We thank the editor and both reviewers for their valuable comments, which helped us to substantially improve our technical note. Below we respond (in red) to the editor/reviewer comments (*in black*)

Editor

I'm sorry for the delays in this processing, a number of personal issues have come up. I find this very difficult to make an opinion on this technical note for 2 core reasons. Firstly a technical note for HESS is detailed as thus:

Technical notes report new developments, significant advances, and novel aspects of experimental and theoretical methods and techniques which are relevant for scientific investigations within the journal scope. Manuscripts of this type should be short (a few pages only). Highly detailed and specific technical information such as computer programme code or user manuals can be included as electronic supplements. The manuscript title must start with "Technical note:". For manuscripts focused on the development and description of numerical models and model components, we recommend submission to the EGU interactive open-access journal Geoscientific Model Development (GMD).

Whilst to some extent the authors acknowledge a lack of 'novelty' due to previous use of these formulations. I'm going to have to see a better justification in the final response to say why this is a standalone technical note that fits the comments about HESS technical notes detailed above. There are comments made to the extent that a paper was justified to enable the model to be 'reproducible'. I'm sorry but I then do not see enough technical depth (many which the reviewers raise) to make this so. So again more work is needed here to justify publication.

We agree that the novelty aspects are limited, but want to stress the point that our approach allows to use the Δh parameterization by Huss et al. (2010) approach for glaciers which advance during some time. This has not been feasible before. The main contribution of our TN, however, is to show how the method can be implemented into a hydrological model. In order to make hydrological modeling reproducible, the clear documentation of model routines is important. We realized that our descriptions needed to be clarified to reach this goal. For this we have significantly rewritten the manuscript and added a flowchart as new figure.

Secondly the authors responses to some very good critical analyses are very general. The author response is meant to clarify what changes will be made to answer the points raised by reviewers. That is often very limited and I have no clear idea as to what the new paper would look like at this moment in time.

We are sorry for the partly too general responses in the discussion phase. We hope that our responses below and the concrete changes make things clearer.

So recuasse of these two points then i will need to confirm publication once a clearer development of the paper and revisions is realised and more of a clear justification as to why a technical note is justified. Either due to novelty or to explain to the audience how this helps researchers directly

implement these methods and resolves some of the practical aspects in doing so.... Also the reviewers did request seeing the revised manuscript again, Jim

We are looking forward to the feedbacks of the editor and the reviewers on our revised version. We feel that this review-revision round substantially improved the clarity of our manuscript.

Reviewer #1

This paper presented a medium-complex glacier melt module and its combination with the HBV model. The idea is good and practically possible. This manuscript will be helpful for beginners of hydrological modelling in glacierized catchments.

We appreciate this positive assessment and actually assume that the paper will also be useful for more advanced modelers, who are faced with the challenge of simulating glacier dynamics in bucket-type hydrological models.

However, when I try to follow its instructions, I get lost because some critical problems are not clear. And the comparison with previous studies is not well addressed. First of all, the required data should be indicated before the implementation of the model.

We clarified the data requirements. Beyond the data needed for the hydrological modeling, i.e., precipitation and temperature and potential evaporation, one needs the initial areal extent of the glacier(s) as well as an initial thickness profile, i.e. a table with glacier area and ice thickness values for each elevation band.

Compared to study of Luo et al. (2012), what is the superiority of the Δh parameterization method? Why do you choose the Δh parameterization method compared to volume-area scaling?

With the volume-area scaling no catchment-specific information is considered, whereas the Δh parameterization allows considering elevation distributions and the thickness profile. In volume-area scaling any volume change directly translates to area changes, although this has not always to be the case. The Δh parameterization allows attributing the glacier area changes to the different elevation zones, which would not be directly possible with a simple volume-area scaling, which does not allow assigning the region of glacier shrinkage to any particular elevation zone (see also the discussion in Stahl et al., 2008). As discussed by Huss et al. (2010) the Δh approach is a simple but still physically based approach to consider changing glaciers. We added this motivation to the introduction text. We added a discussion of these advantages of the *Δh*-parameterization compared to volume-area scaling in the revised manuscript.

The statement “The Δh parameterization method is used to generate the volume-area relationship (Page 4 line 28)” is biased, as the Δh parameterization is used to generate the spatial distribution of Δh based on relative elevation.

We clarified that we here mean that the Δh approach is used to generate the volume-area relationship for the specific catchment (i.e. the relative elevations from the Huss-equations are transferred to real elevations for this particular setting)

Page 5 Lines 23 - 31 The relationship of Δh and mass balance change should be indicated more clearly.

