Ms. Ref. No.: HESS-2014-419
Title: Linking baseflow separation and groundwater storage dynamics in an alpine basin (Dammagletscher, Switzerland)
Hydrology and Earth System Sciences

Dear Editor,

In this reply letter, we discuss revisions we have made in response to the two reviews. We have addressed each of the comments and criticisms given by the referees. Our answers are listed below, together with specific replies and references to changes made in the manuscript. The structure was partly modified and a new figure was added to address possible misunderstanding. A Table was also added to better explain the parameters used in the calibration process. A manuscript with all changes marked (highlighted) is submitted together with this reply letter. Please note that the line numbers have been modified and that we refer to the new numbers in the responses below.

We thank the reviewers for their comments, and believe the changes based on their recommendations led to a significant improvement of the manuscript. We hope that the revised manuscript will now be accepted for publication in HESS.

Yours sincerely, Florian Kobierska (on behalf of all authors)

REVIEWER 1 – General comments

REVIEWER: This study is presenting a conceptual model of groundwater flow contributions to streamflow in the forefield of a glacier. Although the model presents an interesting data set and conceptualization of the different aquifer or reservoir dynamics and interactions hypothesized for shallow groundwater flow dynamics it is omitting and over-simplifying several hydrologic components that influence streamflow and groundwater flow over the active summer melt period to a degree that it is questionable whether the processes included in the two groundwater reservoir model are producing the right answer for the right reason. Dynamics such as changes in the active layer depth of the forefield (defining the active or drainable aquifer thickness), the unknown subsurface contribution of flow from the upslope dead ice body and diurnal snow and glacier melt contributions (how were those estimated in the model?) are unsatisfactorily considered.

AUTHORS: We agree that the presence of an active layer in the forefield could have an impact on the hydrological cycle during the summer season. Sorry, we forgot to mention that our geophysical findings implied the lack of permafrost in the forefield. This information has been added to the manuscript (lines 143-144). The lack of permafrost means that changes in groundwater levels reflect changes in groundwater volumes (rather than a change in the lower boundary due to permafrost melting). Further details on the geophysical results can be found in the PhD Thesis Kobierska (2014) which is now available online.

Regarding the estimation of the glacier melt contributions, the advantage of the mixing model is that it does not require estimating those flows thanks to the mass balance assumption of Equation 1. This study could have been improved by estimating glacial melt rates and comparing them to the results of the mixing model (by reusing Equation 1 with the modelled groundwater exfiltration and total flow). However, estimating glacial melt rates for an hourly time step would introduce further uncertainties.

REVIEWER 1 – Detailed comments

REVIEWER: The active layer depth within the forefield of the glacier is typically increasing over the summer season. It is unlikely that flow occurred in the frozen glacial deposits early in the summer season. How was this dynamic increase in the active layer depth or active thickness of the aquifer considered in the model? Because of that freeze-thaw dynamic at the beginning of the summer season flows should be entirely dominated by melt contributions before the active layer depth is large enough that greater groundwater flow contributions occur. This is contrasting the dynamics described on page 12202 (last paragraph).

AUTHORS: Sorry, we did initially omit to mention the results of other geophysical results which implied that there is no permafrost throughout the forefield. This point is now clarified in the site description (lines 143-144). Please refer to Appendix A of the PhD Thesis Kobierska 2014 (http://dx.doi.org/10.3929/ethz-a-010264039) for further explanation and figures. We added this link to the list of references.

Regarding spring / early summer flows, there was every year a fast filling of the piezometers with successive snowmelt pulse. The quite high electrical conductivity of streamwater during this period may be counterintuitive as one would expect snowmelt to present lower conductivity. However, as the soil usually does not freeze in the forefield during winter, there is quick snowmelt infiltration in the upper soil layers in spring, which creates a large interflow.

REVIEWER: The dead ice body located upslope of the forefield will continuously contribute flow to the forefield aquifer and stream. The rate at which the dead ice body is contributing flow depends on the summer air temperature. How was this flow contribution considered in the model?

