We thank the reviewer for her/his in general positive assessment of our manuscript and the helpful comments. Below we respond (in blue) to the reviewer comments (*in black*)

The paper is well written and clear in most places.
Thanks!

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.
We find submission of the manuscript as a Technical Note to be justified as, when we implemented the method, we indeed found most previous descriptions of other implementations to be not very detailed (= reproducible). While the method itself is not new, the comparison of the different implementations of the Δh-parameterization into a widely used semi-distributed hydrological catchment model and the test of their effect over a >100 year simulation period is novel, in particular with the use of a look-up table that is generated by a pre-simulation application of the Δh-parameterization to allow the advancement of the glacier.

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?
We agree that a detailed evaluation of model performance would be beneficial, even if the Δh-parameterization itself is well-established (not novel as correctly stated above). However, this test would require data from several glaciers and the analyses would go beyond the scope of the technical note. We want to emphasize that we on purpose decided to present the new implementation as technical note and not as full paper. We have made this new model version freely available to other researchers and first studies using this new routine are appearing in literature (e.g., Van Tiel, M., Teuling, A. J., Wanders, N., Vis, M. J. P., Stahl, K., and Van Loon, A. F.: The role of glacier dynamics and threshold definition in the characterisation of future streamflow droughts in glacierised catchments, Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-119, in review, 2017). For these studies it is important, that the model implementation is clearly described in literature and these studies will then ultimately contribute to assessing the model performances in various environments (beyond what would be able for us alone to achieve).

(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).
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.

We fully agree that obtaining data on initial glacier ice thickness distribution is a challenge. However, glaciologists have developed various methods for this as nicely reviewed by Farinotti et al, 2017, and, more important, this challenge applies to any approach where glacier area is considered to change. So, unless we want to consider glaciers as static in area (i.e., infinite ice thickness) there is actually no way to get around the initial ice thickness estimation. We will clarify better where our initial ice thickness came from, how a glacier profile is defined in the specific implementation and the assumptions made, but for alternatives we think adding a reference to Farinotti et al. 2017 will be more beneficial than some (arbitrary) testing of other options.

(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.

We do show parameterization uncertainties in the figures. The ranges can be complemented with some numbers in the text if it's generally felt that this is needed. Annual rather than cumulative flow in Figure 3 may also provide an easier to interpret values. We suggest to discuss this more clearly in the revised version. Of course the significance for the simulation of total runoff depends strongly on the catchment (i.e., its glacier coverage). However, even if in some catchments the quantitative effect on simulated runoff would be relatively small, an adequate representation of changing glacier area is definitely desirable because it enables additional model validation (in terms of glacier simulation) and will help in the identification of the most appropriate snow/ice related model parameters being crucial for modeling alpine catchments.

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.

We did ensure that volume, area and thickness are related at all times in our application and match the modelled the mass balance. This is an important comment and indeed this was one of the reasons that prompted us to introduce the width scaling and to ensure mass conservation including a redistribution of water in model stores when some glacier elevation zones melt out and change landcover type (actually, we now realized that this technical detail needs to be mentioned in revised manuscript). We will explain this more explicitly in the revised version.

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?
If there are several glaciers within one (sub)catchment these are represented in a summarized way, i.e. as one glacier. But the model could, in principle, be extended to include several glaciers or the model could be applied to individual glaciers separately.

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.
Calibration and validation are described in detail by Stahl et al. (2017) and we will add some more information on this in the revision.

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?
This is an important aspect – also see our responses above: we can add some summary numbers to complement the figures in the revised version. The effect of considering changing glacier areas on runoff differs of course depending on catchment and glacier area change. We demonstrate the effect with an example here, but more examples are provided by Stahl et al., 2017. The new routine will potentially be even more important for the simulation of future scenarios beyond 'peak water'.

Detailed comments:
Page 1, line 34: Some references should be provided here
We will add these
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.
Thanks for making us aware of these studies, which we will include in the revised version.

