Authors response - hess-2022-110

Towards a hydrogeomorphological understanding of proglacial catchments: review of current knowledge and assessment of groundwater storage and release in an Alpine catchment

General comments

We thank both reviewers for their careful and constructive comments on our manuscript. We attempted to address all discussed issues in a new version, which has been largely reshaped. While the core messages and the methods did not change, we re-organized better some sections and improved some parts of the methodologies and results. We will first give here an overview of the main changes of each section and then provide a point-by-point response to both reviews' comments.

Main manuscript changes and new structure

1. Introduction

As suggested by the reviewers, we discarded a large part of the literature review provided in the introduction. The new introduction is now shorter and only includes one or two paragraphs summarizing the main landform dynamics. As a result, we also changed the title of the manuscript to remove the review aspect. We decided to keep Fig. 1 (overview of landforms), as we think it helps to clarify the understanding of the processes discussed in the paper. We upgraded the figure following the reviewers' comments.

2. Study site and experimental methods

This part was not changed much except for the ERT section, where we included some additional description. We also highlighted more clearly that all datasets are available on Zenodo for further details. We also upgraded Figure 2 (overview of the catchment), by performing a more advanced methodology to classify landforms (see methods).

3. Methods

We now introduce this section with a figure summarizing the workflow adopted in this study, attempting to bring clarity in the methods used in this study.

We moved the section "Estimation of hydraulic conductivity in the outwash plain", to the first part (Sect. 3.1) of the methods, as this part is related to field methods introduced previously.

The Section "Assessing the hydrological response based on aquifer characteristics and recession analysis", was split into two parts to better describe the two different analyses : we now first (Sect. 3.2) introduce the general concepts of the recession analysis and describe the methodology of the <u>catchment-scale</u> recession analysis. In this section, we also moved part of the information which was wrongly included in the results previously. Table 1 on hydraulic conductivities of different landforms was moved to the results part.

Then a second section (Sect. 3.3) describes the recession concept that we used to characterize the recession time scale and storage-discharge of <u>individual landforms</u>.

Then a new section (Sect. 3.4) describes the newly adopted methodology for superficial landform classification, where we now combine a DEM and orthoimage to perform a supervised random tree classification method.

Finally, the last section (Sect. 3.5) where we describe the simple model of proglacial landform dynamics, was moderately updated to include some missing information. We also now have included the code and data for the snow mass balance model in the supplementary material.

4. Results

We now start by introducing the field observations and moved the catchment-scale recession analysistothe end of this section. We now start (Sect. 4.1) with EC observations. We attempted to be more concise in the description of the observations and discarded or moved some parts to the discussion. The figure showing Streamflow EC (new Fig. 4), was updated and improved by adding an additional year of data which provides some additional evidence of a missing storage.

Sect. 4.2 introduces the groundwater dynamics in the outwash plain. Here again, we discarded or moved some parts to the discussion.

The sect. 4.3 shows the hydraulic conductivity estimates. Here we now included a new figure (Fig. 7) to introduce an ERT profile where we highlight the bedrock and aquifer, as requested by the reviewers. The rest of the section was updated to move some information into the methodology or discussion.

We then introduce the results of the landform-based model (sect 4.4). This section introduces the previous Table 1 (review of hydraulic conductivities). Then, the newly estimated areal extent of the landforms in Otemma were updated based on the new classification method (Table 3). Additionally, we included an uncertainty margin in our model of landform storage by including a range of min. and max. hydraulic conductivity for each landform (Fig. 8). Results remain in general comparable to our first draft manuscript.

Finally, Sect. 4.5. shows the catchment-scale recession analysis (which was previously the first section). Here, some significant parts were moved to the methods. The new Fig. 11 shows the same results but was reshaped to be more compact.

5. Discussion

The discussion was improved in some parts, some key information from the literature were also moved to here. In general the content is similar.

Sect. 5.1 now discusses more in-depth the simple modelling approach. Sect. 5.1.1 to Sect 5.1.3 remain similar with some updates to be clearer and more concise. Sect. 5.1.4. was improved by including some parts previously included in the results. Sect. 5.1.5 (Missing storage) : remains similar, we included some more justification for this missing storage and the limits of the catchment-scale estimates.

