Hydrol. Earth Syst. Sci. Discuss., 7, C4779–C4785, 2011
www.hydrol-earth-syst-sci-discuss.net/7/C4779/2011/

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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Received and published: 20 January 2011

Response to Reviewer 2 - part 1

We would like to thank this reviewer for his/her comments about our manuscript and the concise summary of our contribution to the field of conceptual modeling of glacier-fed discharge.

We give hereafter detailed answers to all specific comments, without following the order of the initial comments; we have chosen an order of importance of the topics.

There is one topic that we haven chosen to answer in a *separate comment*: the question referring to the *thermal state of the snowpack*. Since we give a detailed answer to this comment, a separate answer will simplify reading and referencing.

Model does not include firn

Can the authors comment on what effect this might have on the results? How much firn cover existed on the glacier at the beginning of the simulation period and how did it change through time? For example, Braun and Escher-Vetter (1996) showed how the depletion of firn at Vernagtferner, beginning in the early 1980s, influenced both meltwater production and runoff routing. If such transient responses are not explicitly accounted for, to what extent might the apparently optimal parameter distributions and model structures be distorted?

Reviewer 1 also commented on this, stating that the model is too simple. As already discussed in our answer to reviewer 1, adding a firn component would only have a noticeable effect during extremely hot years.

We would like to emphasize here that we do not ignore the existence of firn; we simply do not use different parameters (degree-day factor, retention capacity, runoff coefficient) for the seasonal snow and for firn. Since the degree-day factor for firn can be assumed to be higher than for snow, our model might slightly underestimate the production of meltwater during very hot years.

The extent of exposed firn can be estimated based on the following reasoning: firn is only exposed to melting in years where melting is exceptionally stronger than in preceding years. If this situation lasts for several consecutive years, the firn will retract and not be exposed anymore. In technical terms, firn is only exposed if the elevation of the equilibrium line altitude (ELA, the line where the glacier balance is zero for that given glaciological year, i.e. accumulation equal ablation) is higher than the longer term mean ELA, which we call here $\overline{ELA}$.

To give an idea of the order of magnitude of the size of the exposed firn area, we consider the yearly ELA estimated by Huss et al. (2008) and take $\overline{ELA}$ equal to a moving average over 6 years (see Fig. 1 of this comment). This gives a maximum amount of potentially exposed firn of 20% of the glacier surface for two years, which would correspond to one elevation band. For the other years the percentage of exposed firn is significantly less, and only 2.5% in the mean over the 30 years considered.

This small amount of potentially exposed firn, justifies not using a separate melt factor for firn. However, considering the retention capacity of potentially thick firn layers remains important (see also response to reviewer 3).

The melt underestimation that might result from slightly too low melt factors during years with exposed firn is acceptable in view of what follows:

If we do add firn, we have three additional parameters to estimate, of which two can only be calibrated on discharge dynamics, increasing the number of parameters for the meltwater - runoff transfer from six to eight. The overall identifiability of the parameters would decrease; and there is a high chance that the additional firn parameters, referring to a process that is rarely active, do actually "distort" the other runoff parameters. An option would have been to calibrate the firn parameters during an extremely hot year; however, the extreme year 2003 is unfortunately outside the observational period.

How might adding further model complexity influence the conclusions? How specific are the conclusions?

Thanks for this important comment, this should certainly be addressed in the discussion section and the conclusion.

The most important conclusion of our analysis it that information about seasonal balances is required to reliably calibrate a discharge model for catchments similar to the one studied in the paper with a significant glacier cover. This is due to the fact that neither the annual glacier balance nor the discharge do contain sufficient information about the intra-annual variability of the accumulation and melt processes to calibrate the model. This conclusion is not model specific; we cannot think of examples where

the annual glacier balance contains information on the distribution between accumulation and ablation. The discharge can, in particular cases, contain direct information about the ablation; for example for catchments that are almost 100% glacier-covered.