AUTHORS: Melting of the dead ice body is considered as glacier melt in our model. There was no need to model this component because it is substituted for “$Q(t)$-$Q_{gw}(t)$” in the mass balance equation (Equation 1). We have made this clearer with better wording (lines 254-256) and a new figure schematically summarizing the modelling framework (Figure 3).

REVIEWER: Page 12189, Lines 15-16: I would like to see a little bit more information on the use of electrical conductivity for estimating groundwater flow contributions. What is meant by “seasonal envelopes”?

AUTHORS: The sentence was improved (line 68). By envelopes we meant the combined ranges of flow and streamwater electrical conductivity variations.

REVIEWER: Page 12189, lines 27-28: What are the two benchmark models mentioned?

AUTHORS: Those are the two partial models used to assess that our “full” model is not over-parametrized. We have clarified the text and do not use the term “benchmark” for the models anymore (lines 82-83).

REVIEWER: Page 12189: The mixing model is assuming a time-varying input of groundwater. However, it is not clear to me whether the authors assume that the electric conductivity remains constant over time in order to define the groundwater end-member or whether it is changing values contemporaneously to the change in groundwater contributions.

AUTHORS: Yes, the sentence was ambiguous and has been improved (line 72, 82-84). It did not refer to the endmembers but to the measured values of groundwater levels and streamwater electrical conductivity which are both time-varying inputs. The electrical conductivity of pure groundwater exfiltration and pure glacier melt were estimated by field measurements and are assumed constant. This is presented in Section 2.3 “Electrical conductivity endmembers” where we justify the values of those endmembers.

REVIEWER: Page 12190, line 17: Over which period was the annual air temperature estimated. Where was the temperature measured? What is the mean summer temperature?

AUTHORS: The annual air temperature was estimated between November 2008 and November 2012 (lines 104-105) and was measured at the meteorological station in middle of the forefield (see Figure 1). The mean summer (1 June to 1 November as in Table 1) temperature from 2009 to 2012 was 8.1°C.

REVIEWER: Page 12191, lines 9 ff.: I would mention that depth to the groundwater table were measured in piezometers instead of “two in groundwater tubes”.

AUTHORS: Ok, this is changed throughout the manuscript (i.e., lines 126, 128, 130).

REVIEWER: Page 12191: What was the length of the piezometers and to which depth were they installed in the forefield?

AUTHORS: The piezometers were up to 1.5m deep (lines 130-131). The deepest piezometers were at S3 and the biggest measured differential between levels in stream and aquifer was 1.33m between $S3_{near}$ and $S3_{far}$. They could not be installed deeper due to difficult site conditions.

REVIEWER: Page 12191, lines 25-29: Please add references to this section to corroborate your statement.

AUTHORS: We now only mention the situation at our site without unreferenced generalization (lines 145-149).

REVIEWER: Page 12192, line 3: Please explain what you mean by “passive aquifer”.

AUTHORS: By passive aquifer, we meant the deeper part of the aquifer which does not contribute to streamflow at the discharge station but may contribute further downstream. The flow through this part of the aquifer is still the biggest unknown in the current understanding of the hydrological mass balance at this site. We agree that the term “passive” was unclear and now use “non-contributing” throughout the manuscript (first introduced line 146).

REVIEWER: Page 12192, line 6: What was the overall range of groundwater level change and stage measured? What is the accuracy of the Hobo U20 pressure transducer?

AUTHORS: The average groundwater level range of the 5 piezometers we used and over the 4 years of data was 0.97m. Stream stage at S7 had 160 cm of variation. The accuracy of 0.3 cm and resolution of 0.14 cm of the Hobo loggers was added to this paragraph. Adjustment for atmospheric pressure variations was also performed (added to manuscript lines 159-162).

Those details can also be found in Magnusson et al. 2014.