The section introducing the perceptual model was split in two for more clarity. Now Sect. 5.2 discusses landform connectivity (little changes with previous manuscript). Sect 5.3 now retains the information of the perceptual model. We added a bit more discussion here and tried to better explain our perceptual model. Most notably, the reviewers made some comments on the figure. We did not significantly change the design of this figure but improved the clarity of how it was designed and how it should be interpreted. (see detailed response below). We also removed the term proglacial since our approach was performed for the entire catchment, which is includes paraglacial landforms such as bedrock and talus slopes.

6. Conclusion

Some wording waschanged to improve clarity but it remains largely similar.

Point-by-point response: Anonymous Referee #1

Note: References to Figures in the authors' response are based on the number from the new manuscript.

Major revisions

Reviewers' comments	Authors' response	Changes in manuscript
The work is **too long** and fair-winded. Some parts are not essential, and can be condensed, moved to supplementary or just removed (see suggestions below). I suggest to shorten the work of at least 1/3, to get it more readable	We removed a large part of the introduction (the review) which significantly reduced the work. Some parts were also better organized which now avoids redundancies between sections. However, we also included some additional information upon the reviewers comments which made some parts a bit longer. The current manuscript contains ~10 300 words, against 13040, so about 1/4 shorter. Although it is still somewhat long, we believe that all parts are now well organized and are useful for the purpose of the current manuscript.	• Remove a large part of the introduction • better organize some parts and remove redundancy
The work has a **weird structure**, being a combination of a review paper (with several drawbacks as written) and a research work (which is better presented and written). I suggest to **discard the review part**, and shift to a research paper offering a brief meta- analysis in the discussions. Also, the review part do not offer a particularly innovative view when compared with other works (e.g., Hayashi 2020), and the text as well as the main figure could be improved. Under my suggested reshaping, I **suggest discarding Figure 1**, in part redundant with Figure 11, and **move table 1 to discussions** (because part of your meta-analysis)	As discussed, we agree to **discard the review part** largely. Only some key information were included in the introduction (2 paragraphs). **We decided to keep Figure 1** at this stage, as we think it helps the reader to get a quick overview of the different landforms which we discussed in the manuscript. This figure is simple and can be read quickly, not significantly increasing the necessary time to go through the paper. We nonetheless improved the figure based the authors' other comments. Concerning **Table 1** (review of hydraulic conductivity), we agree to **move** this figure to the results part, as some information are used in our modelling framework. We believe that the new manuscript now successfully shifted to a research paper.	• Removed a large part of the review section and moved only key information to the discussion and a brief overview in the introduction. • Updated Figure 1. • Moved Table 1 to results.

Reviewers' comments	Authors' response	Changes in manuscript
Methods and results parts can be condensed quite a lot. Also, some parts of the results belong to methods or discussion, i.e., the description and discussion of chosen models and tools in methods and data interpretation in the discussion. I highlighted only some of these parts in the pdf file but please shorten and move the text to its correct position in the manuscript	We made an effort to re-organize the manuscript in a clearer way. We carefully checked and moved parts from the results which belonged to methods or results. (See above *Main manuscript changes and new structure).*	• Move parts of results to methods. • Better organize the methodology with a clearer structure • Include a new figure summarizing the workflow to improve clarity (Fig. 3). • Move some parts of results to discussion or remove if redundant.
The work has some **typos**. I highlighted some of these, but please carefully read the work to check these errors before sending for review	All authors read the last version of the manuscript to remove typos.	• Correction of typos

Supplementary comments in the pdf

Reviewers' comments	Authors' response	Changes in manuscript
Title : suggestion of new title.		We changed the title to remove the review component
Line 5 : not necessary part in the abstract		We removed this sentence, as well as adapted the part concerning the review
Line 21: please reshape it, little bit twisted		We changed the sentence and removed the temporal aspect
Line 26: unclear what you mean		Changed to regional diversity
Line 33: this part can be condensed a lot…also, why you refer to the Andes?		Removed the references to Andes. Shorten the rest.
Comments line 66 to 160		This part was removed
Figure 1: The legend is missing. what about other proglacial features that you describe inthe text? 1) seems a terminal moraine		• We adapted the figure and removed the beige area which was not clear. • We included rock glaciers • We changed the lateral moraine location • Legend is in the caption

