Point-by-point response to the reviews

In this document:

- in black: editor and referee comments are shown
- in blue: responses to the referee with our relevant changes to the updated manuscript

Editor decision

Reconsider after major revisions (further review by editor and referees)

Comments to the author:

Dear authors,
I think the paper has a lot of potential and I would encourage you to send in a revised version of the paper considering all proposed changes in the responses to the discussions and I will send out the paper again to the two reviews for another round of reviews. Please provide a detailed response to the reviewer comments, and a revised version including all changes marked so the reviewers can do a much more focused review.
Best regards
Markus Weiler

Author's response:

We are very grateful to the editor Markus Weiler for re-emphasizing the interest and potentials of the article. As suggested, we offer a revised version of the manuscript (author's response and author's track-changes file) in which all referee comments are considered.

Due to major revisions, many changes (deleting text for rewriting, moving parts, new figures, multiple corrections, etc.) have been made in the new version of the manuscript. Thus, the document "authors track-changes file" comparing the previous and the current version of the manuscript is in our opinion complicated to follow and not very readable. Instead, we recommend to read the updated version of the manuscript with the associated "author's response" document, including the most relevant and important changes in response to the referee comments.

Comments from 2 referees

Page 2 to 7	Anonymous Referee #1, 09 Oct 2022 https://doi.org/10.5194/hess-2022-175-RC1
Page 8 to 14	Ciaran Harman #2, 25 Oct 2022 https://doi.org/10.5194/hess-2022-175-RC2

General changes

- For this new version of manuscript, the initially single-layer numerical groundwater flow model is now discretized into 6 layers of equal thickness (about 5 m for a 30 m aquifer). All simulation results were updated with this new parameterization. The impact on the results is minimal, if not negligible.

Lines [137 to 139]

"The 3D model domain is discretized laterally using the regular mesh of the DEM, and vertically into 6 layers of equal thickness. Convergence tests have been performed to ensure the stability of the result independently of the numerical discretization."

- All results presented in the new sub-section "3.1 Detailed analysis of model results on a single site" currently focus on the Canut catchment (site 6 on the Figure 3).

Figure 4, Appendix B and Supplement 1 are updated for this catchment.

Author’s response for RC1

Anonymous Referee #1, 09 Oct 2022

General comment

In the submitted manuscript, Abhervé et al present a study that deals with the estimation of hydraulic conductivity at the catchments scale. Solving the groundwater flow equation for R/K (R: long term recharge, K: catchment-scale hydraulic conductivity), the authors try to find most realistic values of K for 24 catchments in north-western France with different geologies, for which they compare simulated stream network extent with independently derived stream network maps. As a measure of similarity, they use the difference of the averaged over- and under-estimated stream network lengths. The results show that estimated K values cluster by the geologies of the 24 catchments with increasing estimated Ks towards more permeable rock types (Limestone). Sensitivity analysis shows that the resolution of the available DEM for estimating K is much less important than the stream map product to calculate most suitable K estimate.

Overall, the topic of the study and the produced results are of great value for the hydrological community and beyond since K estimates are usually available on much smaller scales (points to contributing areas of wells during pumping tests). The presented approach would provide K estimates at a scale most useful to be transferred into prediction models.

Response:
We are thankful to the reviewer for highlighting the interest of the developed methodology and its value for the hydrological community. All comments and suggestions raised by the reviewer are considered in the new version of the manuscript.

However, I see two major weaknesses that need to be addressed until this work can be considered for publication:

- There is a lack of reference to proceeding studies and methods to estimate K both in the introduction and the (very short) discussion. Although the authors estimate K at the catchments scale, they should provide more information about existing approaches to estimate K (e.g. well cores, pumping tests, model calibration) and for which scale they are applicable. There should also be more information on previous work trying to up-scale this information for large-scale modelling. Hartmann and Moosdorf are mentioned but no information about their upscaling approach and the range of K values they obtained. Also, there is some recent work on earth-tides and their usability for K estimation on larger scales. There is also need for mentioning typical ranges of K for different lithologies found by different studies or provided by established textbooks (e.g., Freeze & Cherry).

Response:
We agree with this comment. We have provided a more exhaustive review of existing methods in the introduction, with some references and more details. as follow.