This conclusion is transferable to many other case studies in climates where there is a distinct accumulation and ablation season, which applies to most glacier catchments in the world (see Kaser et al., 2010) but namely not to glacier catchments in the tropical Andes, where ablation also occurs during the accumulation season.

Adding complexity to a model, almost surely increases the variability of the model response. This additional variability is imposed on the model by the modeler and reflects his/her a priori knowledge (and should of course reflect the variability in the modeled processes). Thus, a more complex model might not show the need for seasonal balance information during calibration - either because it reproduces by chance the seasonal balances or because the modeler had sufficient a priori knowledge. Distinguishing between the two cases is not trivial.

All other conclusions are at least partly dependent on the model complexity and on the available type of input. For example, the estimation of seasonal temperature lapse rates would not be necessary in presence of a distributed temperature input field. If we had used an extended temperature-index approach where degree-day melt factors are corrected to account for aspect and slope (e.g. Hock, 1999), we might possibly also not have seen a need for seasonal lapse rates, since radiation could have accounted for the seasonal variations.

We are currently working on a study that attempts to quantify whether including potential radiation in a degree-day model simply emulates the absence of spatially and temporally distributed temperature information or whether it adds information (Tobin et al., 2011). This study is completed in a research catchment with detailed meteorological observations; we will try to transpose the method and results to the Rhone catchment to investigate the question of how the use of an extended temperature-index approach might interact with seasonal lapse rates and with the onset of melt outflow from the snowpack in spring.

Is all of the catchment above tree-line? If not, what is the effect of not accounting for processes such as interception loss?

The outlet at Gletsch has an altitude of 1760 m asl; this is slightly lower than the tree line in this area; according to the Swiss topographic map (scale 1:25’000), 2.4% of the catchment area are covered by forest and another 19.0 % by other vegetation.

Given the low potential evaporation in our catchment, the effect of fast evaporation of intercepted water (rather than slow transpiration after infiltration into the soil) is certainly negligible. Not including interception in a conceptual rainfall-runoff model is in general ’dangerous’ since a model might later on be used for a different climate than for the one for which it has been developed (this risk is low with our very specific model). Should this kind of model be used for much lower altitudes, it is indeed important to include interception since even if its effect is not visible in the overall water balance, it might affect the runoff parameters (see also Savenije, 2004).

Provide a more specific review of the ways in which previous studies used mass balance information to assist in calibrating a hydrologic model

We will sharpen the review and better discuss how our contribution fits into existing work.

A map of the catchment would be useful

We will include a sketch showing the location and topography of the catchment (see also Fig. 1 of our response to reviewer 3).

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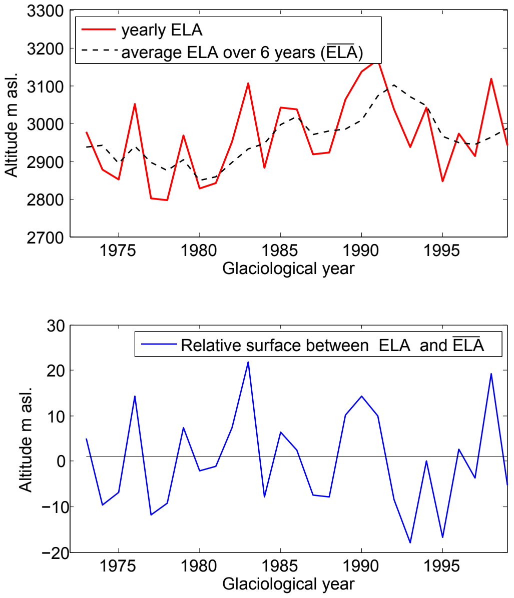

Fig.1: top: yearly ELA and $\overline{ELA}$ over 6 years for the Rhone glacier; $\overline{ELA}$ is assumed to approximate the elevation of the firn line; bottom: amount of glacier surface (in %) that corresponds to the altitude difference of ELA and $\overline{ELA}$; this surface is positive if ELA>$\overline{ELA}$, it is negative if ELA<$\overline{ELA}$; positive values indicate exposed firn.

Fig. 1.