Reviewers' comments	Authors' response	Changes in manuscript
Line 241 : this sentence sounds a little bit arrogant...can you reshape it and provide a reference for your strong statement?	We decided to include this remark as this paper (Clow et al., 2003) is usually cited as a reference (e.g. Hayashi, 2020) but has a fundamental mistake. We contacted the authors and journal who did not agree to publish an erratum. The error comes from their recession analysis where they wrongly included time ($S_g=Q_0*t/\alpha$) leading a large overestimation of storage in the talus.	• This comment was moved to the discussion and adapted.
Line 254 : you can condense it		• This was shorten and included at the end of the introduction
Line 275 : Tyrolean-type water intake, what is it ?	It is a type of bottom intake	• We included a reference describing Tyrolean water intake
Line 288 : (weather station) who installed it?	We installed it.	• Changed wording, added more precision
Line 300 : (gauging station) who did it?	We installed it.	• Changed wording
Line 311 : please add the ERT lines in the map. who did the measuements? Have these been published? What instrument was used	We performed the ERT measurements. Data are published on Zenodo, but this was only indicated in the data availability final section.	• Added description of the ERT device, methodology of data acquisition and inversion • Added a reference to Zenodo • Moved all information regarding ERT in Sect. 2.4
Line 452 to 599	We thank the reviewer for highlighting some misplaced parts, we reviewed and moved them to their correct section	• Re-organize all text parts which did not belong to results. • Removed useless wording
Line 669 : perhaps worth merging this part with the previous one? Moraines are glacial deposits...	Although they are both moraine materials we decided to keep them separated as their slope induce somewhat different hydrological processes. Merging them would not match the logic of the paper we used so far.	• Unchanged
Line 695 : i think that here you should discuss also permafrost ice as a potential "missing storage"...even though it is unlikely that it represents a considerable part of it	We agree that we have not mentioned permafrost or rock glacier	• We added a paragraph to include those features in the discussion (Sect. 5.3)
Line 700 : what is the difference with the second hypothesis? please define it better		• We added more clarity to that sentence

Reviewers' comments	Authors' response	Changes in manuscript
Figure 11 : in my opinion, you should substitute "water storage" with "water source" or something similar...you focused on water outflowing the landforms, and not on the water stored in the hydrological compartment...thus, snow is actually snowmelt, glacier is glacier ice melt, rain is rainwater...	We agree with the reviewer. We changed the legend to "Water source in landform", as well as the description of the source. We updated the table to make clearer how the model was designed (arrows are based on new Fig. 12; pies from our simple model). We changed talus slope to steep moraine in this new version, as the new classification method led to less talus and more moraines.	• Changed legend to "Water source in landform" • Changed snow to snowmelt, ice to glacial stream, rain to rain water. • We updated the caption to clearly indicate • We included a paragraph in the methods • We updated the figure (increase lateral moraine, changed beige area to bedrock)

Point-by-point response: Anonymous Referee #2

Major revisions

Reviewers' comments	Authors' response	Changes in manuscript
The manuscript presents two interrelated but separate pieces of work in one large package: (1) literature review of proglacial landforms and (2) case study of storage assessments in an Alpine catchment. As such, the manuscript has an **unusually large volume**, approximately double the size of standard journal papers. The literature **review is informativ**e, but it does not offer much new insights. Therefore, I suggest that Section 1 (Introduction) be reduced to 10-15% of the current volume.	We admit that the review part is rather long but our intention was to bridge the gap between existing hydrological and geomorphological reviews. The hydrological part certainly overlaps strongly with the work of Hayashi (see also Reviewer comment 1). We believe the review is useful to provide an overview of geomorphological and hydrological processes for non-expert readers, but may be too long for others. We agree that the review can be more strongly synthesized by keeping only key information about the proglacial landforms and their hydrological dynamics. Some part of the review was also repeated in the discussion, which we improved.	• Removed review part from introduction • Include one paragraph to provide a short overview of proglacial and periglacial landforms and their hydrological processes • The introduction was shorten to about 25% of the previous volume
That will still leave a much larger volume of texts compared to standard research papers, meaning that the **rest of the manuscript will have to be condensed substantially** to make it more concise and useful to the intended readership.	We agree that some parts of the documents were not well organized, some parts were rearranged in the right section and some text has been reduced to be more concise.	• Reduced introduction • Reorganize results and move text parts to relevant section (method or discussion) • Final manuscript reduced by 1/4

