Authors response - EGUSPHERE-2022-1503

Current and future role of meltwater-groundwater dynamics in a proglacial Alpine outwash plain

General comments

We thank both reviewers and the editor for their careful and constructive comments on our manuscript. We attempted to address all discussed issues in a new version of the manuscript. Overall, the structure of the paper remains similar, but we added precisions in various parts of the manuscript as proposed by the reviewers. We also added some additional sentences in the conclusion regarding the model choices and how our results are applicable to other sites. Finally, a few short paragraphs of minor interests were removed to limit the length of the manuscript.

Point-by-point response: Anonymous Referee #1

Revisions

Reviewers' comments	Authors' response	Changes in manuscript
In line 165, the authors utilized the unconfined aquifer formulation for groundwater modeling. Given the shallow depth of the aquifer body, it's possible that the vadose zone could significantly impact your results. Have you assessed whether neglecting the vadose zone is applicable to your domain? It would be valuable to explore this possibility further to ensure that the assumptions align with the specific conditions of your study area.	In this study, we indeed decided to neglect the vadose zone. Due to the very coarse texture of the outwash plain sediments, it is expected that the capillary fringe is thin and that the water content in the unsaturated zone above the water table decreases rapidly. In our case, the soil texture is composed of more than 90% sand and usually less than 1 to 2% clay. This leads to a soil texture dominated by large pores where the capillary effect is limited. This statement has been tested by building a very simple HYDRUS-1D model which solves the Richard's equation and allows accurate modelling of the interface between a saturated and unsaturated zone. It shows that the water content above the saturated zone drops rapidly to values close to 0.05, in the first 50 cm. Similarly, the unsaturated hydraulic conductivity drops sharply and also leads to a very dry soil surface, where actual evapotranspiration is strongly limited. Additionally, residual water content in the vadose zone should have a limited effect on diel groundwater fluctuations.	We added a similar justification in the discussion (limitations) and made the absence of vadose zone clearer in the methods.

Reviewers' comments	Authors' response	Changes in manuscript
Additionally, during the winter period in the upstream portion of the domain, most of layer 1 is unsaturated. It may be worthwhile for the authors to consider solving a Richard-based model for the first layer and using a simpler formulation for the other layers. Given the ample data available for the studied domain, have the authors considered using UFZ MODFLOW package or other models that allow for this type of modeling, such as ParFlow (Maxwell et al., 2005) or OpenGeoSys (Kolditz et al., 2012)?	We did not consider implementing any unsaturated flow package based on the assumption stated above. In addition, even though the processes in the superficial unsaturated layer may not be fully well represented, the focus of this study is on the aquifer-scale groundwater flow and exchanges with the stream. Due to the automatic calibration based on an extensive database of observations, we believe that the seasonal aquifer behavior is correctly modelled and the use of a more complex (unsaturated) model would not change the modelled behavior and would not provide additional information regarding the research questions. We believe that unsaturated processes would only be required for processes such as soil water - plant interactions such as root water uptake or evaportranspiration, which can be safely neglected in our case.	(same as previous point) We added a similar justification in the discussion (limitations) and made the absence of vadose zone clearer in the methods. We also mentioned other model types in the discussion.

Reviewers' comments	Authors' response	Changes in manuscript
Did the authors investigate whether water evaporation, which may be more significant during summer days when solar irradiation is stronger, was negligible? Additionally, it is not clear to me if the authors evaluated the rain runoff during raining events. It would be valuable to understand how rainfall was accounted for in the study and whether it was incorporated into the groundwater model.	As discussed above, evaporation from the top sediments is highly limited by the coarse nature of the sediments and the limited capillary effect. As such, we neglected this process. We also did not specify that we also neglected the direct recharge effect of rain falling on top of the aquifer. This simplification was made as we could never observe any groundwater response after a rain event in any groundwater wells. Precipitation seemed indeed to only impact groundwater levels for rain events larger than 10 to 20 mm/day (Fig. 6), but the rapid recession of the groundwater head seems to indicate that transmission of the rain water through vadose zone is fast and water retention in the unsaturated sediments is strongly limited after the day of the event. Moreover, in our model, we only accounted for lateral recharge during such rain events but model results show a similar groundwater response as observed, suggesting that the additional input of rain water on the surface did not influence much the groundwater levels (Fig. 6). We nevertheless attempted to add a simple module to simulate a delayed rain input to the aquifer but this resulted in an overestimation of the water heads for all rain events. The reason for this lack of response is likely due to the effect of the dry unsaturated zone, which likely retains part of the rainwater infiltration in the unsaturated zone or/and preferential flowpaths in the sediments which limit diffuse surface recharge and concentrate surface water infiltration to specific locations within the outwash plain which are not monitored or rapidly drain into the braided stream. Since the groundwater head did not respond to most rain inputs, we believe that neglecting the unsaturated zone, as well as the direct input of water from precipitation falling on the sediments allows for an adequate simplification of the model which aims at modelling the seasonal groundwater behavior of such a system and not specific processes in the vadose zone.	We added a similar discussion in the discussion (limitation) section of the new manuscript. We made it clearer in the methods that rainfall on the surface was not accounted for.

