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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.

**Anonymous Referee #1**

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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 ap-



proach 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.

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.

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).

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?

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.

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?

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.

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.

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

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