The **case-study** part of the manuscript presents **unique** and **interesting information**, which will be of great interest to the reader of this journal. However, it has s**ome fundamental issues** that need to be addressed before the manuscript can be considered for publication. Overall, the case study needs to re-examine some of the assumptions that are central to the results. I will elaborate more in my specific comments below. Particularly important issues are indicated in my comments on Line 420, 660, 696, 698, and 718	We thank the referee for his/her helpful comments. Generally, we think that parts of the results were not entirely clear, especially regarding the modelling parts. We also realized that we introduced some confusion by refereeing to "proglacial" landforms, while we were actually refereeing to the entire glaciated catchment. The answer to the specific comments is given below.	• Improve the description of the methodology • Add a graph with the workflow • Clear confusion between proglacial and catchment-scale results

Specific comments

Reviewers' comments	Authors' response	Changes in manuscript
Figure 1. What do beige areas between bedrocks represent? Are they same as beige areas inside the lateral moraine? Please clarify that in the figure legends.	We reviewed the figure and removed the undefined beige areas, which is now mostly replaced by bedrock. We also added some other features such as rock glaciers.	• Update the figure and remove undefined areas
Line 271. Please include the latitude and longitude of the study site. This will allow interested readers to look up the sites easily on Google Earth and other programs.		• Added coordinates (45°56'3''N,7°24'42''E)
Line 288. Please indicate the weather station in Figure 2.		• Update figure 2 to show the location of weather station
Line 291. Please indicate the elevation of the weather stations and their direction (e.g., northwest) with respect to the glacier snout.		• Added elevation (2450 m a.s.l.)
Line 305. If these devices are fully screened, they are not piezometers. Please use proper terminology, such as water-table monitoring wells.	Thank you for the comment. We used *groundwater wells* or simply wells.	• Changed piezometers to groundwater wells in the whole manuscript
Line 312. Please present more detailed information on the ERT methodology, for example, electrode spacing, configuration, and data inversion methods.	We now brought together the ERT description together in this section (Sect. 2.4). We also added a clear reference to the published dataset on Zenodo, which includes all analyses.	• Add ERT description • Move all part related to ERT in Sect 2.4. • Add reference to dataset

Reviewers' comments	Authors' response	Changes in manuscript
Line 313. How was the presence of buried ice blocks identified?	It was detected due to a very high resistivity. This sentence was removed as we do not further discuss this topic here	• Remove sentence
Line 396. How was B determined based on ERT results? Considering the quality of ERT data and spatial heterogeneity, the determination may not be straight forward. Please explain this more carefully.	We determine B by identifying a sharp transition in resistivity value (between 500 to 2000 Ωm for water-saturated sediment and 4000 to 7000 Ωm for bedrock). The depth is relatively flat in the center of the floodplain along a transect, between 10 and 15 meters.	• The methodology is now indicated in Sect 2.4. • The actual depth is moved to results (Sect. 4.3). • Fig. 7 was added in the results to show an example of the bedrock identification
Line 402-406. Please present this information in the section of field methods (see my comment on Line 312). In general, the manuscript suffers from a lack of organization, meaning that methods are not presented in where the reader expects them to be.		• This was moved to Sect. 2.4.
Line 416. This terminology (Smax) is misleading and inappropriate. What it represents is not the maximum amount of water that can be stored in the unit. It is the storage corresponding to the initial flow (Q0) at the beginning of recession analysis period. I suggest it to be changed to S0 instead.	Yes thank you for this comment. We changed Smax to S0 and took care to be specific in its definition	• Changed Smax to S0

