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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Bridging glacier and river catchment scales: an efficient representation of glacier dynamics in a hydrological model" *by* Michel Wortmann et al.

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Thank you for the review and constructive comments. We have responded point by point below with your comments in italic.

This manuscript deals with the representation of glaciers in hydrological models. This is a very important issue since, as the authors correctly describe, the link between glacier models and hydrological models is important but often not fully represented in modeling. The manuscript describes a new modeling approach and its application to two catchments. My main concern with the manuscript is that the proposed model (routine) is not fully clear (at least to me) and convincing. Other (minor) concerns relate to the optimization approach and the structure/presentation of the manuscript. Overall,

I think this manuscript can make a useful contribution, but requires a major revision (including new computations) to make full justice to the new model development.

There are certain aspects where there are serious doubts whether the chosen approach is realistic. My doubts might results from misunderstandings, but even in this case this highlights issues with the manuscript. In the end, we all know that a good model fit does not ensure that the model is working for the right reasons. Therefore, it is important to clearly motivate the different equations/approaches being used.

Thank you for this assessment and the suggestions for improvement. We have revised the manuscript to make the model description more clear and convincing, emphasizing the proven and tested nature of most of the equations (specifically in sections 2.3, 2.4, 2.7, 2.9). The description of model calibration using a multi-objective evolutionary optimisation in section 2.12 is extended. To allow a deeper look 'inside' the model, we have additionally provided a sensitivity analysis of all calibration parameters with regard to the calibration objectives in the supplementary material.

Several of the equations seem to be (semi)empirical, but this is not always clearly stated (e.g. Eqs 2, 3, 14). How generally valid are equations such as Eq 13? This needs to be clearly stated. The annual variation of radiation is a simplified approach of a full geometric estimation, which also would have been possible. While there might be cases where this results in wrong results, Eq 11 might result in general the correct pattern. However, I am a bit confused by the 12 in Eq 12. Sounds like months, but I still do not see why one should divide by 12, sorry.

The empirical or semi-empirical nature of the equations was further described as appropriate. Equation 3 was improved as suggested by reviewer #3. The parametrization of Eq 13 is now better described in section 2.9. As you correctly observe, a full geometric estimation of potential radiation is possible but it would introduce an additional calibration parameter, which we aimed to avoid. The 12 in Eq. 12 are the potential hours of sunlight at the equinox to scale the potential hours for each unit. It is added to section 2.8: (12 signifying the potential hours at the equinox on an unshaded horizontal surface).

Eq 16 does not seem to agree with the common view on precip variation with elevation, where precip increases up to a certain elevation and then stays rather constant. Instead Eq 16 results in a symmetric variation below and above some elevation m. Playing around with different values (Tab1) the values also seem unrealistic (factors up to 10, i.e. a precip correction of 1000% and a rather sharp decrease of the correction factors above and below the elevation m (I got a factor of 1.0 for most elevations).

Eq. 16 was only introduced to overcome the precipitation discrepancies of the data-scarce Upper Aksu catchment (only one station with long-term high-altitude observations is located west of the catchment), and it is based on the approach by Immerzeel et al. (2012, 2015). We have made this more clear in section 2.10, paragraph 2. The maximal correction factor of 10 was corrected to upper boundaries applicable to only the Aksu catchment (6) in accordance with Aizen (1995), who report precipitation totals of 1000mm/a for the catchment at high altitudes. However, for the Upper Rhone this precipitation correction function was not necessary, and correction factors from the Swiss Hydrological Atlas (Sevruk, 1985; Kirchhofer, 2000) were used instead.

The transformation from snow to ice is not fully clear (P5L15ff). It sounds like all snow is transformed into ice at the end of the summer season (realistic?) but then only if a critical height is exceeded. Sorry, I am lost here: why is snow only transformed into ice if the height is larger than the height at which ice flow would start?

In the model, we assume that the snow pack is turned into glacial ice if the snowpack exceeds the critical height. All snow processes are governed by the snow module of the SWIM model (based on the snow module by Gelfan et al., 2004), which describes the share of ice and water in the snowpack as well. The critical height is used to determine at what point the snowpack is subject to creep and slip. The global shear stress (taken from Cuffey and Paterson (2010)) is the best estimate we can find for

data scarce catchments. This is now clarified in section 2.3.