Lines [37 to 48]

"Quantifying groundwater fluxes remains a challenge, as the hydraulic properties of aquifers, i.e. hydraulic conductivity (K) and transmissivity (T), have classically been constrained through sparse borehole-scale characterization (Anderson et al., 2015; Carrera et al., 2005). They are classically estimated using hydraulic tests at centimeter scales for laboratory experiments up to decameter scales for well tests (Domenico & Schwartz, 1990; Freeze & Cherry, 1979; Renard, 2005). Other methods have been proposed at larger scales based on the analysis of streamflow dynamic (Brutsaert & Nieber, 1977; Mendoza et al., 2003; Troch et al., 2013; Vannier et al., 2014), earth tides (Hsieh et al., 1987; Rotzoll & El-Kadi, 2008) and borehole head dynamics (Jiménez-Martínez et al., 2013; Zlotnik & Zurbuchen, 2003), as well as from the calibration of large scale hydrological models (Chow et al., 2016; Eckhardt & Ulbrich, 2003; Etter et al., 2020). Multi-objective calibration has been proposed to reduce uncertainties, considering complementary data like temperature (Bravo et al., 2002), groundwater ages derived from environmental tracers (Kolbe et al., 2016) or continuous geochemical monitoring (Schilling et al., 2019). In addition, recent advances in machine learning technics show promising results to evaluate hydraulic properties at the regional scale (Cromwell et al., 2021; Marçais & de Dreuzy, 2017; Reichstein et al., 2019)."

Lines [49 to 59]

"To tackle the numerous challenges related to the upscaling of hydraulic properties from the local to the regional or global scales, several databases provides exhaustive compilations of measurements performed all around the world (Achtziger-Zupančič et al., 2017; Comunian & Renard, 2009; Kuang & Jiao, 2014; Ranjram et al., 2015). By compiling values obtained from calibrated groundwater models, Gleeson et al. (2014) proposed a global-scale hydraulic conductivity map GLHYMPS, with an update by Huscroft et al. (2018), where values have been interpolated based on a high-resolution global lithology map (GLiM) (Hartmann & Moosdorf, 2012). Besides inconsistencies and methodological biases already supported in Gleeson et al. (2014), the compiled permeabilities above the regional scale (>5 km) (Gleeson et al., 2014) are not suitable at the catchment scale. Therefore, estimating subsurface hydraulic properties that correctly represent observed catchment-scale processes remains a major challenge for the hydrological community (Blöschl et al., 2019). New opportunities has been identified through the increasing availabilities of surface observations (Beven et al., 2020; Gleeson et al., 2021), specifically with application for ungauged basins."

We have also completed the discussion to put our results in perspective with previous estimates of catchment-scale hydraulic properties. A new specific sub-section is added in the discussion.

Lines [343 to 375]

"4.2 Comparison of hydraulic conductivity estimates with local values"

- There is a lack comparison to K values obtained by different approaches and studies. The authors relate their results to the works of Stoll and Weiler (2010) and Lou et al. (2010), who used very similar methods. But I would expect more comparison and discussion to independently derived K values, ideally for some of the test sites but at least to typical ranges provided in textbooks (e.g. Freeze & Cherry) or the values provided from up-scaled map like the one of Hartmann and Moosdorf (note that there are more recent versions of the global permeability map available). Generally, the values found here seem to be quite similar compared to the differences of several orders of magnitude for different geologies mentioned in Freeze & Cherry or even in Stoll and Weiler (2010).

Response:
As mentioned earlier, we have added a specific section and figures to compare our results with the K values obtained by different approaches. Specifically, we compare our K estimates with the ones obtained by independent local approaches using hydraulic tests (Jiménez-Martínez et al., 2013; Le Borgne et al., 2006) and numerical groundwater flow model (Kolbe et al., 2016), compiled in regional data synthesis (BRGM, 2018; Laurent et al., 2017). In addition, we provide to the readers a comparison of our estimates with the ones compiled in the well-known GLHYMPS 2.0 database (Huscroft et al., 2018).

Line [360]

The Figure 8 comparing our K estimates with local values in added in the sub-section "4.2 Comparison of hydraulic conductivity estimates with local values".

Line [458]

The Figure C1 in the Appendix C displays the comparison with GLHYMPS2.0.