REVIEWER: Page 12192, Lines 20-21: You assumed a snow density of 0.3. Snow density can vary greatly (0.05 – 0.7) depending on the climatic conditions and the moisture content of the precipitation contributing to snow. How did you decide for this value? Why didn't you determine the snow density from one or several snow cores or snow pits?

AUTHORS: This value is not used in any of the results. We only used a density estimate to quickly demonstrate that no single hydrological component (rainfall, snowmelt and glacier melt) dominates the site's water balance. From field experience in this area and previous modelling results (part of the modeling output data for the preparation of Kobierska et al. 2012), a density of 0.3 is a reasonable average for the snowpack at peak SWE.

REVIEWER: Page 12192, Lines 22: How was snow depth measured at the AWS?

AUTHORS: Snow depth was measured with an ultrasonic sensor "Campbell Scientific SR50". The data was temperature corrected. This was added lines 173-174.

REVIEWER: Page 12193, line 2: Do you mean water temperature here?

AUTHORS: Yes, this has been corrected (line 182).

REVIEWER: Page 12193, line 11: How did you derive the different EC zones? Please explain the method/approach used.

AUTHORS: Zones L1, L2, H1, H2 and H3 serve as a visual representation of low and high EC zones based on 238 single EC measurements (Table 2) and previous work by Tresch (2007) at this site. Only the endmember EC values impact our model and not the extent of those zones. Naturally, the ruggedness of the field site did not allow measuring groundwater and glacier melt electrical conductivities everywhere in the forefield. This justification has been added to the manuscript (lines 192-197).

REVIEWER: Page 12195, line 18: How are the two groundwater reservoirs connected? Is there percolation/exfiltration between the two reservoirs? Do both reservoirs contribute to streamflow or is only the fast reservoir contributing to streamflow in the summer? I see that some of these points are clarified in section 3.2.3, however, I would suggest stating those key assumptions earlier on.

AUTHORS: The whole 'Model' section has been reorganized. The reservoirs are explicitly not connected but they both contribute to streamflow. How the 'slow' reservoir is recharged during the main season to provide the $baseflow_{max}$ of 70 l/s is not required for our model. Both the side moraines and the 'fast' reservoir would contribute (lines 281-287).

REVIEWER: Page 12195, line 19: It is important to mention here that the slow reservoir, although remaining constant (or full – constant is a bit confusing here!), is still contributing to streamflow at a constant rate. The current wording gives the impression that the slow reservoir is not contributing to streamflow during the summer at all.

AUTHORS: OK, the wording was improved (lines 281-287).

REVIEWER: Page 12196, lines 6-8: Did you measure electric conductivity in the stream throughout the winter? This would have provided an indirect way to quantify flow in the winter using the EC-based mixing model.

AUTHORS: We measured electrical conductivity for as long as possible, but data became unreliable every year between November and December as the sensor ran dry or was obstructed by ice. Maintaining a sensor in a location that could provide both summer and winter measurements would be very difficult. If placed deep into the streambed, the sensor would be unattainable in summer due to very high and turbulent flows. During most of winter, getting to the forefield is too risky due to avalanche danger. Moreover, digging through the snowpack to reach the streambed presents the danger of falling and drowning.

REVIEWER: Page 12196: Equation 5 states how the integral water level was estimated based on all available groundwater level measurements. How was the initial groundwater storage estimated? Did you use any of the geophysical data (e.g. the estimated average aquifer depth of 10 m) to determine the groundwater storage size? Also you mention that only piezometers far away from the stream were used to estimate the integral water level. Was the water table measured in near-stream areas always near the soil surface or why were those piezometers not included in the estimate?

AUTHORS: This is one of the limitations of the study mentioned in the discussion. Our model gives some understanding of a realistic reservoir volume but does not allow fully characterizing it. The initial groundwater storage was estimated based on the area of the forefield (see lines 432 and 499-506) and the maximum of $L_{integral}(t)$.