Each annual net mass change over the entire glacier area in a catchment calculated from the semi-distributed mass balance routine is distributed to the different elevation zones using the empirical Δh parametrizations from Huss et al. (2010) – see also the detailed description in Huss et al (2010). We clarified this in the text by changing the notation ('k+1' and 'k' instead of 'old' and 'new')

How to account for the snow redistribution effects? Could you provide the detailed information, for example, with equations?

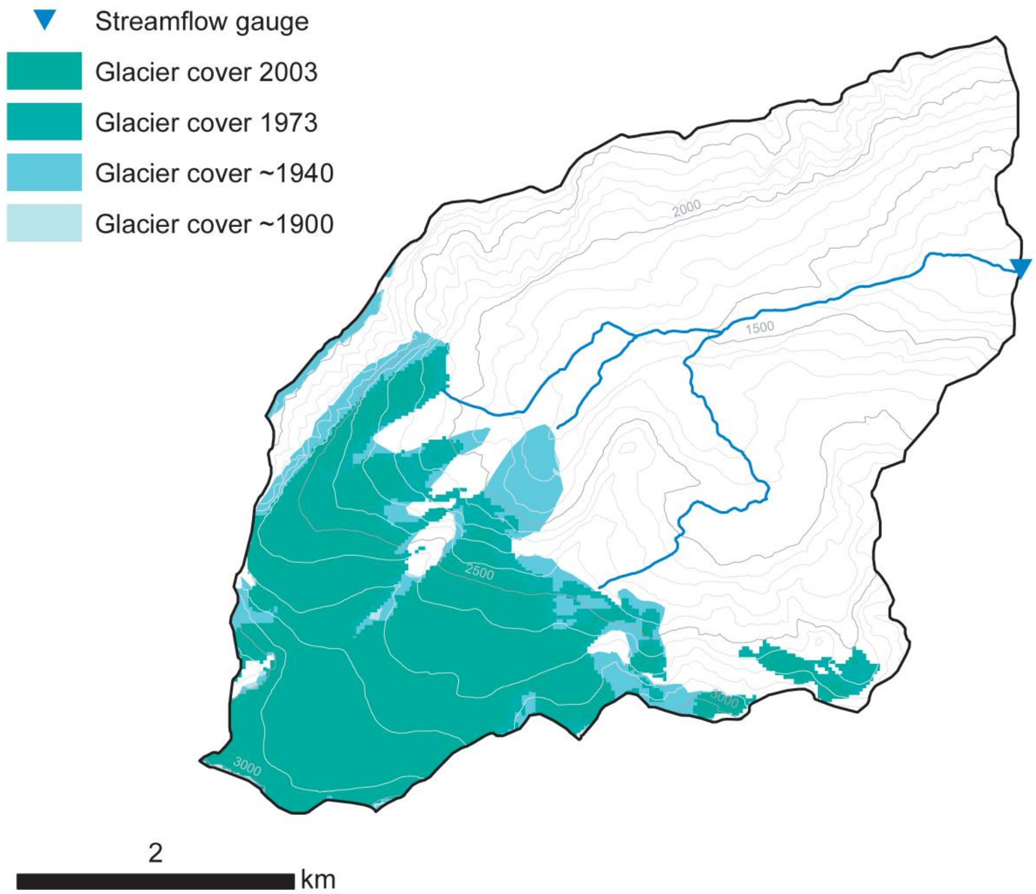
Snow redistribution is an important issue, although not the focus of this technical note. Basically, for higher elevations a threshold was defined in HBV and all accumulated snow exceeding this threshold is redistributed to lower elevations. This was already described on page 4, lines 17–22 (now on page 5 lines 12ff slightly revised). We are aware that our approach is a very simplified representation, on the other hand, snow-redistribution also is not the focus of this technical note. Basically we are assuming that the high-altitude non-glacier areas correspond to preferred snow-erosion sites and glaciers and lower-altitude zones correspond to preferred snow deposition sites. We also added a reference to a new review paper focusing explicitly on snow redistribution (Freudiger et al., 2017).

Will the glacier dynamic module take the "equilibrium line altitude" into consideration?

This is done implicitly by the Δh approach, but the ELA is not used explicitly

Provide a map to illustrate the study area in the case study.

While we agree that maps are in generally helpful, we hesitate to add a map to this technical note. After all the Alpbach catchment is only used as example. However, we added the map below in our HESS-D response. For the paper we would prefer not to add a map, but if the editor/reviewers wish, we could add such a map in the final manuscript.