For those reasons, and for the more specific comments in the following, I recommend major revisions.

Specific comments

- The introduction provides the motivation of the study and moves quickly to methodological aspects (LL 46 and following). Please move methodological parts to the methods section and provide a more detailed review of the state of the art of K estimation identifying the research gap addressed by this study.

Response:
The state of art of K estimation is updated as previously shown. In addition, the research gap addressed in this study is better highlighted in the new version of the introduction.

- Equation (1) describes anisotropic conditions (different Ks in the directions of x, y and z), while K estimates of this study assume isotropic conditions (no specification of K direction). Please simplify Eq (1) or clarify why the more complex version of the equation is shown here. Also, shouldn't W have the unit [L T-1] and not [T-1] as indicated in L 114?

Response:
We propose not to display the classic groundwater flow equation in 3D (steady state with isotropic and uniform K) because this information is finally not very useful and relevant.

- I am not sure if the performance metric J, as specified in Eq (2) will give you the best estimation of K of a given catchment. Since real geological systems are always heterogeneous and anisotropic, a best estimate of a catchment's K might give you a small over-estimation and a larger under-estimation of stream lengths, while J would find its optimum when both lengths are the same. Why did you not choose a metric that minimizes both over- and underestimation?

Response: A clarification is made regarding the performance criterion.

Lines [188 to 189]

"$\overline{D_{so}}$ and $\overline{D_{os}}$ intersect when the calibration criterion J is met. This criterion based on both $\overline{D_{so}}$ and $\overline{D_{os}}$ achieves the best equilibrium between over- and under-estimations."

The very important point about heterogeneity and anisotropy is addressed in the new last sub-section of the discussion "4.3 Sensitivity to input/model parameters and related improvements for broader applicability".

Lines [386 to 391]

"For site 8, differences come principally from the model and, more specifically, from the assumption of a uniform hydraulic conductivity. For this site with lateral lithologic heterogeneity, we found that the model underestimates the extent and density of the stream network in the part with dominant plutonic rocks, and overestimates them in the schists. On the site 8, the IDPR (Mardhel et al., 2021) indicates that the granitic area is less permeable than the schist area, and generally displays the limestone sites 21 and 22 primarily dominated by infiltration, consistent with our results."

Lines [412 to 416]

"At the current stage of the method, catchment-scale lithological heterogeneities can be considered by applying the methodology independently on sub-areas characterized by a homogeneous lithology. For example, on the studied site 8, application of the methodology on granite-dominated sub-catchments should result in lower K estimates than on the schist areas. Localized heterogeneities including weathering, fractures, faults, and other discontinuities cannot be identified. They should be explicitly introduced in the model and characterized by other methods."

- Subsection 3.1 provide new methods Eqs (3-5), which should be moved to the methods section.

Response:
These equations and their description are moved to the appropriate new method sub-section:

Lines [185 to 195]
In the sub-section "2.4 Calibration criteria between observed and simulated spatial patterns"

- The Discussion section is much too short and it should be separated from the conclusions. Please discuss here your assumptions and resulting uncertainty, compare to more other studies (not just Stoll & Weiler and Lou et al.), and explain under which conditions and how the approach can be applied at other catchments and the limits of transferability.

Response:
We agree with this comment concerning the discussion.

The discussion is now proposed in 3 main sub-sections listed below.
1) methodological aspects are discussed based on other studies
2) K-estimates are compared to local values, and finally
3) the sensitivities are raised mentioning the potential/limitations of transferability

Thus, the discussion has been largely completed and is now separate from the conclusions.

Lines [318 to 341]
"4.1 A new calibration method for the assessment of effective catchment-scale hydraulic properties"

Lines [343 to 376]
"4.2 Comparison of estimated hydraulic conductivities with previously published values"

Lines [378 to 417]
"4.3 Sensitivity to input/model parameters and related improvements for broader applicability"

Figure 7 (originally Figure 6 in the previous version of the manuscript) has been moved to this subsection of the discussion.

Author’s response for RC2

Ciaran Harman #2, 25 Oct 2022

The authors present a methodology for estimating a catchment-scale effective hydraulic conductivity by matching the surface expression of a 3D groundwater model and an estimate of the stream network.