Page 3, line 21: ice accumulation => snow accumulation
We will change this to Snow and ice accumulation

page 4, line 16: A transformation time of 1-3 years is too fast. Please provide a reference and choose more realistic numbers
We agree. What we meant was that it takes 1-3 years until snow is transformed to firn (https://nsidc.org/cryosphere/glaciers/questions/formed.html). The further transformation to ice varies with climatic conditions and can take 10 to more than 100 years. However, when simulating melt rates, we here treated the firn as ice when simulating melt rates. One can also note that this approach and even the used parameter value agree with the approach used for the snow-firn(ice) conversion by Luo et al. (2013 in Journal of Hydrology).

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.
We will expand the description, however, this aspect is not the focus of this manuscript. Snow redistribution is a challenge on its own, which is further discussed in Freudiger et al. (WIRES Water, in press – reference will be added).

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.

Thanks. In the revision we will delete the word balance, i.e. relation between glacier mass and area. Yes, the application of the Δh-parameterization leads to such a single-valued relation between glacier mass and glacier area in each of the different elevation zones (as stated in the same paragraph lines 28/29). We will clarify this part.

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.

We both agree and disagree. The issue is that we need to provide enough details. Please note that we on purpose did not mention things like file names etc.

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

h_i,old is computed iteratively, for initial value see above

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

We will provide another digit and reference. The estimates are 4.03 km^2 for 2010 (Huss pers. comm., based on Fischer et al. 2014) and 4.57 km^2 based on Paul et al. 2011

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

This is described further down (from line 26) and will be extended in the revised version.

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

We will extend the description of the input data.

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

See above, will be clarified

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

This text does not refer to the volume-area scaling implemented in the glacier routine but to the initial conditions. However, it seems that the reviewer interpreted it that former way. We, thus, realize that there are two issues:

1) We will clarify that page 8 line 29 is about the reconstruction of the initial ice thickness distribution. This text refers to the reconstruction of the initial ice thickness which is described in Stahl et al. (2017) and for which we will also provide a more detailed explanation in the revised version (see

comment above). Basically our reconstruction is mainly based on two physically-based relationships taken from Bahr et al. (1997): i) the general volume-area scaling relation ($V = c \cdot A^{\gamma}$ with $\gamma = 1.375$) and ii) a proportionality of glacier width and the square root of glacier thickness. For clarification: While the latter relation is indeed also used in our model approach to represent the change of glacier width within a certain elevation band (as explained on page 6 line 5 ff. and also applied by Huss & Hock 2015), the "classical" glacier volume-area scaling is not directly used in the modelling itself. Hence, it is not used in combination with the Δh-parameterization in the implementation of our glacier dynamic representation approach as perhaps suspected. It was solely used to derive estimates on total glacier volume for the state in the years ~1900, ~1940, and 2003. These obtained glacier volume estimates were used as data for model initialization (initial ice thickness distribution) and calibration (see observation based data in Figure 3 b).

2) Regarding combining volume-area scaling with the Δh-parameterization, we argue that the original Huss approach is somewhat unrealistic as the glacier area only changes at the glacier terminus. The scaling approach allows considering the fact that a thinning glacier in some elevation zone also causes (small) decreases in area. We do not see a fundamental problem of the use of a scaling approach to allow for these (small) areal changes.

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

Will be changed

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).

This is an interesting suggestion. However, please note that the data from Fischer et al. (2015) covers only the period 1980-2010, which makes such a validation difficult as the initial conditions might not match. Therefore this option might be less suitable after all, but we will consider it as an additional model validation. However, please note that Figure 3 b (simulated change in glacier water equivalent) already visualizes the performance in terms of change in total glacier volume. The shown observation based change in glacier ice water equivalent values for the years 1940, 1973, and 2003, which were also used for model calibration, actually are directly converted from our estimates on glacier volume based on glacier area data for those years. We agree that for hydrological modeling an accurate simulation of glacier volume matters more than that of glacier area. That is why we used glacier volume changes (as glacier water equivalent changes) for model calibration. We think an additional validation using the data by Fischer et al. (2015) might be mainly informative regarding the uncertainty of the glacier volume estimates used by us (differences compared to reference data by Fischer et al.) rather than regarding the capability of the model to represent the change in glacier volume adequately.

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.

We were apparently not clear what 'this approach' is referring to. Here we meant our approach (specific implementation to represent glacier dynamics in a semi-distributed hydrological model), whereas the reviewers' comment refers to the dh-parameterization. We will clarify this.