Detailed comments

Reviewers' comments	Authors' response	Changes in manuscript
Line 172: could the author report more detail on the coupling method between surface and subsurface model that the authors had employed among the possibilities offered by MODFLOW? This information could be possibly helpful in providing a deeper understanding of the results obtained.	We used the most state-of-the-art packages offered in MODFLOW to represent surface-groundwater interactions. The streamflow (SFR) package is the only package which allows to simulate the dynamic stream stages at every node based on a complex channel cross-section and on Manning's equation. Some issues in MODFLOW representations have previously been discussed in the literature (Brunner et al., 2010). One challenge is the inability to simulate negative pressures in the case of a perched stream. We believe this should be at least partially mitigated by the calibration procedure.	We provided more details about this module in the methods.
Could the author provide more detail on how the two objective functions described between lines 195 and 201 are used for calibration? In particular, the authors defined a single objective function that utilizes a weighted sum of the multi-objective function. How was the weight of the OBJ function selected? It would be helpful to provide a clear explanation of this process to ensure the reader understands how the calibration was performed.	Concerning the single objective function, we first weighted both objective functions (SWE and snow cover) equally, that is the total initial weighted sum of residuals for both objective function is equal. In this way, both the objective functions are optimized to a similar degree. Based on the initial calibration results, we then decided to increase the weight of the snow cover objective function to improve the representation of the snow cover fraction, which tended to decrease too quickly in the model during the late season. This iterative adjustment of the weighting resulted in a total weight of 1 and 2.4 for the SWE and snow cover objective functions. This final weighting allowed to better match the measured snow cover fraction over the whole season, while maintaining a root mean square error of about 100 mm for the estimation of the SWE.	We added this information in the methods.
is it possible to include section number 4 in Figure 2?	Yes	Added lateral recharge to Fig. 2

Reviewers' comments	Authors' response	Changes in manuscript
In line 381, you mention that exfiltration is also correlated with stream water infiltration, citing Figure 8c. However, it is not clear from this figure how these two variables are related. It seems that exfiltration has a similar hysteresis effect to upstream discharge, as you note. To gain a better understanding of the relationship between these two variables, it would be helpful to include a graph comparing upstream discharge and exfiltration, if possible.	Figure 8c already shows the relationship between stream infiltration and groundwater exfiltration. We decided to leave the figure as is to avoid increasing the manuscript length.	We left the manuscript as is.
In line 411, could the authors provide more explanation about the selection of the porosity value?	Yes, we forgot to specify this in the methodology and added this to the methodology. Porosity was estimated by measuring the saturated water content using a Decagon 5TM at five locations in the upper sediment layer. Additionally, the sediment texture was analyzed by laser granulometry, resulting in a large dominance of the sand fraction (>90%). Such coarse sediment texture usually has a low porosity of 0.25 to 0.3.	Added this precision in the methodology