Reviewers' comments	Authors' response	Changes in manuscript
Line 420 and Figure 2. I do not think that the classification approach solely based on slopes adequately captures the spatial extents and distribution of the landforms for the purpose of this study. For example, a quick examination of satellite images on Google Earth indicates that much of '22-42 deg (talus slopes)' on the north side of the instrumented area are likely bedrock slopes covered by a thin layer of soil and vegetation. They are clearly not talus slopes and hence, will have completely different hydrological storage functions. This applies to other landforms as well, putting the entire exercise of data analysis on a shaky foundation. I strongly recommend that the authors use an approach combining digital elevation models and satellite images to come up with more appropriate landform classification, and reanalyse the data set.	We added a new section 3.4 which describe the new methodology. We now combine a DEM-based slope classification with an orthoimage and perform a supervised classification. The results were adapted in Fig. 2. The new classification now also includes grass on bedrock. The classes' areal extents are relatively similar, except for talus slopes which decreased by half and was allocated to bedrock mostly. Flat moraines also increased by 30%. We changed the areal extents in the model and performed the analysis again, which does not show significant changes, except for the maximum storage volumes of flat glacial deposits. which increased by 50%. In particular, the interpretation remains similar as the rate of the recessions in each landform did not change, so that the winter storages are similar, with only a bit more storage in the flatter glacial deposits.	• Added a new Section 3.4. with the new classification method • Updated Fig. 2 • Updated the areal extents used for the model (Table 3) and adapted the model results accordingly (Fig. 8)
Line 437. 'Fitted by matching the snowline limit'. How was it done? Please explain the methodology. It may not be a straightforward task in a mountain environment with frequent cloud covers obscuring satellite images.	This is now better detailed. We used 3m resolution daily Planet.com images. Since they have a daily return coverage, it was always possible to obtain at least a weekly snow line estimation. We updated slightly our mass-balance model and added more information on the calibration. Here we want to stress again that our purpose was to only create a realistic dataset of snowmelt and rain input inside the catchment, but we don't need a fully calibrated spatially distributed model since our whole model purpose is to highlight general trends (to build a perceptual model) and not absolute estimates (we do not aim to simulate the measured discharge of a specific year). The current mass-balance is now however well calibrated and represents well the water inputs of year 2020.	• Added more details of the snow-mass balance model and the calibration procedure using Planet imagery and SWE observation

Reviewers' comments	Authors' response	Changes in manuscript
Line 438. If the elevation of the weather station is lower than the average elevation of the catchment, the data may substantially underestimate winter precipitation. How was this issue addressed? Please explain.	The weather station is at the foot of the glacier at 2450 m. We corrected for precipitation lapse rate based on calibration. When the SwissMetNet stations were used in winter a fixed correction factor was further defined and also calibrated.	• Added a clearer description of the mass-balance model and the calibrated model parameters.
Line 441. The model provides a first estimate of storage dynamic. What does this exactly mean? Please explain it more precisely and specifically.	See comment on line 447 below. In general, we moved the discussion on our model design to the Discussion part of the manuscript	• Move discussion of the model design in the Discussion section.
Line 443-444. Only a small fraction of river discharge was allowed to recharge the outwash plain. Was this observed? Or assumed? Please clarify. If it is an assumption, please present a clear justification.	This amount is actually based on an estimation based on stream gauging along the outwash plain, but also due to further modelling results of the outwash plain which will be soon submitted in another research article. Moreover this amount was found to correctly represent the spring groundwater increase in the outwash plain. Finally, for the purpose of our model a stricter definition of the recharge would not lead to significantly different results as the outwash plain remains filled during the whole summer so that higher infiltration would not lead to higher storage.	• Added a sentence to specify more clearly this choice.

Reviewers' comments	Authors' response	Changes in manuscript
Line 447. Simple model. I feel that the model may be too simple for the purpose of determining aquifer storages with sufficient rigor. Please improve the presentation of scientific rigor in various parts of Section 3.	We agree that our model uses a rather simple approach. Again, this is also on purpose in order to be easily reproducible in other catchments, without the need to set up a complex hydrological model. We agree that the method and result sections could be improved. In particular, we introduced some confusion using the term "proglacial" while we were sometimes refereeing to the whole glaciated catchment. While our approach is clearly simple, we believe that it is valid to represent the individual storage-release response of specific superficial landforms, and especially to compare their relative significance in terms of response time scales. In addition, the estimated volumes and timescales agree well with other more specific studies as presented in the discussion. Our goal here is to provide the reader with a simple approach, easily reproducible at other locations, to acquire a general understanding of the key processes of glaciated catchments, which can then be used to develop more complex models or approaches with a better representation of the groundwater processes. In order to provide a more robust model, we included a margin of uncertainty concerning the estimated storage volumes. This was done by defining a minimum and maximum value for the hydraulic conductivity, rather than only a mean value. We performed our model with each extremes and show now in Fig. 8 an uncertainty band.	• Adapted the methodology section (Sect 3.5) to explain more clearly the procedure • Added a paragraph (end of Sect 3.5) on the estimation of the partitioning of the different sources of water which we used in the perceptual model (Fig. 13) • Added an uncertainty band for the model results of Fig. 8. • Moved the discussion of the model adequacy to the Discussion section (Sect. 5.1)
Line 452-454. This needs to be described in the method section, where the water balance equation is introduced (Line 431). Please see my comment on Line 402.	Yes, various parts of the results were moved to the methodology, especially for the previous Sect. 4.1. We also moved the previous Sect 4.1 (catchment-scale recession) to the end of the results.	• Moved parts of results to methodology, remove redundant information
Line 454. Figure 4. Should this be Figure 3?	Yes thank you	• Changed reference to correct figure