There is only one glacier located within the Alpbach catchment, which is not convincing though Δh has been successfully incorporated into WASA in Duethmann et al (2015).

The glacier routine presented here was successfully applied to 49 diverse catchments in the Swiss Alps with many of them also having more than one glacier (see Stahl et al. 2017). We intentionally selected only one catchment with one main glacier for demonstration purposes for this technical note.

Stahl, K. et al. 2017: The snow and glacier melt components of streamflow of the river Rhine and its tributaries considering the influence of climate , Final report to the International Commission for the Hydrology of the Rhine Basin (CHR). [online] Available from: www.chr-khr.org/en/publications

We added this to the text.

Line 6 on Page 4: The sentence "which represents the different albedo of ice compared to snow and typically takes values of about 1 to 2" indicated that glacier melt factor is 1-2 times of snow melt factor. Is it universally true? This need to be verified. How about the debris cover area?

Thanks for pointing out that we missed to acknowledge the source of this information. Hock (2003) reviewed numerous studies and our factor is based on her Table 1. We added this reference to the text.

Hock, R. (2003). Temperature index melt modelling in mountain areas. Journal of Hydrology, 282, 104–115. http://doi.org/10.1016/S0022-1694(03)00257-9

Debris cover is not an issue in this catchment, but modelers can consider the effect if necessary by adjusting the degree-day factor for ice melt

Page 5 line 16: "the conversion of snow to ice takes about 1-3 years" needs references.

We thank for this comment which made us realize that we were not very exact in our description. 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 agrees with the approach used for the snow-firn(ice) conversion by Luo et al. (2013).

We clarified this in the text.

Page 9 line 27: I am not clear about how will the glacier retreat. When Δh is larger than or equal to the thickness of the glacier borders, the glaciers for the corresponding area will disappear?

Yes exactly: if the computed change in an elevation interval is larger than the remaining ice, the glacier area in this interval is set to zero and the model glacier will be reduced by the corresponding area. In addition the glacier area will also be (slightly) reduced for higher elevation intervals when the thickness is decreasing (Eq 7).

Page 9 Line 3: this sentence "This is due to the Δh-approach, which distributes the change in glacier mass balance over the different elevation zones, in combination with the implemented width scaling, which relates a decrease in glacier thickness to a reduction of the glacier area" is confusing. The Δh-approach is used in combination with the width scaling in this case study? The details should be illustrated in model setup section.

While in the original Δh-approach the glacier area is reduced only from the lowest elevations, we added an area reduction at higher elevations when the thickness in these zones decreased to improve the realism of the glacier area changes. We amended this sentence in the revised version to be more specific. The combination of the *Δh*- approach with the width scaling is shown in the new Figure 1 and described in detail in the model section 2.1 (assuming that the reviewer with "model setup section" refers to that section?) in the paragraph before Eq. 7. Figure 2c illustrates the glacier area over elevation bands that results from this combined application of the *Δh*-parametrization and the width scaling according to Eq. 7, whereas the illustration of simulated glacier volume over elevation is not affected by the width scaling, i.e. results from the application of the original *Δh*-parametrization method without the glacier width scaling. We tried to clarify this by adding some more explanations and additional references to the illustrations in Figure 1 in the revised manuscript.

The idea of classification of "aspect classes" seems to be a very useful conduct? What are the glacio-hydrological effects?

The use of aspect classes will cause the ice and snow melt to be faster or slower and, taken together, to spread over a longer time period. This approach has been used previously, for instance, in the ETH version of the HBV model (e.g., Hottelet et al., 1993; Hagg et al., 2007).

We added this to the text.

Hottelet, C., Braun, L. N., Leibundgut, C. and Rieg, A.: Simulation of Snowpack and Discharge in an Alpine Karst Basin, in Snow and Glacier Hydrology (Proceedings of the Kathmandu Symposium, November 1992). IAHS Publ.no. 218, pp. 249–260., 1993.

Hagg, W., Braun, L. N., Kuhn, M. and Nesgaard, T. I.: Modelling of hydrological response to climate change in glacierized Central Asian catchments, J. Hydrol., 332(1–2), 40–53, doi:10.1016/j.jhydrol.2006.06.021, 2007.

References: Luo, Y., Arnold J., Liu S., Wang X. & Chen X. 2013 Inclusion of glacier processes for distributed hydrological modeling at basin scale with application to a watershed in Tianshan Mountains, northwest China. Journal Of Hydrology 477,72-85.