To represent a balanced spatial average we selected only one piezometer per transect. We chose the far piezometers since they were least affected by stream level variations (lines 327-329).

REVIEWER: Page 12197, line 6: How did you estimate the residual water storage volume?

AUTHORS: The residual water storage was calibrated independently of the other parameters to ensure the best possible validation performance. Without adding a residual water content, we initially noticed that the calibration for 2011 was good but did not translate well for the validation over the remaining years (Table 3). By analyzing the groundwater data for this year, we realized that the catchment was much wetter at the end of the season than the other years in record. It is for this reason that we introduced the residual water storage and it proved satisfying as calibration performance only decreased a little compared to the corresponding increase in validation. We have now included those precisions and a new table explaining this process (see Section 4.1, Table 3 and Table 4).

REVIEWER: Page 12198, line 4: What are the remaining years? Please add time period in parentheses.

AUTHORS: This was clarified as follows: “The model was calibrated for each full hydrological year (four years from 2009 to 2012) and validated with the three remaining years” (lines 375-376). The validation years are also explicitly written in Tables 3 and 4.

REVIEWER: Page 12198, line 22 ff.: Why do you keep the groundwater exfiltration rate constant in equation 3 instead of keeping the end-members constant (e.g. low EC for melt runoff, high EC for groundwater) and use a simple 2-component hydrograph separation to estimate groundwater contributions to streamflow? If you measured EC in the streamwater, snow/glacier melt and groundwater over time one doesn’t have to know the groundwater flow rate to determine the contribution to streamflow in Equation 3.

AUTHORS: The groundwater exfiltration rate is not kept constant in Equation 3. It is a function of time and is presented in the next paragraph 3.2. The EC values of the end-members are kept constant and the glacier melt contribution is not modelled (as mentioned in our response to the general comments). With those variables and assumed constants, the 2-component hydrograph separation allows us to estimate the groundwater contribution to streamflow. By doing so we estimate aquifer properties based on our groundwater exfiltration model. We could have presented it differently by explicitly excluding total streamflow from the calibration procedure and focusing on modelled groundwater against measured groundwater (assuming a perfect mixing model). This would however have yielded the same calibration results. We wanted to visually see the effect of the groundwater model on total streamflow, which enabled us to better understand model deficiencies as explained in section 5.4.

REVIEWER: Page 12199, lines 4-5: Unclear wording. I don’t think “compensate” is the right word here.

AUTHORS: This section was partly deleted and ‘compensate’ was reformulated (lines 397-402 and later 431-437).

REVIEWER: Page 12201, lines 13-16: This part is confusing. Just state that you estimated the groundwater contribution from the slow reservoir from the baseflow recession and that this storage is linearly draining at a rate of 0.07 m3/s and that this value was used to define the constant exfiltration rate. There are two many terms introduced that are describing the same hydrologic component (e.g. exfiltration rate, baseflowmax). Try to reduce the number of terms for the sake of consistency.

AUTHORS: We have reworded this whole section (section 3.2.2, lines 301-321) but kept the term $baseflow_{max}$ because we used baseflow as the output from the 'slow' reservoir at any time, also when it is less than this maximum value.

REVIEWER: Page 12202: It would be interesting to see a figure comparing the hydrographs of observed flows versus PEC and PGW.

AUTHORS: Please see below the figures comparing P_{EC} and P_{GW} with measured discharge. We can see that P_{EC} yields much better results than P_{GW}. For now, we did not integrate those extra figures in the manuscript because we think that the performance values in Table 3 are sufficient and that the interested reader can also access this response.