Reviewers' comments	Authors' response	Changes in manuscript
Line 455-456. Which gauging station was this recorded at? GS3? Please include this information in the figure caption as well.	Yes	• Included GS3 in caption
Line 456-457. The increases of flow during the recession period (Figure 3) do not look like 'very small' noises. What causes the increase of flow? Please explain.	This was not clear. Here we were referring to noise in the raw discharge data of the Figure 10 (in the current version) and not the noise dQ/dt. Looking at new Figure 10, the discharge data decrease with step-like intervals due to the resolution of the measurement device. Some noise also appears, likely due to high sediment load and partial clogging of the sensor. Since the rate of decrease (dQ/dt) changes rapidly at those low values, a strong smoothing which only provides the seasonal recession trend was used on the discharge data, prior to calculate dQ/dt.	• Explain better the step-like decrease. • Move this to methods (Sect. 3.2)
Line 479. Please see my comment on Line 416.		• Smax was changed to S0
Line 506. This is an unusually large value for rain. Please examine the possibility of contamination by sampling devices or sample handling. Rain sample values are expected to be similar to snow sample values.	We can exclude contamination since the samples were cleaned with DI water. From the literature, we do not think this is a particularly high value (e.g. Zuecco et al., 2019). From the composition of major ions in rain, it seems that rain contained more Calcium and Sulphate than snow. We can only make the hypothesis that snow underwent some biological processes leading to a loss of ions or a different precipitation composition in winter.	• Added a sentence on this issue and a reference to Zuecco et al., 2019
Line 539. This statement contradicts with the caption of Figure 8, which states that the lateral gradients are directed towards the main river. Which is the correct observation?	The direction of the groundwater gradients can indeed be somewhat puzzling. We tried to better explain this in the new version. It seems that, although there is a small lateral gradient from the hillslope, the gradient is in the opposite direction from the river to the first well close to the river. The river stage seems to always be higher so that it creates a hydraulic gradient at the riverbed which can only lead to surface water infiltration to the aquifer.	• Specifiy the direction of the gradient near the stream and riverbed infiltration • Adapted the text on the aquifer gradients to be clearer and more concise

Reviewers' comments	Authors' response	Changes in manuscript
Figure 8. Please include the unit for river discharge and specify the vertical axis for discharge. Is the 'glacier outlet' discharge measured at GS1? Please clarify.	Yes	• Included an axis for discharge • Added GS1 in caption
Line 545. Water contribution from the hillslopes. This is a losing stream. How is it possible for it to be gaining groundwater from hillslopes? Please clarify.	Related to comment on Line 539. The aquifer seems to show two lateral gradients and it can only gain water from multiple locations if a perched tributary flows at the surface of the outwash plain or if the stream level is higher than the groundwater. The main gradient is downstream so that all contributions in the upper part of the outwash plain is drained downwards to the downstream part of the aquifer where groundwater upwelling occurs.	• Adapted the text on the aquifer gradients to be clearer and more concise
Line 582-583. Please show the actual ERT data to demonstrate the results. The ERT data will be useful for demonstrating bedrock delineation as well (please see my comment on Line 396).	Yes, we added one ERT profile at the location of the upstream groundwater wells to illustrate our results and added a short interpretation of the results. We also reference the access to the full dataset published on Zenodo.	• Added Fig. 7 with an ERT profile at the location of the analysis. • Added a paragraph on the depth of the bedrock
Line 585-587. Decagon 5TM device. This information should be presented in the method section.	Yes, thank you	• Moved to method (sect. 3.1)
Line 590. Please be mindful of the number of significant digits.	Yes, thank you	• Changed the number of digits to one
Line 617-624. Figure 9 shows the water storage per unit area for each landform, irrespective of the area coverage of landforms within the catchment. Outwash plain may have a large storage (mm), but it may contribute relatively little to total catchment storage. This need to be explained clearly in this paragraph.	There was a confusion here, likely also due to an incorrect use of the term "proglacial" while we were modelling the entire glaciated catchment. The results are actually scaled for the entire catchment (the volume of storage [m^3] is divided by the total catchment area [m^2]). In this way, the relative storage volumes in *mm* are readily comparable at the catchment-scale which was the purpose of our approach and the suggestion of the reviewer.	• We added more clearly that the volumes are scaled by the catchment area in the Figure caption and in the text.