We thank the reviewer for making us aware of this interesting paper, to which we refer now in the revised manuscript.

<u>Reviewer #2</u>

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 indeed found most previous descriptions of other implementations to be not detailed to be reproducible and hence find a Technical Note on this implementation warranted. While the method itself is not new, two novel aspects are (i) 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, and (ii) 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. We further describe in detail amendments such as the width scaling and the re-distribution of the 'model water'

after changing the landcover of a particular model unit from glacier to non-glacier to provide a complete description of the implementation (see also new Figure 1)

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. Actually there is a recent paper by Huss and Hock (2015) with a global assessment of the Δh-parameterization which we now refer to in our manuscript. 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, or Etter, Simon, Nans Addor, Matthias Huss, David Finger, Climate change impacts on future snow, ice and rain runoff in a Swiss mountain catchment using multi-dataset calibration, Journal of Hydrology: Regional Studies, Volume 13, October 2017, Pages 222-239, ISSN 2214-5818, https://doi.org/10.1016/j.ejrh.2017.08.005.). With this technical note we want to provide a clear description of the model implementation applied in these and future studies which 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 thickness 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 challengeHowever, 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 the model representation of 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 now added an appendix and explain better where our initial ice thickness came from, how a glacier profile is defined in the specific implementation and which assumptions were made. We benefited from ice thickness data provided by Matthias Huss (generated based on approaches included in Farinotti et al. 2017). However, the special challenge with our example was obtaining ice thickness distributions for the state a century back. For simulations starting more recent, either ice thickness data sources are available or appropriate data to apply methods described in Farinotti et al. 2017 (we added this valuable review as a reference)

(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 actually do show parameterization uncertainties in the figures (ranges in Figure 4 (3 in previous 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. Below we add a table showing the model performances of the different model variants as additional information.

NSE	GACR	No GACR	GACR-w	GACR-a
Min	0.817	0.818	0.774	0.801
Median	0.837	0.851	0.839	0.831
Max	0.855	0.863	0.854	0.843

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.

This is an important comment. We can confirm that we did 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 added some text to explain this more explicitly in the revised version (see also new flowchart, Figure 1).

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. From the perspective of glacier geometry change modelling this is a clear limitation as discussed in the second paragraph of the discussion section. But the model could, in principle, be extended to include several glaciers or the model could be applied to individual glaciers separately. However, for many hydrological applications the simulation of several (small) individual glacier areas as one model glacier may be an acceptable or even practically necessary solution as the glacierized portion of the catchment is often small and similar conceptually summarized processes are used in hydrological models for other runoff generation processes in the (larger) non-glacierized part as well.

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 added 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. 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 added four references to more complex models.

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 included in the revised version together with some more justification on why we choose a simpler approach.

Page 3, line 21: ice accumulation => snow accumulation
We changed 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). We clarified this in the text.

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 expanded the description slightly. 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, 2017– reference added in the manuscript).

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 deleted the word balance, i.e. relation between glacier mass and area (actually we realized that this had to be done at several places). 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. Section 2.1 has undergone significant revisions as a whole for clarification.

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 agree, but argue that it is important to clarify what the user needs to specify. Please note that we on purpose did not mention things like file names etc. We kept the term glacier profile in the revised manuscript also to refer briefly to the distribution of glacier thickness over elevation and introduced thisterm more clearly.

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

h_i,old is computed iteratively, we changed the notation to k+1 and k to make this clearer and added some text.

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

We provided digits and references. The estimates are 4.03 km^2 for 2010 (based on Fischer et al. 2014) and 4.57 km^2 for 2003 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 (following paragraph) and we added an appendix describing this in more detail.

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

We extended 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, see appendix

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 required for model setup. However, as this appears to be unclear, :

1) We clarified that this (old page 8 line 29) is about the reconstruction of the initial ice thickness distribution. This text refers to the reconstruction for which we 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 7, eq. 7) 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 4 b /previously 3b).

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

Changed (now in the Appendix)

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 the period 1980-2010, which makes such a validation difficult since our simulation stops in 2006 due to the available input data. Therefore this option might be less suitable after all. However, please note that Figure 4 b /previously 3b) (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. However, the focus of this technical note is not the hydrological simulation of changing glacier volumes per se, but how a change in volume can be translated into changes in glacier geometry and land cover type in the model. 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 al-

ready 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 clarified this and rephrased several sentences in the discussion section to better reflect this as suggested.