This is a valuable contribution, and very interesting. I expect there will be more work in this area in the coming years. I have one major issue that I believe the authors should address, and only a few minor concerns. My recommendation of major revisions reflects only the one major issue I mention below. Otherwise only minor revisions are needed.

Response:

We are thankful to the reviewer for highlighting the interest of our work and the positive assessment of the manuscript. The comments and suggestions raised by the reviewer were useful to improve the manuscript. All recommendations have been considered in the new version of the manuscript.

Major issue

- I strongly recommend that the paper be changed to characterize the approach as providing estimates of the transmissivity, not the conductivity. I realize the authors may not welcome this suggestion and will likely be tempted to argue that the difference is trivial and not worth the effort to modify so many figures and text. I would urge them to consider the recommendation seriously though.

  · Firstly, the transmissivity is the physical property that most controls the surface drainage expression, not the conductivity. The results of Litwin et al 2022 (see in particular figure 4, and note that gamma is a dimensionless transmissivity, and Hi is better understood as related to K/R) show that under geomorphic equilibrium the drainage density is most closely related to the transmissivity, not merely the conductivity. The drainage network appears where the groundwater table needed to transmit water downslope just reaches the surface, so it is the depth-integrated conductivity that matters, i.e. the transmissivity. Because of this, the drainage network contains information about the transmissivity, not about the underlying conductivity.

  · At present, the thickness is set to 30m in the model, which seems arbitrary and unjustified given the likely variability in permeable thickness across the region. One might reasonably ask whether changing this (to, say, 20m) would affect the estimates of K? It surely would. In fact, it would almost certainly result in an increase in the estimates of K by just enough so that the product of K and 20m would be the same as in the case where the depth was 30m (i.e. K would be about 50% larger for the 20 m case than it was for the 30 m case). In other words, I would guess that there is a roughly linear sensitivity of K to the choice of thickness.

· Such sensitivity will be an impediment to efforts to understand the results and make comparisons between studies. Conductivities from different studies will not be comparable if they are based on models with different assumed thicknesses.

· However, if the authors reported that their model estimates transmissivity, they would likely find that those estimates are not so sensitive to the choice of thickness. Varying the thickness will likely not change the estimated transmissivity nearly as much. This will make the results more robust, and easier to compare with other future studies.

· The approach presented here is important and will probably be taken up by others in the future. It would be in the long-term best interests of the discipline that this important issue be clarified early on. I urge them to consider it.

Response:
We fully agree with the reviewer and seriously consider these welcome recommendations. In the new version of the paper, the approach is now characterized as providing estimates of the transmissivity. As stated in the title and reported in the text, we currently estimate the hydraulic properties, and not only the hydraulic conductivity.

Indeed, we highlight from the introduction that the spatial distribution of groundwater seepage is controlled by the transmissivity (T) as shown by previous studies (Bresciani et al., 2014; Litwin et al., 2022; Luijendijk, 2021).

Lines [66 to 70]

> "Under steady state conditions, the distribution of groundwater seepage is then controlled by the characteristic hillslope geometry, the recharge rate (R), and the aquifer transmissivity (T), i.e. the product of the hydraulic conductivity (K) and the saturated aquifer thickness (d_{sat}) (Bresciani et al., 2014; Gleeson & Manning, 2008; Haitjema & Mitchell-Bruker, 2005; Litwin et al., 2022; Luijendijk, 2021)."

In "2 Materials and Methods" section, we explain how the transmissivity is obtained and a specific sub-section is added to clarify this point. In addition, Figures 1 and 2 are updated with transmissivity.

Lines [110 to 113]

> "From the optimized K/R, the optimal hydraulic conductivity K_{optim} is deduced by considering the recharge R. The optimal transmissivity T_{optim} is obtained considering the average thickness of the saturated aquifer d_{sat} computed by the model (section 2.2.5)."

