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Hydrology and Earth System Sciences Discussions

Interactive comment on "Ice volume distribution and implications on runoff projections in a glacierized catchment" *by* J. Gabbi et al.

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General comments

This well-written manuscript analyzes how the estimation of the ice volume distribution affects runoff projections under climate change scenarios for a highly glacierized alpine catchment (the Mauvoisin catchment in the Swiss Alps). This topic is highly relevant for the quantification of future runoff volumes in high alpine environments, e.g. for hydropower production. The study compares the effect of different estimates of the ice volume distributions (observation based, modeled-based, a uniform distribution) on simulated runoff and completes a sensitivity analysis.

In its current form, the manuscript reads very well but it leaves me with the general impression that I do not really know what has been done in terms of modeling. All the modeling methods are presented elsewhere and reading this paper, I cannot judge how the results might be related to the modeling assumptions. In particular, there are almost no details on the hydrological model and its calibration (on very uncertain lake inflow data, see detailed comments), and the related glacier evolution model is, as far as I can see, not outlined (there is only a reference).

Furthermore, the results are not compared to the results presented in (Schaefli et al., 2007b), which was a detailed study on the effect of climate change on the hydropower production system of Mauvoisin. Such a comparison (at least in a few words) seems important to assess the effect of scientific progress / additional information on the runoff projections : the study of Schaefli et al. made much more simplifying assumptions about the ice volume distribution (at a given moment in time, infinite ice volume for all catchment units covered by ice) and about the glacier evolution (glacier surface evolution with the so-called accumulation-area ratio, AAR). The infinite ice volume assumption would as such have been an interesting setting to test since this represents in fact the simplest assumption for studies that do not have any ice volume estimates (rather than your uniform assumption which still assumes knowledge of the total ice volume). And a comparison with the AAR method would be interesting because it is still widely used and this study presents a unique opportunity to show how this extremely simplifying assumption affects runoff projections.

Detailed comments

Introduction: I suggest removing references referring to studies in the tropical Andes and the Himalaya when talking about alpine systems; please refer to my paper (Schaefli et al., 2007b) rather than to my PhD thesis. Furthermore, I would not include references from before the 90ies as “in recent years”; did Bergström study alpine basins? And Singh and Kumar? I suggest including (Horton et al., 2006) instead.

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Case study: The entire catchment connected to the accumulation lake of Mauvoisin has a size of 169 km2 (Schaefli et al., 2007b). From Fig. 1 it is clear that you did not consider a part of the right hand side water intakes. As far as I know, these water intakes do not have flow measurements, i.e. their contribution to the lake filling is included in the water level measurements. How did you subtract them? Please mention explicitly that there are no runoff measurements, only indirect lake inflow estimations based on the lake outflow and lake level measurements (where the lake outflow is not observed directly but estimated from hydropower production, see the case study description in Schaefli et al., 2005 and Schaefli et al., 2007a). How did you handle the rather important uncertainties that arise for the estimation of lake inflow (e.g. negative inflow during the winter months, see Schaefli et al., 2007a)? How did you calibrate your model on monthly data and a single year of daily data?

Input data: For what time periods did you have observed input data? Why did you not use the precipitation station of Mauvoisin?

Model calibration: On which period did you calibrate, on which period did you validate your model? how can you give a range of Nash values for a single year of daily observations? A Nash-value of 0.79 is relatively poor for this type of runoff regimes (Schaefli and Gupta, 2007); given that you analyze annual runoff volumes, it would be interesting to know the bias of the model.

Paper structure / methods: I think that there should be a methods section describing the basics of the used model to understand how it simulates glacier evolution and runoff and to have an overview of how many parameters have to be calibrated. I also would like to have an idea how the meteorological variables were interpolated; furthermore it should be mentioned how results back to 1900 are obtained. At the moment some methods are included in a section 4 that seems to be a mixture between methods and results. I also suggest to include some more text on the climate scenarios, mentioning at least some key numbers (future temperature, precip., spread between climate models).

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Interactive Comment

Paper structure / results: I suggest moving Section 4.2 and 4.3 to a results section since otherwise it is not clear what is obtained from modeling and what from observation; this should be very clear (e.g. beginning section 4.3 discusses observed evolution; should this not be presented elsewhere?). All results discussing future evolution should explicitly refer to the future time period (e.g. for the uncertainty assessment on top of page 7522 it is not clear to what time period it refers).

The discussion in 4.3 would benefit from a few references to existing study (since the main features of future runoff regimes are obtained in all similar studies).

Sensitivity analysis: From Fig. 10, it appears that the moment of maximum annual runoff is almost linearly related to the initial ice volume estimate; what feature of the model / ice volume distribution leads to this result?

Discussion: It seems like a uniform assumption is not too bad after all, especially if interested in long term projections; may be the sentence "A uniform ice volume distribution (..) is therefore for projections not suitable." is a bit too strong

Uncertainty discussion: What is the order of magnitude of the uncertainty related to estimated area-average precipitation input? Are you confident that your calibrated model is suited to simulate future runoff? Table 3 indicates that in the future, up to 25% of the non-glacierized runoff is surface runoff. What explains this?

Conclusion: for my taste, the conclusion is too strongly focused on the now well known effect of climate warming on runoff, which was not the main focus of the study; it is in exchange, not stressed how important good ice volume estimates might be for projections for the next 30 years or so; and no outlook is given on how such good estimates might be obtained.

Table 3: Does the indicated year correspond to the end of the 30 year period? it is clear from the numbers that the annual runoff corresponds to the sum of the other components. The naming of these components is a bit misleading since for the glacier part,

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the naming refers to the input type (melting ice or melting snow), whereas for the non-glacier part, the naming refers to the flow path. There is certainly snow runoff from the non-glacierized catchment parts, would it not be interesting to know its evolution, even if it is not the focus of the paper? Furthermore, "Q non-glacierized" should probably be relabeled since it is only surface runoff.

Fig. 4: I suggest to add uncertainty bands also to the observed ice volume evolution; also, it is not clear from the text, what explains the uncertainty bands of the projections. To my view, there is not enough information about the climate scenarios to understand this spread.

Fig. 6: it would be interesting to add "plausibility" intervals to the runoff simulated for the past; these bands could account for the sources of uncertainty quantified in the text; not quantified are parameter uncertainty and area-average precipitation uncertainty.

Fig. 7: The subsurface component of the runoff should be relabeled, it is not groundwater; I suggest relabeling all of the runoff components; from this figure, I conclude that Qsnow also contains snow-induced runoff from the non-glacier covered catchment part (given that the contribution of the glacier-free part starts so late in the year); please clarify.

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