value Q bar refers to the observed mean annual discharge, which should not depend on the parameter set)

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- We will merge eq. 4 and 5 and omit the explanation in parantheses, p8674, line 3-4

- Eq. 6: the normalization is not really required for optimization (when we used it for plotting, we mentioned it); for reasons of coherence, we will introduce it here

-Eq. 7: on the right hand side it should read W(zref) and V(zref); we apologize for this mistake

- p 8663, line 8ff, Comment: *This sentence reads a bit confusing*; answer: we will rephrase it

- Numbering of the figures: we will check to avoid an inconsistent order of figures

-Comment: *Figure 1 is not needed*; answer: the figure shows the important concept of non-dominated and dominated volume by points of a Pareto-curve; Pareto optimality has become a well-known concept, however, the concept of dominated / non-dominated volume is rarely mentioned explicitly; since another reviewer asked for a figure showing the catchment, we will replace fig. 1 for the revised version and refer to the HESSD manuscript.

- Figure 2: (reviewer suggest using a boxplot); in a boxplot it could be hard to distinguish between the median of the observations and the simulation; we will check whether it gives a better visual representation;

- Fig 3, we will include the inset as a separate figure

- Fig 5: the correlation plots show the same data as the histograms and nicely fit in here; we think that this should be ok.

- Fig 6: we agree that it is small but we submitted it as a vector-file and it can be zoomed in on the screen. We will try to find a better solution to present these results without increasing the number of figures

- P8683: comment: *the coefficient of variation can't be negative, can it?* answer: it is defined as ratio of the standard deviation to the mean of a variable and can thus be

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negative;

- P 8686,: line 10: comment: *what is meant by negative degree-day factors?* answer: the text states "negative degree-day factor anomalies" which uses the often-used concept of an anomaly with respect to a mean value. Here it means that for this period, Huss et al., 2009 found degree-day factors that are smaller than the mean value for the entire observation period ; we will rephrase the sentence.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8661, 2010.