Besides my concerns on the validity of the different equations, the study would also benefit from investigating more clearly the effect of their use. Based on the idea that a model should be as simple as possible, but not simpler, I would suggest to evaluate the contribution/importance of the different equations on the model outcome. This would also allow to better estimate the importance and potential uncertainty effects of the different parts (e.g. debris cover, precip correction,...). The way the model is presented and tested now does not allow this more detailed look 'inside' the model and provides too little motivation on why certain equations were used. For a first paper on a new model I would find a more detailed analysis of its parts highly valuable.

Thanks for this suggestion! To allow a closer look into the effects of the different parameters, we have provided a sensitivity analysis for all calibration parameters (based on their correlation with the calibration objectives) as well as an analysis of specific results produced with different parameter sets. The latter includes an analysis of the ice flow rheology parameter and avalanching. The sensitivity analysis and its results are presented in the supplementary material.

Another question is the effect of the use of units for the glacier between which the ice flow is routed. Based on the description I would expect numerical issues and thus the chosen number of units could have quite some effect, has this been tested?

If by numerical issues the exchange of ice between irregular-sized units is meant, we prevented these issues by aggregating ('cleaning') the glacier units to a minimum size to ensure that similarly sized units are produced. The elevation intervals in valleys and on hillslopes are chosen with the unit size distribution in mind. This is now better explained in section 2.2 and min., mean and max. unit sizes for both catchments are given.

Parameter optimization and uncertainty: the optimization procedure resulted in different solutions along the Pareto front. While with this approach one does not have to *assign weights to the different objective functions, it 1) can result in parameterizations which are very poor according to some criteria and 2) neglects multiple almost similar solutions at one location along the pareto front. I would recommend considering a combined objective function after all for these reasons.*

We agree that there are drawbacks in using a multi-objective calibration. However, there are advantages and drawbacks in both methods. The algorithm used here and variants thereof have been successfully used in data-scarce, glacierised catchments (e.g. Duethmann et al., 2014, 2015; Rahman et al., 2012). It is used here as a tool to find an efficient optimisation of multiple objective functions without assigning weights prior to calibration. A single combined objective function would require weighing up an acceptable error in river discharge with an acceptable error in mass balance before calibration. Since they are not known and may vary between catchments, we feel that a multi-objective approach is more appropriate here. The possibility of very poor results by some criteria is eliminated by choosing a subset of parameter sets with a minimal performance criteria. The min., median and max. values of the results are given in table 4. We have now added the minimal criteria (that were previously only briefly mentioned in the results) to the calibration section 2.12.

The authors switch partly between past tense and present tense in the description of their work (e.g. p11L18: is and L30 was), please use past tense consistently.

Thank you, that is rectified.

The conclusions read mainly as a summary and do not really summarize the conclusions of this study.

We feel that the extracted concluding sentences listed below are indeed summarising the conclusions of the manuscript (and referee 3 seems to agree: 'The paper is generally well-written, with strong conclusions that are well-supported by the model results.'):

- The new approach to representing individual glaciers and their ice dynamics in a hydrological model bridges the gap between distributed, physically based glacier dynamics models – that are typically only applicable to single glaciers or small glacier groups – and large-scale empirical glacio-hydrological models.
- This allows for accurate and integrated glaciological and hydrological assessments of entire, highly glacierised catchments.
- The intermediate complexity enables ensemble modelling approaches for calibration and scenario analysis by radically reducing computing time compared to fully distributed glacier models.
- The calibration yielded good results compared to both discharge and glaciological observations, but performance depends on data quality – precipitation observations in particular.
- The parameter uncertainty is comparable to uncertainties of glaciological observations (e.g. glaciological or geodetic area and mass balance observations) but may become large over longer simulation periods due to the variable initialisations.
- In data-scarce catchments, the model highlights the need for precipitation correction and is able to inform the method of correction by initialising ice cover and calibrating the model using discharge, glacier distribution and glacier mass balance in the multi-objective calibration procedure.
- The model helps to prevent overestimations of glacier melt in-lieu for negative biases in precipitation observations that are ubiquitous in mountainous catchments.
- The application to the arid Upper Aksu catchment shows that adequately simulating glacier dynamics (including accurate rates of accumulation and ablation) is

vital to properly model this and similar river basins due to their high contribution of glacier melt to discharge.

- The intermediate complexity of the developed glacio-hydrological model means that the model is well adapted to large, partially glacierised and data-scarce catchments, as they are often found in High Asia and other mountain ranges of the world.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-272/hess-2016-272-AC1-supplement.pdf

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