We thank the editor and the reviewer for their continued efforts which clearly help to further clarify and improve this technical note. Below we respond (*blue italic text*) in detail to the different comments (black).

Best regards,

Jan Seibert, on behalf of all co-authors

Editor comments:

The reviewer has done a swift and thorough job of reviewing the manuscript and for the most part if the authors respond to these well (as they did before) then we should be able to proceed to publication. However I want a stronger clearer statement as to how this is a technical note and therefore has the appropriate novelty before this is accepted, cheers, Jim

> *We appreciate the positive evaluation of our revisions. We want to emphasize that in this technical note we describe how existing approaches can be combined in a novel way to simulate changing glacier areas in a hydrological catchment model. To date, few of the widely used hydrological catchment models allow simulating changing glacier areas inside the model. For some of these models there are plans of implementing this (e.g. PREVAH) and hence presenting a flexible approach how to do this is timely. A novel technical aspect to the general delta h approach is the use of a lookup table, which allows advancing glaciers. It should be noted, that when we started this work and discussed different options with Matthias Huss, he raised the issue that their approach would not allow increasing areas. So even if we used their existing approach we modified it in an important way that will be of interest to the community. Furthermore, the approach of a lookup table can also be used with other glacier models, for example one could run a dynamic glacier model to simulate the melt of the glacier and in this way obtain the volume-area information for the lookup table. In other words, this approach is really flexible and allows the use of more complex models if the necessary data is available (of course still with the limitation of negligible delays between mass and area changes). We now emphasize this aspect clearer in the manuscript.*

Reviewer comments

The authors have provided a well-formulated and complete revision of their manuscript. Most of the comments raised by the reviewers were addressed and new analysis has been added to the paper. The article presents the method (glacier retreat scheme for lumped hydrological models) in a clear and reproducible way and will be helpful to the community. Nevertheless, there are some (mostly minor) issues with the present version of the article that should be addressed before final acceptance.

Thanks for this kind assessment of our revisions. We would like to stress that the approach can also simulate glacier advance and that the model should be considered semi-distributed due to its use of HRUs by elevation and aspect classes.

- Page 5, line 15: The reasoning is somewhat unclear here. Obviously, the authors have a complete snow redistribution model available (is this described in Freudiger et al, 2017? – the text seems to provide this hint) but decide not to use it and go for an extremely simple approach. I would avoid mentioning the "other" approach for removing "snow towers" and just describe what has been done.

Actually, in the review paper of Freudiger et al 2017 the challenges of modeling snow restribution are described but no 'best' or 'complete' snow redistribution model is presented.. We clarified in the text what we meant in a general way and what our approach was.

- Page 5, line 19: Please shortly explain how the parameters were chosen and if there was any possibility to validate them.

We added the values and a short motivation of these choices. Obviously, the whole issue of snow redistribution and its representation in a simple catchment model would motivate a study on its own, but this was not the focus here.

- Page 5, line 25: The "lookup table" seems to be one of the major modifications in comparison to the original dh-parameterisation. It is mentioned for the first time here but unfortunately it remains highly unclear what this table (1) actually is and does, and (2) how it is derived. Regarding (1): This is clarified later in the paper (with the aid of the figure). However, it should be outlined here. Regarding (2): The formulation calls for better explanation. "… a lookup table can be derived from any glaciological model". So, another model (a glaciological model) is needed to generate this lookup table? Which model was used by the authors? Apparently, this might quite drastically limit the applicability of the authors' proposed model implementation as not all hydrologists might have a glaciological model at hand to produce this lookup table. Maybe it is just a misunderstanding that can be clarified with a better formulation?

1) We are glad that the lookup table approach became clear now and changed the text to better describe already here what the lookup table is.

2) This is a misunderstanding. What we meant to say is that other models could also be used to create the lookup table instead of the Δh-parameterization method. We changed the text to clarify this, in particular we moved this point entirely to the discussion.

- Page 10, line 28: The area change 1900-2006 is correctly calculated by the full model. However, the two individual periods 1900-1940, and 1940-1973 are completely off. This should also be mentioned. I suggest providing the results for relative area change (observed / simulated) for all four experiments in a table. This would better allow tracking how well the model implementations work.

We compiled this information in the form of tables below.

For the text of the technical note, however, we do not find this information so helpful. The basic information is given in the Figures and we feel that providing the exact values rather confuses than helps. Please also note that we do not want to pretend that the new model routine is fully validated quantitatively.