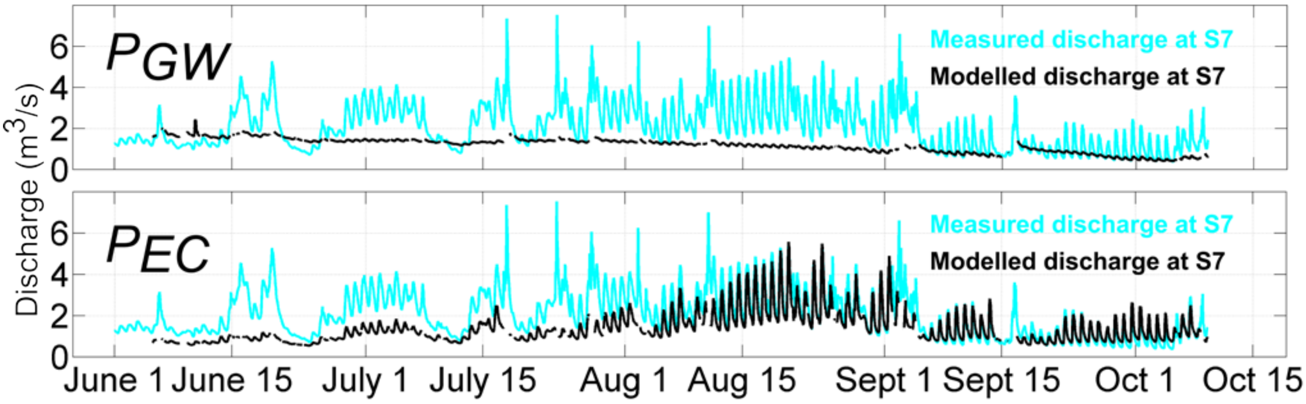

Figure 4'. Measured and modelled discharge (m^3s^{-1}) at S7 for the partial models P_{GW} (upper panel) and P_{EC} (lower panel).

REVIEWER: Page 12202, lines 20-21: The authors state that the modeled stream width is a function of groundwater exfiltration and that this is suggesting that the mixing component is well parameterized. Where is the data shown that is supporting this conclusion? I don't see estimated stream width in either a table or figure. Is the modeled stream width supposed to range between 5 and 14 m as stated in line 14 (same page)? Even though braided rivers are "hard" to measure one could have attempted to provide a comparison to field measurements. There are always spots where braided rivers have confluences where one could measure stream width and depth. This is otherwise a far-fetched statement.

AUTHORS: We have added the stream outline to Figure 6 (now Figure 7) following a request from the second reviewer. It illustrates the difficulty of defining stream width for such a braided network of stream reaches. We did measure a few sections but any reasonable accuracy is illusory. We have deleted this section of the paper, as both reviewers expressed strong doubts and the value it brought was limited.

REVIEWER: Page 12205, line 1. The authors state that the piezometers were "empty" by the end of the season. How deep were the piezometers? Are you sure you didn't just see empty piezometers because they were not installed deep enough?

AUTHORS: The piezometers were only up to 1.5m deep because difficult field access and rugged terrain did not allow installing deeper wells. Yes, at the end of the season, the piezometers appeared empty because they were not deep enough. This is for this reason that we introduced a calibrated residual water content value in the fast reservoir at the end of each season. As mentioned in the discussion, this is a limiting aspect of this study. However, considering our results with those experimental conditions, we think that our methodology could be very interesting to test in better instrumented sites.

REVIEWER: Page 12205, lines 17 ff.: I would add that this deeper reservoir has an “active” volume of 1000m by 400m by 1.7m. The authors should mention in the site description whether permafrost exists in the forefield and how active layer depth changes over the season. Forefields of glaciers are typically characterized by several dead ice bodies or saturated moraine or glacial till material that is frozen during the winter. Thus during the spring snowmelt, runoff occurs on top of frozen and supersaturated soils. The hydrogeological description provided in section 5.2 however implies that the moraine deposits in the forefield of the Damma glacier remain unsaturated for most of the time except for the summer melt season. This should be discussed.

AUTHORS: Ok, “active” has been added (line 519). Yes, as mentioned earlier, we are now more explicit about the lack of permafrost in the site description. This area of the Swiss Alps is characterized by heavy snowfalls which lead to unfrozen soils during winter. This is why the soil becomes completely unsaturated by spring. Soil moisture and soil temperature data at the AWS support this conceptual model of the forefield.