Point-by-point response: Anonymous Referee #2

Major revisions

Reviewers' comments	Authors' response	Changes in manuscript
Some of the description of the numerical model structure, as well as the forcing data applied under the future scenario (which seem to be missing).	Indeed, we corrected this in the document (see also pdf comments below)	• Added more details to the methods
In addition, I am unsure whether the 3 day model reinitialization period is sufficient (perhaps it is, since the aquifer system seems to form a kind of bedrock-dammed "bucket" in which groundwater levels near the surface are maintained). Several additional (generally minor) comments are provided in the attached PDF.	The 3 day reinitialization period seems indeed rather short but was verified before running the full calibration. The initial groundwater head was initialized 1m below the ground, which is not far from the measured groundwater stage. Due to the very conductive nature of the sediments, the aquifer rapidly reaches an equilibrium with the stream and 3 days was found to be sufficient to obtain such equilibrium.	• We detailed this in the Sect 3.2.3
More generally, in agreement with Reviewer 1, I also think it would have been interesting to apply a "fully-integrated" hydrological model code such as ParFlow, ATS, or HGS to the problem. Some studies applying these models at catchment scale in mountain settings have emerged (ref1,ref2); multi-scale work using nested integrated models could perhaps be proposed as a future research direction for such research, since this would enable many strong a prior assumptions made regarding relevant processes, channel locations, recharge locations etc. to be relaxed, whilst retaining detail in the aquifer of interest.	We thank the reviewer for this broader comment. As discussed before, we believe our approach also has some benefits compared to a more complex fully integrated model, as it allows us to focus more on one structure, with more control over the specific processes occurring in this aquifer. We also agree that future work proposing a more integrated modeling of the whole catchment would be interesting but would require more detailed geological data, especially regarding the nature of the bedrock fractures and groundwater flow within such aquifer. We finally believe that using different methods with different levels of complexity, as well as detailed analysis of field data of various sources are all required to provide a fully sound understanding of alpine catchments, as each method is limited by the choices of model structure and assumptions.	We added a section in the discussion part of the revised document to highlight our choices and the potential use of fully-integrated models.

Supplementary comments in the pdf

In the following, we review comments directly inserted in the pdf. We highlight here only major comments, while minor suggestions such as wording were directly corrected in the new version.

Reviewers' comments	Authors' response	Changes in manuscript
Line 2: I'm not sure that I necessarily agree with this. such interactions may have previously been occurring at glacier beds. Also what, more precisely, do you mean by "significant"? Certainly they become more "visible".	Yes, we were here mostly referring to the appearances of some superficial landforms due to sediment release (such as the outwash plain) and the thawing of frozen soils.	• We added some precision regarding the processes.
Line 6: Please rephrase this sentence as the meaning is unclear at present. A "focal point" is also rather vague.	Indeed, this sentence was vague.	• We rephrased this sentence to make it more specific
Line 9 : This is a little confusing. Was it only the comparison of the model with the observations that revealed the strong interactions? I would rather have expected that, for example, the observations show this to a certain extent and the model allows the processes to be further elucidated, and/or various hydraulic parameter values assigned and/or predictions made (at unmeasured locations or for the future). Perhaps consider rephrasing this sentence to better capture what the respective insights from the observation and model are.	This is indeed confusing	• We simply removed the confusing reference to model and data, as it is already described in the previous sentence.
Line 12: Presumably only when there is still a glacier melt fed stream to provide the recharge?	Not necessarily. Since they are bedrock dammed, their hydraulic gradient decreases rapidly when recharge is limited so that the groundwater levels drop especially in their upper part, while the lower part (near the bedrock dam) remains saturated.	• We rephrased to be more specific.

Reviewers' comments	Authors' response	Changes in manuscript
Line 19 : It is unclear from just the abstract whether the statistics in these last few paragraphs are referring to a generic outwash plain, or the specific one studied. If the former, then surely there would be quite considerable variability between types of catchments, for instance the geometries of the bedrock, the size of the glacier, the characteristics of the alluvial aquifer, and also the permeability of the bedrock? It could be useful to clarify this for readers of the abstract, and if necessary expand on it later in the manuscript.	Indeed we are talking about this specific case study	• We rephrased to make this point clearer
Line 46 : It could be interesting to reflect on the proportion of deglaciating catchment in which outwash plains form. Presumably in some settings, e.g. where the downstream gradient is very steep or there are few bedrock overdeepenings / constrictons, there is not the opportunity for them to form? Any such statistics, estimates or comments could further contextualise the work.	Thank you for this comment. We provided some rough statistics of post-LIA fluvial systems (Carrivick et. al, 2018) to better contextualize this work. Nevertheless, an exact mapping of current and future outwash plains has not been proposed yet. This work may encourage more research to fill this knowledge gap.	• Added two sentences in the introduction
Consider adding the locations of the electrodes to Figure 1. Also, specify that the inversions were 2D (I assume?), not 3D?	Yes	• Added electrodes to Fig. 1. We specified that inversion were in 2D
Line 133: Did you delineate this interface on the profiles (and if so, was it spatially "coherent")? Did it define the lower boundary of your numerical model?	Yes, this is described in section 3.1.2. Profiles were coherent.	• No change
Line 134 : Perhaps explain a little further why so many were done. I assume this was to get good spatial coverage of the entire system, rather than for example to look at any changes (e.g. in moisture conditions) on a given transect via repeat surveys, since ERT may generally struggle to pick up such variations?	Indeed, some lines were repeated over 3 years to assess changes in buried ice, but only one was used for the bedrock dealination. In total 12 were used.	• Corrected the number of lines and added them on figure 1.