Lines [196 to 204]

"2.5 Estimating the optimal hydraulic conductivities

The model parameter K/R ratio is calibrated by minimizing the objective function defined by Eq. (Eq. (1)), for a given aquifer thickness (d). Optimization is performed by a dichotomy approach (Burden & Faires, 1985). The convergence criterion is reached when K/R varies by less than 1 %. In order to ensure that K estimates are representative of catchment-scale processes driving the spatial distribution of the stream network, independently of the aquifer thickness set in the model, we computed the equivalent normalized transmissivity, T/R, by multiplying K/R by the average saturated aquifer thickness (d_{sat}) computed by the model at the catchment-scale (Figure 2b). In our modeling approach, K and d are input parameters of the model, while T is an output including the computed d_{sat}. Finally, optimal transmissivity T_{optim} and hydraulic conductivity K_{optim} are evaluated assuming the applied average groundwater recharge rate, R, and under known aquifer thickness."

As suggested, the approach now provides estimates of the transmissivity.

Figure 4

Added transmissivity value

Figure 5 and 6

The initial figures displaying the K-values now shows the values in transmissivity (T)

Table 1

Added transmissivity and average aquifer thickness values (d_{sat})

We extensively analyzed the effect of the aquifer thickness. The results of the sensitivity analysis are added to the Figure 5, with the corresponding paragraph below.

Lines [256 to 264]

"We evaluate the impact of the maximum aquifer thickness on T_{optim} by running the calibration procedure considering five different values of d: 5, 10, 50, 100 and 300 m. We found that the simulated stream network matches the observed one for all thicknesses (d) (Figure 5a1, A to F). However, we found differences in the estimated T_{optim} (Figure 5a2). For cases C, D, E and F, where the maximum aquifer thicknesses are greater than 30 m, the optimal transmissivity T_{optim} remains constant at around 4.0 x 10^{-4} m^2 s^{-1}. For cases A and B with smaller thicknesses (<30m), T_{optim} reach much larger values of 4.1 x 10^{-3} and 1.8 x 10^{-3} m^2 s^{-1} respectively. Such divergences come from the breakdown of the Dupuit-Forchheimer assumption . Small thicknesses bring the flow lines closer to the surface and widen the seepage areas (Bresciani et al., 2014), effects that must be offset by substantially higher hydraulic conductivities and transmissivities to lower the water table."

Minor issues

- It isn't clear to me why the recommendation of using lower resolution DEMs is justified. I understand that registration errors are enhanced with a high resolution DEM, but that doesn't seem like a problem that arises from the DEM itself. Wouldn't it be better to create a buffer around the stream network to account for uncertainty in its location?

Furthermore, it is not clear why r_optim should be less than or equal to 2 "considering that the mismatch cannot exceed the resolution of two pixels" (Line 210). I don't follow the reasoning, and the restriction is clearly violated in the results. What is the purpose of normalizing by pixel size? Doing so will always result in a larger 'error' for high resolution DEMs. The larger values of r_optim for the 5m and 25m don't reveal a clear deficiency to me.

Response:
Thank you for raising these very important points.

We agree that the deficiencies noted with the r_{optim} criterion for the higher DEM resolutions of 5 and 25 m are not caused from the DEMs themselves. This is corrected and clarified.

Figure 5

Results for cases with DEMs resolutions of 5 and 25 m are displayed, with the corresponding paragraph updated as below.

Lines [271 to 277]

"However, for the 5 and 25 m resolutions tested (cases A and B), the distances $\overline{D_{so}}$ and $\overline{D_{os}}$ are highly sensitive to the mismatch between an increasingly accurate DEM and a coarsely defined stream network. The main factor driving $\overline{D_{so}}$ and $\overline{D_{os}}$ is no longer the hydraulic conductivity but the mismatch between the DEM and the observed stream network with r_{optim} values becoming larger (respectively 46.5 and 7.7 for the 5 and 25 m resolutions tested). These results emphasize that DEMs with too fine resolutions, here 5 and 25 m, cannot be used with the observed stream network selected in this study, at least at the current stage of the methodological development."

Lines [404 to 405]

"Third, the resolution of the DEM and reference stream network must be close. Nevertheless, the observed stream network layer could be adjusted to better match the DEM resolution."

Concerning the normalization from D_{optim} to r_{optim} based on the pixel size.