Table: Absolute area [km²]

Reference year	Glacier area [km²]							
	Ref	GACR		GACR-a		GACR-w		No GACR
		Min	Max	Min	Max	Min	Max	
1901	6.23	5.92	5.98	5.92	5.98	6.21	6.23	6.23
1940	6.16	5.65	5.65	5.54	5.71	6.00	6.14	6.23
1973	5.20	5.05	5.15	4.74	4.84	5.71	5.71	6.23
2003	4.54	4.45	4.55	4.51	4.55	5.34	5.42	6.23

Table: Catchment glacier coverage [%] another option would be relative change compared to initial ref. area

Reference year	Catchment glacier coverage [%]							
	Ref	GACR		GACR-a		GACR-w		No GACR
		Min	Max	Min	Max	Min	Max	
1901	30.1	28.6	28.9	28.6	28.9	30.0	30.1	30.1
1940	29.8	27.3	27.3	26.8	27.6	29.0	29.7	30.1
1973	25.1	24.4	24.9	22.9	23.4	27.6	27.6	30.1
2003	21.9	21.5	22	21.8	22	25.8	26.2	30.1

Table: Relative difference of simulated glacier area compared to reference glacier data

Reference year	Ref. data	Rel. difference of simulated glacier area compared to ref. data [%]						
		GACR		GACR-a		GACR-w		No GACR
	[km²]	Min	Max	Min	Max	Min	Max	
1901	30.1	- 5.0	- 4.0	- 5.0	- 4.0	- 0.4	- 0.1	0.0
1940	29.8	- 8.3	- 8.3	-10.0	- 7.3	- 2.6	- 0.3	+ 1.1
1973	25.1	- 3.0	- 1.0	- 8.9	- 6.9	+ 9.8	+ 9.8	+19.7
2003	21.9	- 2.0	+ 0.2	- 0.7	+ 0.2	+17.5	+19.7	+37.1

- Page 11, line 9: A general comment on “glacier advance”. The authors directly link increases in glacier mass to advances of the glacier front. They do not consider a temporal delay. This strongly contradicts observations: Several years or even decades of mass gain are required (depending on glacier size and shape) until ice flow dynamics have changed in a way to make the glacier snout advance. This is completely neglected in the presented approach. I fully understand that this is not feasible, and probably also not necessary (!), for such a model, but this effect should be critically discussed by the authors and formulations should be adapted throughout the paper to not imply equality of glacier mass gain and glacier advance.

We fully agree and clarified that we make the assumption that delays between glacier mass and area changes can be neglected and that glacier retreat and glacier advance follow the same (but reverse) pattern. At the same time, as the reviewer noticed, for catchment models focusing on ice melt runoff the more complex details of delays in the glacier responses are not feasible to implement, and probably also of secondary importance. We addressed this more clearly in the discussion section.

- Page 12, line 22: This statement should be corrected. Modern remote sensing data provide elevation changes for virtually all glaciers in the world (see e.g. Brun et al., 2017, NGEO). Glacier-specific elevation changes are also available for all glaciers in Switzerland, for example (Fischer et al., 2015, The Cryosphere).

We have to agree with the reviewer that data often would be available to established individual local Δh-parametrizations. What we meant to say was that it is in practice much easier to use established Δh-parametrizations than to establish specific relationships between mass balance changes and glacier thickness distributions for future conditions of individual glaciers. We changed the text to clarify this.

- Figure 1: Nice! However, I strongly recommend enlarging text size (and reduce unfilled white spaces) to make the figure readable more easily.

Thanks, we enlarged the text size

- Figure 3c: Maybe show relative instead of absolute errors? This would be more informative. Why is there a difference for the 1900 surface?

We considered relative errors, however, deemed those as less informative as the values get large for small glacier areas in lower elevation zones even when errors are not that big (compared to other elevation zones)

The difference in the values for 1900 arise from differences during the warm-up period of three years prior to 1900, during which the glacier already decreased. We clarified this in figure 4 and the figure caption.

- Figure 4a: Why do not all runs start with the same area? Also 1900 should have a dot for an observed area. The dots should have error bars (!) and might be linked with lines to make the figure clearer.

The difference in the values for 1900 arise from differences during the warm-up period of three years prior to 1901, during which the glacier already decreased. We clarified this in the text and *added the warm-up period in Figure 4 as well as the initial glacier area taken from the historical maps (Freudiger et al., ESSDD)*

We agree that the shown glacier area values are far from certain and added the source of the individual values and error bars, which correspond to an accuracy of +/- 5 %. It is based on the accuracy given in the references Freudiger et al. (ESSDD), Paul et al. (2011), and Fischer et al. (2014). In the references for the glacier inventory 1973 there isn't any uncertainty specified, but we assume a similar accuracy of +/- 5% as for the inventories from 2003 and 2010. For the two early glacier area values, it should be noted that an inaccuracy of +/- 5 % has been attributed to the digitization of the historical maps by Freudiger et al. (ESSDD, in review), while an additional considerable uncertainty of the glacier outlines or the historical maps itself needs to be assumed, yet remains very difficult to ascertain. Hence, for those values +/- 5% should be regarded as lower bound value. Additionally horizontal error bars, i.e. temporal uncertainty, are shown in the Figure for the two early glacier area values from the historical maps. These horizontal error bars correspond to the period between the release dates of the 2 individual map sheets that cover the Alpbach catchment, i.e. 1894 and 1899 for the first value used as initial glacier area in the simulations and 1933 and 1942 for the latter value. Probably even a period extending further to the past could have been indicated, since the underlying survey had probably taken place before the release year of the individual map sheets.