Reviewers' comments	Authors' response	Changes in manuscript
Line 136 : with what results / outcome? What did you do in case of any differences?	Mostly to check for data quality and check which method was most suited. Most profiles agree well and we used dipole-dipole results for the bedrock profiles	• We rephrased this
Line 166 : It is very unclear whether the the domain was of uniform vertical thickness or not. Figure 2b suggests not. This means that the lines drawn (L162) were not of equal depth. Was the third layer from the surface 4 m thick, and then the final layer of variable thickness spanning the distance to the top of the bedrock? At the moment, the text suggests that you have two 4 m layers, and hence a total model domain of constant thickness. Please clarify. Also, was there any geophysical support or physical reasoning for the choice of 4 (spatially continuous) layers, with interfaces at the specified depths?	Yes, we fixed this. The last layer is indeed variable. 4 layers were used as a trade-off between allowing substantial parameter variation with depth and limiting the number of calibration layers (and parameters) which increased computational costs.	• We rephrased this. We also pointed to figure 9 were the layers cross-section can be viewed.
Line 170: How reasonable is this assumption, given the previous statements about the dynamism of these environments? Taking a fully-integrated simulation approach using a code such as ATS or HGS would have at least allowed smaller channels to be also represented (assuming they are represented in the terrain model). Such an approach may have also refined the representation of surface-subsurface feedbacks more generally. Indeed, some issues in MODFLOW representations have previously been discussed in the literature (e.g. ref1), though I have not followed developments regarding the extent to which the latest MODFLOW versions mitigate such limitations.	This is indeed one limitation of MODFLOW. We attempted to address this issue in the discussion already. We further detailed this point in the limitation section. Concerning MOFLOW limits, the only improvement in modflow is the definition of irregular cross-sections which allows a better estimation of the wetted area of the stream, but otherwise the points from Brunner et al., 2010 are still valid. In our model, the grid size is small and dry cells are allowed so that errors in the water table is limited. The major challenge is the inability to simulate negative pressures in case of perched stream. We believe this should be at least partially mitigated by the calibration procedure.	• We added a precision concerning mudflow limitations in the method (Sect. 3.1.3) and further in the discussion.

Reviewers' comments	Authors' response	Changes in manuscript
Line 195 : But how does this transfer the precipitation from those steep slopes to the corresponding downhill slopes?	We neglected snow redistribution and snow was simply not accumulating on steep slope. This is definitely a simplification, but we assume that the redistribution of snow to other flatter parts is compensated in the model calibration by other parameters. For instance, we use a factor to correct winter precipitation based on the measured winter precipitation at the nearby weather station. This factor may be somewhat increased to account for snow redistribution on flat parts. In any case, SWE in the flatter part was better constrained due to the SWE observations in this part which are part of the calibration objective function.	• We left this as is to not increase the manuscript length to much.
Figure 2 : Why would you expect lateral recharge to be point based? Do these locations correspond to gullies? Would one not expect at least some recharge across the 2D lateral domain of the aquifer model? Also, why should there be no recharge from the slope on the south side?	In this study, we mostly included surface water flow from the hillslope resulting from snow melt and rain. Those inputs were defined as point recharge as observed in the field. No surface flow was observed in the south side of the hillslope, likely due to less solar input and less melt. A diffuse subsurface recharge from the hillslope was not included due to a lack of data regarding this process. We however assume that diffuse recharge is likely not a dominant process in summer due to the coarse and steep nature of the hillslope and crystalline bedrock as discussed in previous work. In winter lateral subsurface recharge may play a more important role when melt is assumed to be very limited. This process is however difficult to measure in the field. From our previous work in this catchment in general, we have highlighted a winter baseflow of about 0.3 mm / day, potentially due to bedrock exfiltration. If we assume such a diffuse recharge from the bedrock below the outwash plain sediments over its entire surface, this corresponds to a recharge of 0.5 L/s. This flux appears to be a hundred times lower than the winter stream infiltration estimated for winter.	• We added a precision concerning recharge in the methods (Sect 3.1.4). • We added a discussion regarding lateral recharge and potential infiltration in Sect 5.2.