Reviewers' comments	Authors' response	Changes in manuscript
Line 660. These landforms cover the entire catchment, not just the proglacial zone. Given that glacier outflow is sustained during winter months (Figure 8), the storage capacity of these landforms in the entire catchment during winter months needs to be evaluated. I see this as a fundamental issue in this study.	This was again not well explained. In our approach, we already include the areal extent of the landforms of the entire catchment (and not just the proglacial zone as wrongly stated). We are actually comparing the storage capacity of the entire catchment as the reviewer is suggesting.	• We try to correct this confusion in various parts of the results and discussion
Line 696-697. Can you quantify the total storage provided by these landforms in the entire catchment (see my comment on Line 660)? How does it compare to the total amount (mm) of winter flow measured at GS3? This information will provide an important 'reality check' for the perceptual model.	See comments above. It is already estimated at the catchment-scale. For instance the cumulated discharge at GS3 in winter is about 20 to 25 mm. This amount is only explained by including a "missing storage" of 40 mm which drains by half during the winter, with a slow recession, as discussed in the paper.	• We included a sentence to specify the cumulated winter discharge (20-25mm) • We discuss in more detail the recession time of the discharge at GS3 which is significantly larger than any landforms.
Line 698. This value (40 mm) is solely based on mathematical reservoir models, which in turn are based on several assumptions, which may or may not have the physical basis validated by field data. While this approach is useful, its limitation needs to be clearly acknowledged.	We completely agree about the limitation of this approach. We included a paragraph about those limitations	• We included a paragraph to highlight the limitation of the approach (Sect. 5.1.5)
Line 718-719. Please consider the areal extent of the landforms in the catchment-scale storage calculation (please see my comment on Line 660).	See comments above. This was already our purpose.	• Improve the clarity in the text
Line 732. Having seen the results from an objective set of eyes, I do not believe that they 'indicate clearly' that winter baseflow is governed by non-superficial reservoirs. Please re- evaluate the assumptions and calculation methods, and re-examine this statement.	We hope that with those clarifications, we have cleared any confusion and have now better convinced the reviewer about the validity of our method and discussion that the superficial landforms cannot explain the catchment-scale response.	

Reviewers' comments	Authors' response	Changes in manuscript
Figurer 11. This is a confusing diagram. Fluxes (snowmelt, rain, etc.) are mixed up with storage volumes. Please use a different scheme to represent the perceptual model.	We chose not to change this figure too heavily, but tried to improve the legend and improve the clarity of how it was built. For instance, we believe that this perceptual model has some strength as it not only relies on our subjective perception of our results, but is bound to our model results which show a simplified representation of reality. Indeed, the fluxes are based on results in Fig. 12 and the storage partitioning based on Fig. 8. We would like to keep both Fluxes and storage as they are the two components of the storage-release approach ($S=eQ^c$) and provide a full picture only together : A large storage will only lead to little discharge if the recession constant (e) is large (like the bedrock). On the opposite, a small storage with a fast drainage may provide large discharge but only on short time-scales. Finally, we included the partitioning of the sources of water (which we now explain at the end of Sect. 3.5) in order to give some insights into the future changes that may occur with a shift in the melt period.	• Changed the beige area in the figure to bedrock • Made the arrow clearer • Changed the legend to better explain the diagram • Improve the caption to better explain the sources of information used to build the diagram