We didn't add lines linking the reference glacier area point values, because of the long time differences between the points, the different data sources.

- Figure 4b: This is not observed volume! A well-constrained bedrock topography from the dataset of M. Huss is available for 1973 and 2010 but the volumes for 1940 and 1900 are derived from volume-area scaling, i.e. an extremely simple and highly uncertain model. So, it cannot be used for model validation! As already suggested in my last review, there are actual measurements of ice volume change for the respective glaciers (Fischer et al., 2015). They do not refer to the entire study period but nevertheless would allow validation over several decades.

We agree that the shown volume estimates should better not be termed observation-based. We changed the figure legend and used different symbols for the values 1973 & 2010 (based on data from Matthias Huss) and the other values. We are aware of the high uncertainties especially in the 1900/40 source data as well as in the volume estimates for the years 1901, 1940, 2003 related to the volume–area scaling but argue that these data are still better than having no information at all. Actually, these data are not used for model validation but for model initialization (initial glacier volume) and calibration. Since the model has been calibrated with the shown calibration, it cannot be used for model validation. It was not our intention to validate the glacier mass balance here, but to present the newly introduced glacier area change approach with an example dataset and to demonstrate the effect of the different tested variants on e.g. the simulated glacier mass balance. It is clear that the inconsistency or inhomogeneity due to the combination of these different glacier datasets and volumes based on simple volume–area scaling and the thickness data based on more appropriate data and the approach presented in Huss & Farinotti (2012) is problematic if one aims at getting most accurate values for individual glaciers. For our example application starting as *early as 1901 more appropriate data have not been available. We do not consider this a crucial issue for the demonstration of the glacier area change routine, but we added in the some text critical remarks on the volume–area scaling.*

The challenge with the Fischer et al 2015 ice thickness change data is, as mentioned in our previous response, that our simulations end in 2006 (due to the data availability of the HYRAS data set) As said above, for us validation of the simulated mass balance (volume changes) has not been the focus of this technical note. However, we now added the geodethic ice thickness change from 1981– 2010 from the WGMS / Fischer et al. (2015) in the figure.

- Figure 4c: Please change y-axis label. Glacier runoff is normally interpreted as the water leaving the glacier. These water volumes are much higher as they also comprise seasonal snow melt on the glacier surfaces. The authors show cumulative glacier mass loss here, and not runoff. The legend of Figure 4 should be positioned more prominently – I first didn't find it. Furthermore, I would suggest to not use model abbreviations but more intuitive names.

We agree that the term glacier runoff can be confusing as it is used differently by different authors, we used it as runoff generated by ice melt only (see also Weiler et al., early view). To avoid any confusion we changed the term to 'Glacial ice melt runoff'. We clarified in the figure caption that this term refers to melted ice only (i.e., not snowmelt on the glacier) which is tracked through the model (Weiler et al., early view).
The individual graphs of the figures were arranges closer to each other and the legend was enlarged, we think this should be prominent enough !? For consistency, we would like to keep the abbreviations for the model variants, which we define in Sections 2.2 and use in the text, when we refer to the variants. We think clear synonyms for the variants would become too lengthy for the figure legend and would make the text hard to read.

- Page 26, line 18: Combining volume-area scaling with the ice thickness distribution data set derived by a different methodology seems to be inconsistent, leading to a non-continuous evolution of glacier volume. In any case, the derived volumes for 1900 and 1940 using this method will not be suitable to validate the model regarding glacier mass change (Fig. 4b)! A better discussion is needed.

We are aware of the uncertainties in the 1900/40 data but would argue that uncertain data is better than no data at all.

See also comments re Fig. 4 b above. In the last paragraph of the appendix we already pointed to the considerable uncertainties to be taken into account. In the revised version, we added to this discussion that the combination of volume-area scaling with the ice thickness distribution data set derived by a different methodology results in additional uncertainty and that, if feasible (required date available), it would of course be better to use consistent methodolopgical approaches for all glacier data estimates.

Weiler M, Seibert J, Stahl K., early view. Magic components—why quantifying rain, snowmelt, and icemelt in river discharge is not easy. Hydrological Processes. https://doi.org/10.1002/hyp.11361