Reviewers' comments	Authors' response	Changes in manuscript
Line 228: Could perhaps be mentioned more clearly that the pilot points remain in the same location for each layer. In theory this need not be the case, for example of one expects a different pattern of parameter variabilty per layer.	Although the pilot points are at the same location, they are allowed to vary independently from each layer leading to different patterns after extrapolation.	• We left the sentence as is
Line 245 : Does the model state reequilibrate so quickly as this following changes in the parameter values? I would have expected it to take (potentially much) longer, especially during periods of recession.	Yes, the model stated in the summer and equilibrated very fast (1 to 2 days)	• We added a sentence
Line 286 : Specifically, what forcings were applied to these "future" scenarios. What this basically a resampling / selection from the recent historical observations, or derived from downscaled climate change projects (e.g. delta change method or similar). Please consider adding further details here.	For the future scenario, the goal was only to assess the cascading effect of multiple outwash plain on discharge. We did not create scenario of future discharge, but rather used a synthetic example of discharge, composed of a high discharge period and a recession period, to assess how outwash plain react to such inflow rather than providing a catchment-scale prediction.	• We added a sentence to clarify this
Figure 3 : Was this buried ice reflected in the MODFLOW structure / zonation and hence hydraulic parameter values applied? I expect the interpolations from the pilot points / use of a few horizontal model layers in the grid may not be able to fully "capture" these features (similarly to potential preferential pathways, as stated). If this is the case, it could be worth mentioning briefly.	Indeed, most buried ice structures were small (<10 m^3) and are not captured by the pilot point extrapolation method. Since their size is small their impact on groundwater is only very local. Only the large buried ice blocks shown in Fig 3b were directly included in the model as a no flow zone as they cover a large part of this cross-section.	• We detailed this better in Section 4.1
Line 439 : You mean vertically constant within each layer (I think)? I would clarify this as it can be confusing for readers to follow.	Yes indeed !	• We rephrased the sentence

Reviewers' comments	Authors' response	Changes in manuscript
Line 479 : Developing a nested distributed modelling strategy could have helped here, whereby the detailed aquifer model is nested within, and recieves its boundary conditions from, a coarser but broader model. This could perhaps be suggested as a direction for future modelling studies at this local aquifer scale.	Indeed, and this is what we attempted to due for the northern hillslope part where we estimated discharge from side tributaries. However, for the southern side, water is mainly provided by a small hanging glaciers and we lacked data for calibration there. Although providing a rough estimation of discharge may have been possible, we doubt that is would have significantly impacted the results from our outwash plain model as already discussed	• We added a sentence in the conclusion as you suggested
Line 527 : Based on this indicative information I would again question the suitability of only a 3 day reinitiaisation period for each PEST iteration.é	While the transit time of water in the outwash plain is a few weeks long, the water table reacts much faster to pressure variation (as shown by the groundwater well fluctuation). This also means that the watertable equilibrate very rapidly with the river stage during the model initialisation.	• A sentence was added in the method section.
Line 597 : I would suggest to emphasise that this is active / dynamic GW storage that contributes to streamflow.	Yes we detailed better this statement	• We changed the sentence

Reviewers' comments	Authors' response	Changes in manuscript
	More generally, in this study, we used a rather unconventional approach where we did not model the entire catchment functioning, but rather focused on a specific hydro-geomorphological structure within it and attempted to include all major processes that influence the groundwater storage of this structure. As highlighted by the reviewers, the use of a more complex modelling framework such as HydroGeoSphere, could have provided a more integrated understanding of the processes. We however lack much information about the subsurface geological structure and subglacial processes in other parts of the catchment and believe that the development of a more complex model may be very challenging and may introduce other sources of uncertainties, even more difficult to interpret. With the use of this rather hybrid model (highly detailed 3D aquifer modelling coupled with a rather simple lateral recharge routine), we have more control and understanding of the key processes which occur in the outwash plain aquifer specifically. Since our goal was to focus on the outwash plain and not the whole catchment, we believe this approach is well suited to our research questions. This will be made much clearer in the revised version.	•

Point-by-point response: Editor comments

Here we review comments provided by the editor privately, which we deemed important to address.