Lines [191 to 193]

"The smaller the value of D_{optim}, the better is the match of the simulated seepage pattern and the observed stream network. D_{optim} will thus be used as an indicator of the calibration performance. In order to compare cases with different DEM resolution DEM_{res} [m], D_{optim} is normalized by the DEM resolution"

- The discussion of the errors from line 246 on is important and ought to be expanded. Consider:

  - The assertion that the differences "come essentially from the data" is confusing. It also seems inaccurate, since some of the errors are due to deficiencies in the model (where the assumption of a uniform K strays too far from reality, such as Site 8) and some are due to deficiencies in the data (where the mapped stream network does not adequately capture a more complex reality, such as site 18), and in some cases it is not immediately clear which is in error (Site 23 -- is the true stream in fact offset from the lowest point in the topography, or is there a registration error in the alignment of the stream location data with the DEM data?).

  - In the case of site 21 and 22 does 'non-reported subsurface flow' refer to a karst conduit (i.e. a channel, but underground) that is known to exist (i.e. data exists showing that it is there)? Or could the down-valley flow be through porous media with a higher K than other areas? One might argue that these are quite different sorts of errors. If the former, it suggests that improved accuracy would come from including conduit flow in the observed stream networks. If the latter, it suggests improved accuracy would come from allowing for spatial variability in the K-field of the model. It may not be possible to distinguish between these in practice.

  - -It might be worth discussing a geomorphic justification for the approach. For example, the ideal stream network data is mapped extent of flowing streams, not merely the existence of a blue line on a topographic map, which are often based on purely geomorphic criteria (e.g. minimum upstream area). Further errors would occur where the surface drainage is not in geomorphic equilibrium (e.g. where relic channels remain from historical periods of erosion following deforestation, but they do not carry flow today). The authors should caution that incorrect results would arise from using stream network data that does not actually represent the extent of the flowing stream.

Response:
These very technical and precise comments are clearly in line with our reflections on the subject.

We recognize that the formulation "the differences come essentially from the data themselves rather than from the model" is confusing. We clarify all these specific points in a dedicated sub-section updated in the discussion, as below.

From line [378]
"4.3 Sensitivity to input/model parameters and related improvements for broader applicability"

Lines [379 to 392]
Updated description of Figure 7 illustrating the major errors at our study sites

Lines [399 to 405]
A new paragraph is added to clarify deficiencies from input parameter data (DEM and observed reference stream network) or model assumptions, and associated sources of error.

Lines [406 to 417]

Potential improvements and recommendations on the current model assumptions are discussed. The consideration of heterogeneity is also discussed.

- Can the authors provide better justification for the performance criterion? Why this and not something else?

Response:

Following this comment and the ones from RC1, we have added more details about this point.

Lines [188 to 189]

"$\overline{D_{so}}$ and $\overline{D_{os}}$ intersect when the calibration criterion J is met. This criterion based on both $\overline{D_{so}}$ and $\overline{D_{os}}$ achieves the best equilibrium between over- and under-estimations."

Lines [336 to 341]

"We propose more integrative indicators based on the distance between the observed and modeled stream networks computed along the steepest slope between them, as does the IDPR (Network Development and Persistence Index) to identify zones predominantly favorable to infiltration or runoff (Mardhel et al., 2021). The advantages of this method are to account for the topographical structure within the definition of the distances and to constrain the comparison on the best compromise between the over- and under-saturation, mainly driven by $\overline{D_{so}}$ and $\overline{D_{os}}$ respectively."

- Does the groundwater model include overland flow and reinfiltration? I.e. losing and gaining reaches?

Lines [133 to 134]

"Overland flows and surface water reinfiltration are not integrated as remaining marginal in the conditions of temperate climate and low topographical gradients of the studied sites."

- It is confusing that the description of the recharge rate estimation is included in section 2.1.5 and not in section 2.1.2 (where the estimated recharge rate is presumably used, unless I misunderstand)

Response:

We agree. The description of the recharge is moved in the sub-section: "2.2 Groundwater flow model parameterization".

- A uniform recharge rate is used -- this limitation of the method should be acknowledged. We know that riparian areas receive more recharge than uplands, and that evapotranspiration can be drawn from the groundwater.

Response:
Lines [408 to 409]

> "If the information is available, the method could be tested with heterogeneous recharge applied at the catchment scale."

- Line 203-204: I think D_os and D_so are switched here

Response:
The error is corrected.

Lines [184 to 187]

- Figure 5: please expand the figure so that the horizontal error bars are not cut off.

Response:
The initial Figure 5, currently Figure 6, is expanded.

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