REVIEWER: Page 12206, line 12ff.: Rainwater has an EC of 6.05 µS/cm. If rainfall occurred during times when the groundwater reservoir was “half-empty” the infiltrating rainwater would quickly mix with the groundwater body in the aquifer causing a dilution of the 15.1 µS/cm groundwater while at the same time contributing to streamflow. Since contribution of groundwater to streamflow is delayed how would this “dilution” of the groundwater EC-values influence the uncertainty of the model?

AUTHORS: Yes, this is a fair point we were not able to completely verify. However, from the analysis of the groundwater data and our field experience at the site, we suspect that a negligible amount of rainwater infiltrates into the surface aquifer. Most of the rainfall directly runs off to the stream.

In our opinion, only small flatter sections of zones H1 and H2 present zones where rainwater infiltrates. At H1, those small rainwater infiltration sections did not affect the electrical conductivity endmember of groundwater exfiltration as springs showed constant EC throughout one summer of continuous measurements. Delayed rainwater seeping was indeed observed with negligible flows compared to the main springs in zone H2, thus not having a dilution effect. Moreover, the electrical conductivity endmember of groundwater at our site is a very low value which seems to be attained over short flow paths in the underground (i.e. zone H3 mainly fed by late seeping of snowmelt in the vicinity).

REVIEWER: Page 12207, line 5 ff.: This statement is simply not true. There are several energybalance based hydrologic models that work satisfactorily with available meteorological data (see references below for a few examples).

G. Jost, R. D. Moore, B. Menounos, and R. Wheate. 2012. Quantifying the contribution of glacier runoff to streamflow in the upper Columbia River Basin, Canada. HESS, 16, 849–860.
Reijmer, C. H. and R. Hock, 2008. A distributed energy balance model including a multi-layer sub-surface snow model. Journal of Glaciology. 54, No. 184, 61-72.
Hock, R. and B. Holmgren, 2005. A distributed energy balance model for complex topography and its application to Storglaciären, Sweden. Journal of Glaciology 51(172), 25-36.
Hock, R., 1999. A distributed temperature index ice and snow melt model including potential direct solar radiation. Journal of Glaciology 45(149), 101- 111.

AUTHORS: The aim of this paragraph was to emphasize the difficulty of running physical hydrogeology models in such an environment due to the lack of soil data. We just wanted to illustrate this point by saying that it is difficult enough to run energy-balance models for surface hydrology in such a catchment (‘alone’ replaced by ‘already’ line 570). We did not mean that those models are not up to the task. The model ALPINE3D, developed in our institute, is also a distributed energy balance model well suited for such a site, but only if it can be fed with accurate, highly distributed meteorological data. The article by Kobierska et al. (2012) presents some of the difficulties involved in running high quality simulations for such a site. The objectives of this present study would have required an unattainable modeling precision given the lack of fully distributed meteorological data over the entire catchment.

REVIEWER: Figure 5: Is the reservoir depth plotted in Figure 5 showing the reservoir depth of the slow, the fast or both reservoirs? When is the reservoir considered to be full? Please indicate with a threshold.

AUTHORS: Figure 5 (now Figure 6) only deals with the slow reservoir. The reservoir is full at a depth of 1.73m, which is at the very end of the presented time period. We have now marked this level on the figure and mention ‘slow’ reservoir on the Figure to avoid the ambiguities you raised.

REVIEWER 1 – Minor comments

REVIEWER: Page 12190, line 10: Insert “the” after “a small piece of”.

AUTHORS: Done (line 96).

REVIEWER: Page 12192, line 21: Replace “Cumulated” with “cumulative”.

AUTHORS: Done (line 106).

REVIEWER: Page 12198, line 5: Suggest using “EC data” instead of just “EC”.

AUTHORS: Done (lines 72, 239).

REVIEWER: Page 12206, line 10: replace “a” with “of” before “four”.

