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Interactive comment on "Technical Note: Representing glacier dynamics in a semi-distributed hydrological model" *by* Jan Seibert et al.

Anonymous Referee #2

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This paper describes the implementation of a simple approach to calculate glacier geometry change and retreat designed for hydrological models. This approach, the dh-parameterization, has previously been published in HESS by Huss et al., 2010 ("Future high-mountain hydrology: a new parameterization of glacier retreat"). This parameterization has already been included in various glacio-hydrological models (e.g. Duethmann et al., 2015; Li et al., 2015), its performance has been evaluated in several studies (e.g. Vincent et al. 2014, Huss et al., 2014), and has been used to calculate the evolution of all glaciers globally related to sea-level change assessments (Huss and Hock, 2015). The present paper describes an implementation of the dh-parameterization into the framework of the HBV-light model, including an example application in the Swiss

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Discussion paper

Alps.

The paper is well written and clear in most places. However, some partly important issues need to be resolved before it can be recommended for publication:

Novelty: I am a little bit concerned about the novelty of the study. The paper describes the implementation of a published approach developed for hydrological modelling into another model. Differences to the original implementation are small. The authors clearly describe the origin of the approach and make complete reference to it. For increasing the justification of publishing this article, however, the authors might try to better point out where their paper goes beyond the original study of the dh-parameterization and where the present description facilitates the application by hydrological modellers. The performance of the approach is not extensively tested so far and also the implementation of a glacier advance scheme has been implemented for the dh-parameterization by a different study (referenced in the manuscript). Nevertheless, I think there are some drawbacks to previous implementations / descriptions of the parameterization that could be more strongly highlighted in this paper: (1) How well does the glacier advance module perform? (2) How to implement the glacier retreat model if no ice thickness data are readily available? Some strategies must be provided to make the approach useful to the hydrological community (see also next comment). (3) How do the different implementations of the parameterization affect runoff (i.e. what error in runoff is committed when glacier retreat is not or insufficiently taken into account? Although (1) and (3) are somehow covered in Figure 3 the discussion is completely qualitative. The errors and their significance in comparison to the measurement uncertainties should be stated.

Ice thickness: One of the most important drawbacks of a straight-forward implementation of the dh-parameterization is the need for data on glacier ice thickness distribution. Whereas several approaches to estimate ice thickness with glaciological models have been developed in the last years (see Farinotti et al., 2017, The Cryosphere, for an overview) many hydrological modellers will not have direct access to ice thick-

ness data for their study site in the desired spatial resolution etc. The present study benefits from a data set directly provided externally by the developer of the original dh-parameterization. The present study aims at describing the implementation of the dh-approach into simple hydrological models: Without the availability of ice thickness data this is however not possible – this data is the bottleneck for the dh-parameterization! In my opinion, more effort should be invested in this paper to also describe simple strategies to overcome this restriction. Furthermore, this issue also needs to be much more prominently mentioned in the introduction and the method description. For most of the time the reader is left with no clear idea where the ice thickness is information is taken from – it just seems to be available.

Mass conservation: The dh-parameterization aims at being mass conserving which is crucial for hydrological modelling. In many implementations of the dh-parameterization, mass conservation is a critical issue and can be violated if it is not explicitly ensured. The authors should check if mass is conserved in their implementation and describe their strategy to ensure mass conservation.

Different glaciers: It is unclear what happens if different (separated) glaciers are present in the catchment. Can the authors' implementation of the parameterization only be applied to catchments that contain one glacier? What are the limitations when several glaciers are present in the catchment?

Model calibration and validation: HBV-light is applied for an Alpine catchment for a period of >100 years. It remains unclear in the present paper how the model was calibrated and validated for this application. Some more details are necessary.

Impact on runoff: see also comment above. Here, the present study using a simple and operational hydrological model could go one step further than previous studies: What is the effect of using the glacier retreat parameterization on calculated runoff? Is it possible to quantify the benefit?

Detailed comments:

Page 1, line 34: Some references should be provided here

Page 2, line 2: Actually, full hydrological models, incorporating glacier dynamics explicitly, have been published in the last years (e.g. Naz et al., 2014, HESS; Frans et al., 2016, HP). Reference to these approaches should be made, also to justify the use of strongly simplified glacier models.

Page 3, line 21: ice accumulation => snow accumulation

page 4, line 16: A transformation time of 1-3 years is too fast. Please provide a reference and choose more realistic numbers

page 4, line 17-22: The description of snow redistribution is unclear and needs revision. There seems to be quite arbitrary choices in this approach and justification is required.

Page 4, line 26: "single-valued relation between glacier mass balance and glacier area". Is this really the case? This does not make sense in my opinion and also seems to be inconsistent with the argumentation in the paper. Has the word "area CHANGE" been lost? But even then, the dh-parameerization should be prescribe such a single-valued relation.

Page 4, line 31: Here, and elsewhere. I do not like the partly very method-specific descriptions. Of course the implementation in the HBV-light model relies on a so-called "glacier profile" file. But the paper aims at providing a methodological description for implementing a glacier retreat model. So, I would avoid notions that are too specific to the authors' own model.

Page 5, line 29: Where is h_i,old taken from? (see also general comment above)

page 6, line 35: Please provide a reference for glacier area in 2010 and a more accurate number (i.e. 1-2 digits).

Page 7, line 26: Where is glacier surface geometry for the year 1900 taken from?

Page 8, top: I suggest having a kind of data section here to better organize the input

data for the example catchment

page 8, line 22: It is not clear where the initial distribution of ice thickness around 1900 is coming from.

Page 8, line 29: What is done here exactly? It seems that in addition to the dh-parameterization also volume-area scaling has been used. Please describe how and why. I strongly suggest to not combine volume-area scaling and the dh-parameterization. These are separate approaches that conceptually do not go together well

Page 8, line 31: better 900 kg m-3

page 9, line 20: Instead of using only glacier areas for model validation, the change in glacier volume would be a much better measure to assess model performance in terms of discharge. Such data would be available for the investigated catchment based on Fischer et al. (2015, The Cryosphere).

Page 11, line 16: Well, as the authors describe in the introduction, this approach already has been implemented in other hydrological models. These sentences should be reformulated to better reflect this.

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