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Discussions

Interactive comment on "Modeling the effect of glacier recession on streamflow response using a coupled glacio-hydrological model" *by* B. S. Naz et al.

M. Huss (Referee)

matthias.huss@unifr.ch

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This paper presents the implementation of an ice dynamics model into a physically-based spatially distributed hydrological model. Both models have been published earlier but their combination is interesting. The coupled model is applied to the Bow River catchment for a two-decadal period. The authors do a good job in assembling many data and using them in a multi-objective calibration (discharge, mass balance, snow accumulation, glacier area change). In general, the paper is well written and methods are clearly described. However, I have two substantive comments that should be addressed by the authors before the paper can be accepted.

Previous literature: In the introduction the authors review papers on the subject of hydrological modelling and the consideration of dynamical changes in glacier extent. They cite several hydrological studies that update glacier surfaces "offline" in decadal time steps, or with volume-area scaling approaches, i.e. not with an spatially distributed approach that is integrated in the hydrological model. This leaves the impression that the direct coupling of hydrological models with glacier dynamics has not been performed yet. However, in recent years a considerable number of papers has been published by several groups that calculate annual changes in distributed glacier thickness and extent based on ice flow modelling approaches, or approximations that are based on the concepts of ice flow modelling either using calculated or measured glacier-bed topography (see e.g. Immerzeel et al., 2012, Climatic Change; Huss et al., 2008, Hydrological Processes; Huss et al., 2010, HESS; Uhlmann et al., 2012, Climate Dynamics; and several more references). These studies / approaches have been applied to single glaciers and to glacier clusters. My comment should not indicate that the methods presented here are not worth publishing as they tackle the problem from a slightly different angle, but the authors need to account for the progress that has been achieved in the field of glacio-hydrological studies in the last years.

Lacking evidence that the coupled model is actually "better": A new model is proposed and tested for a 20-year period in the past with relatively small changes in glacier area. In the present form of the paper, I do not see direct evidence that the coupling of the models is actually an improvement for the accuracy of glacier-runoff modelling. In the conclusions the authors claim that the model performs better compared to a static implementation of glaciers in the model. This seems to be obvious just for logical considerations, but it is not actually shown! I would assume that over the modelling period, the results of the new coupled model would only differ very little from a simple model run without the flow dynamics model (as the changes in glacier area are small, Fig. 10). Stating that runoff is underestimated when NOT accounting for the presence of glaciers does not allow evaluation of the increase in performance of the new model (as also stated by the previous interactive comments). A 'typical' hydrological model without a glacier dynamics module would not completely omit glacier coverage, but just not account for the changes. This experiment should also be conducted by the authors so that they can discuss the benefit of the coupled model. However, I assume, that the benefit of the new model would only be revealed when a long time period with significant changes in glacier area is considered. Furthermore, the physically-based model fails to very well reproduce the runoff regime (Fig. 12). The authors discuss the runoff underestimate in late summer and attribute it to an uncertainty that directly originates from the model coupling. The significant overestimate of modelled discharge in early summer (peak runoff) is however not discussed. It would be important to closely look into these issues of systematic deviation from the observations and to track their origin within the fully physically-based model. The fact that the initial glacier extent can only be obtained with a spin-up model run that will never perfectly reproduce the observed glacier area distribution at a given point in time seems to be a major limitation of the presented approach that should be discussed in more detail. This limitation implies that, although exact knowledge about present glacier area distribution can easily be obtained, the model cannot be directly initialized with these.

Specific comments:

- page 5016, line 8: Several physically-based algorithms for calculating subglacial topography have recently been proposed, also by one of the co-authors of this paper. Thus model data on the bedrock topography of glaciers is potentially available. This section might be updated accordingly.

- page 5018, line 13: It would be helpful to provide some more technical details about the hydrological model. I am aware that it is already described in other publications, but it would for example be interesting to know how the different input data fields for the energy balance calculations were extrapolated over the basin.

- Page 5025, line 12: Is the degree-day factor used for snow or for ice? DDF models always discern between the different surface types. The authors should thus also state

the DDF used for the other surface type. The DDFs derived by Radic and Hock (2011) are based on a model with monthly resolution. Time resolution has a major impact on the absolute values of DDFs. If also a monthly model is used here (not stated if I am right) the calibrated parameter might be transferable, if not the DDFs should be re-calibrated.

- Page 5025, line 24: The approach to obtain initial ice thickness distribution for running the model is interesting as it is fully dynamic. However, it is not clear how the input data are obtained: Obviously, the bed topography (page 5025, line 7) is required to run the model. But isn't the bed topography an OUTPUT of this approach? Is there some kind of iteration performed? In presently non-glacierized regions, a surface DEM would of course provide the required model input, but not in glacierized regions. As much as I understand the glaciers are thus built up on top of the present glacier coverage (their surface elevation). This issue should be discussed and the uncertainties be addressed.

- Page, 5027, line 22: stating the Nash-values for simulation without a glacier does not say much. Observed runoff does include glacier melt. When excluding this component in the modelling the comparison is no longer possible.

- Page 5030, line 10: There are quite some different definitions of the glacier contribution to runoff. Although the authors state what they consider as glacier contribution (decrease in runoff when removing ice-covered areas in the modelling) they should maybe also consider discussing other approaches to calculate glacier contribution to runoff. For example, the glacier contribution could also be considered as all water exiting the glacier in a given month (thus including all melt terms and rain over the glacierized surfaces), or the change in water stored by the glacier over a given time span (including snow- and icemelt over the glacier minus accumulation and evaporation). All approaches have their justification but yield completely different results causing quite some confusion in hydrological literature on the topic of glacier contribution to runoff.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5013, 2013.