Reviewers' comments	Authors' response	Changes in manuscript
The recharge of the aquifer (from the top soil) is neglected since "we could never observe any groundwater response after a rain event in any groundwater wells". This observation is somehow controversial. I agree with the authors that the lack of response is probably due to the unsaturated area, somehow delaying the groundwater (GW) recharge. Therefore GW recharge cannot be applied at the time when rain occurs. Nevertheless, it would be appropriate to incorporate groundwater recharge into the groundwater model, accounting for a lag time to allow water to pass through the unsaturated area	We attempted to add a delayed rain module to the model to account for the precipitation input. The rain input was redistributed following a Gamma distribution and we tested different delays. However, since most groundwater heads showed no response, any additional water input led to an overestimation of the observed heads. During large rain events, long delays led to an overestimation the days after the event, while a short delay led to an overestimation of the peak water head. Since sediments are coarse, it is likely that water transmission through the unsaturated zone is fast, but water may infiltrate in preferential flow paths so that recharge is not diffuse and is therefore not recorded by our wells. In any case, compared to total stream water infiltration during the summer ($1e^{6}$ m3), the amount of water input from direct precipitation is low (~$3e^{4}$ m3) which should not impact the seasonal groundwater behavior much.	We added a paragraph in the discussion to address this issue.
Calibration. As detailed in table 1, some parameters are set to constant (not calibrated) values. In particular the specific storage, Ss, is set at e-5 and is constant across all layers. Note that : (i) Ss is a dimensional quantity, I guess the units used here are 1/m (2) Ss should vary according to the geomaterial, i.e. it cannot remain constant across each layer.	Specific storage was not calibrated as this value is much smaller than porosity and the small variations in water pressure lead to negligible changes in water storage for such a small, unconfined aquifer. We tested this statement by varying Ss from $1e^{-3}$ to $1e^{-7}$ in the model, which resulted in very little difference in modelled heads and water exfiltration (less than 0.1%). For this reason, Ss is assumed constant as a spatially varying value would not lead to any changes in the results.	We did not make any change in the manuscript regarding this comment as this seems like a minor issue and we would like to avoid a longer manuscript.
Line 186. The authors should provide an explanation for the selection of the 4 points of GW recharge shown in Fig. 2b.	These points were used as they represent the main observed ephemeral surface streams which drain the entire north side of the hillslope above the aquifer.	This was added after line 186.

Reviewers' comments	Authors' response	Changes in manuscript
- K_gw and S_y were estimated at 25 pilot points for each of the 4 layers (i.e. a 200 parameters, + 8 pilot points for K_b, for a total of 208 parameters). Ground water measurements (used to calibrate K_gw and S_y) were obtained using 8 fully-screened piezometers reaching a depth of about 2 m below the ground surface. Therefore they can provide hydraulic head (or pressure) measurements only for the top layer. Therefore it is not clear how the GW measurements can be used to estimate K_gw and S_y for all layers. This aspect is discussed in section 5.1 (and in particular at line 445). However, it is not evident how the authors have include the diel groundwater variation in the estimate of "depth averaged values of K_gw". Furthermore, since only the first layer is unconfined, values of S_y in the remaining layers do not affect the GW flow system.	It is indeed correct that Sy cannot be estimated below the lowest point of the water table in winter. Note that the aquifer model has four layers but they all represent one unconfined aquifer, which is simply separated into four parts to allow parameters to vary with depth. Concerning Kgw, based on the work of Magnusson et al. (2014) or Montalto et al. (2007), diel groundwater fluctuations are due to a "tidal effect" or the propagation of a diffuse wave in the aquifer from the stream. Based on some simplification, when the aquifer thickness (B) is much larger than the fluctuation, the magnitude of the diffusion depends on the aquifer transmissivity (T). Since T = Kgw/B, the amplitude and delay of the diffuse wave is proportional to the mean Kgw value in the aquifer. Therefore, using the groundwater fluctuations as an objective function forces the model calibration to better represent the average Kgw in the whole aquifer, that is at a depth lower than the piezometers.	We edited section 3.2.3 (Objective function and model calibration) since this is an important point for the calibration.
Line 228. The authors should specify the meaning of "automatic kriging". Additionally, since each kriging technique requires a variogram (including its shape and related parameters), the authors should provide details about the variogram used and how its parameters were evaluated.	Indeed, we can specify this better. In general, we followed the directions provided in the PEST documentation.	We added a sentence in section 3.2.1.
The manuscript is focused on an application to a specific study area. The authors should clarify how their findings can be transferred to other areas, so that their work can be of general interest for HESS readers	Yes.	We provided an additional discussion in the conclusion to better contextualize the findings.