AUTHORS: Done (line 546).

REVIEWER: Please use the term “piezometer” instead of “tubes”. I find the word “tubes” very unspecific.

AUTHORS: Done throughout the manuscript.

REVIEWER 2 – General comments

REVIEWER: The strong point of the study is the intensive data sets on which authors have formed their model. However, how the idea has been implemented is very questionable. Many parts of the manuscript need to be revised as they are either difficult to understand or they have been poorly explained. To my view point, the paper requires major revision. The parameters which are calibrated in this study are not defined clearly. Also, the value for some of these parameters such as residual water storage are not known. It is suggested that authors come up with a table in which all the calibrated parameters and their values are explained.

AUTHORS: The issue of the residual water content was also raised by reviewer 1. We have added details and justifications regarding this parameter (lines 413 to 420 and a new Table 4).

REVIEWER: It is assumed that exfiltration occurs from the side of the river due to gradient. Line 7 page 12199 states that infiltration is happening from stream to the aquifer. This is not clear if this infiltration is assumed to be vertical from the bottom of the river to aquifer or it can happen laterally as well. If it is also lateral, then assuming a constant width is not a correct assumption and this issue may explain the large contrast between the modeled width (5 to 14 meter) and the one the one reported by another research (24 meter).

AUTHORS: The infiltration was assumed to be vertical from the bottom of the river to the aquifer. This whole section has been deleted because we realized that it brought more confusion than added value.

REVIEWER 2 – Specific comments

REVIEWER: Page 12195 line 9: The word **previous** should change to **next**.

AUTHORS: Done (line 254).

REVIEWER: Page 12195 line 9: It has been written that equations (1) and (2) yield equation (3). This statement does not seem to be true. Is it assumed that the discharge due to glacier melt is ignored. If this is so, this section should addressed why glacier discharge was excluded.

AUTHORS: Discharge due to glacier melt is not ignored. It does not need to be determined explicitly because it is instead estimated from end-member mixing of EC (equation 3). Equation 1 is solved for the melt rate, and this is substituted into Equation 2, yielding Equation 3 (more explicit formulation lines 254-256).

REVIEWER: Line 24- 25 page 12205: why is it difficult? This statement needs a justifiable reason.

AUTHORS: Reviewer 1 asked a similar question regarding the passive aquifer which we now exclusively call 'non-contributing'. It is difficult as both the depth of this storage and its hydraulic properties are unknown and technically challenging to determine. This point has been added to the manuscript (lines 530-531). It however does not affect the validity of this study as this deep groundwater flow is an 'invisible' part of the hydrological balance in this first order catchment.

REVIEWER: It is highly recommended to avoid repetitions. Page 12197 section 3.2.3 should be revised as two sentences are saying the same thing.

AUTHORS: OK, we have improved this section (now section 3.2.4).

REVIEWER: Equation (9) assumes that gradient is one. However, there is no explanation why this assumption holds.

AUTHORS: We assumed that infiltration happens into completely saturated media with no extra hydraulic gradient (surface water depth is neglected). This part has however been deleted from the initial manuscript.

REVIEWER: I disagree with lines 18:20 on page 12201. The model underestimates most of the time and I suggest that the explained reasons in section 5.4 to be presented in section 4.2 to describe this inadequacy.

AUTHORS: Yes, the model under-estimates most of the time; except when streamflow is almost uniquely constituted of glacier melt. We now mention those points more explicitly in Section 4.1 and refer to the discussion for further details (lines 422-429). Mid-summer days with high peakflows and substantial flow amplitude strongly influence the calibration procedure, whether based on the Nash-Sutcliffe efficiency or relative error.

REVIEWER: The position of the river in figure 6 should be known. It is not obvious in that picture.

AUTHORS: We have added the position of the stream to the figure (now Figure 7), including groundwater springs in H1, H2 and H3. The stream outline also illustrates the difficulty of defining stream width for such a braided network of stream reaches.