Dear Reviewers, dear Editor,

thank you very much for your constructive comments!

Concerning the first review, we accept that hydrology has become much wider during the previous decades, which goes along with a decrease in average mathematical level. We accept that the reviewer was annoyed by the amount of mathematics that probably goes beyond the background of the average readership of HESS. However, we have the impression that the reviewer strongly underestimates the gap between the theory presented here and what is typically published in mathematical journals. So we do not take the suggestion to move to a mathematical journal seriously. The same holds for the repeated comments about the chaotic presentation of the theory.

The points addressed in the two reports are discussed below, where changes to the manuscript are highlighted in bold letters. Line numbers refer to the version with highlighted changes.

Best regards,

Jannick Strüven and Stefan Hergarten

Reviewer 1

I believe that the theory that is being further developed in this paper is very interesting. The reason I am not quite sure is that the way it is presented makes it very difficult to follow. The write-up of the mathematical development is confusing and sometimes sloppy (see the detailed comments). Some parts are excessively short and read as if the text is is aimed at mathematicians, not hydrologists. The text has several 'clarifying' sentences within sentences (some of them indicated in the detailed comments) that sow more confusion and reduce the readability of the text. The authors really need to step up their game here because a potentially valuable contribution to the literature is nullified by imprecise writing.

One thing that would help is to better introduce the underlying mathematical techniques because a considerable background is required from the reader, and this reviewer is one among probably many who does not have a working knowledge of all techniques used. Adequate referencing to suitable textbooks would also be very helpful.

We tried to provide some more background at several places in Sect. 2, although the starting level will still exceed the mathematical background of many readers.

This imprecise writing also shows in a lax treatment of the underlying assumptions regarding aquifer geometry, and it the treatment of hydrological variables, in particular the hydraulic head. This leads to an error in Eq. (12) that could have easily be avoided by a proper definition of the reference level for the hydraulic head, but for some reason this was omitted. (See the bottom of this comment for details).

We indeed forgot to state explictly that h refers to a steady state (long-term constant recharge) as a reference level. Since Eq. (12) is a sum of decreasing exponential functions, it is immediately clear that $h \to 0$ for $t \to \infty$. So it would have been simpler just to mention that the reference level was not specified instead of constructing errors in the equations. **The reference level should be clear now from the new Sect. 2.1 about the linearized treatment.**

In line 237, a flux per unit width is equated to an area, which is dimensionally incorrect. I give the correct expression in the detailed comments. This error affects Eqs. (39) and (40). The authors need to verify the effect of the required correction on the analysis based on these equations.

Since the second reviewer also struggled with the nondimensional treatment, **we devoted a new Sect. 2.6 to the nondimensionalization.** However, there is no correction to the analysis since the explanation of the nondimensional properties was just too short.

Overall, I have difficult time piecing the theory together because of its chaotic presentation, which is a pity because I find the approach very interesting. But without a step-by-step development of the theory, the train of thought is not clear. Consulting the references helped a little but not too much. The Results section helps clarify some things in the Theory section in retrospect, but that is undesirable. The fact that some of the equations are demonstrably flawed makes everything more difficult: if I have a hard time deriving an equation, I cannot be sure that is because I could not follow your reasoning, or because the equation is erroneous.

The transmissivity is assumed proportional to the storativity raised to the power n. Although this relationship is crucial for the results, you offer hardly any physical support for it, not do you offer alternative relationships. I am aware this relationship was developed in an earlier paper, but there too I did not find it easy to understand its physical basis. As far as I could find there is no critical evaluation of the validity of this relationship published. This weakens the paper. Ideally it should be examined more fully, and if that is not possible to a satisfactory degree, at least acknowledged.

At least in the form with conductivity and porosity, similar relations have been used for a long time. As long as the porosity does not approach unity, there is no reason to assume fundamentally different relations. The nontrivial steps are the transfer to transmissivity and storativity and – more critical – the relation to the catchment size derived from minimum energy dissipation. **Section 2.7 now provides a summary of the respective part of the 2014 paper** and hopefully makes these aspects clearer.

In the abstract, the Introduction (through the rationale for writing this paper) and the Conclusions, it should be explained better what the contribution of the paper is to fundamental groundwater hydrology, for solving real-world problems, or both. If part of the contribution is can only be achieved in the future, after more work has been done, that too is worth mentioning.
This comment in no way implies that such a contribution is lacking, because I think there clearly is, but the reader has to work hard to figure that out by her/himself.

We added some text at the end of the introduction (lines 102–114) to explain more clearly why we started this study and some more discussion at the end of Sect. 3.2 (lines 516–524).

The Results and Discussion section (incorrectly labelled 'Results') is easier to digest. It is not clear from the preceding text why the authors chose to focus on the aspects on which they dwell in this part of the paper. The focus seems to be driven mainly by the availability of low-hanging fruit and mathematical curiosity (both of which are valid arguments). But I would like to see a wider view, with the focus on aspects that steer future work and applications to real aquifers. My mathematical/physical curiosity was directed more at alternatives to the chosen relationship between transmissivity and storativity, applications to non-karstic aquifers with more elongated geometries and a principal flow in one direction, and the effect of a drainage network of streams and rivers. None of these are discussed.

Of course, the low-hanging fruits played an important role. Here the idea to find a simple model for an aquifer with a power-law unit hydrograph with an exponent different from the "usual" -0.5 was the staring point. After developing the numerics, we recognized that this is not the case for the considered dendritic patterns. Then the way was to find out whether we can learn anything from the behavior of such aquifers. Sloping aquifers with a principal flow direction were indeed in focus for a long time, but turned out to be extremely challenging. And finally we have to accept that preferential flow patterns with lens-like structures are fundamentally different. We would probably end up at a point with gradual progress towards existing work (e.g., Simpson 2018, doi 10.1111/gwat.12587), but without fundamentally new results.

In summary, I think the paper in its core makes a meaningful and innovative contribution that is well worth publishing in HESS/EGUsphere, but it needs to be thoroughly rewritten to make this contribution accessible to the readership. This explains my ratings of its significance, scientific quality, and presentation quality.

Detailed comments (more in the annotated manuscript).

Lines 119–125: Just to see if I understand: each node can have multiple donors but only one target. This will funnel the flow into preferential flow paths as the water moves downstream through the aquifer.

Right!

If node will receive no water from neighbor j if its hydraulic head is higher than at least one of the other neighbors of neighbor j. If this hold true for all neighbors, the node in question is not a target of any node. If, furthermore, its hydraulic head is $<=$ than any of its neighbors, the node will be not be a donor either: it is a passive node.
How is the hydraulic head determined in a passive node in subsequent time steps? Does it maintain the hydraulic head it had when they were cut off the flow network and became passive?
I have some difficulty imagining how clusters of passive nodes emerge, but can easily understand how large clusters of passive nodes, once they have formed, could be relatively persistent.

Oh – wrong track, probably because we did not point out clearly enough that the topology (so assigning a flow target to each node) is static and that there is no limitation in the fluxes concerning the sign. Theoretically, water could even be pushed back from the flow target it the head value at the flow target is high. This occurs, however, rarely and does not cause any problem. So there cannot be passive nodes. **We added two short explanations (lines 173–175 and 180–183).**

Lines 230–236: According to lines 230–236, T is proportional to S^n, but there is very limited physical justification for this relationship, even though it turns out to be very important here. The paper from which you adopted this relationship derives it theoretically, but there does not seem to be a physical explanation or an experimental test of the validity of this relationship. It relies on the minimization of energy dissipation, but this process is assumed to have run to completion in the modelled aquifer. How long does this take in a real-world aquifer?

The power-law relation itself – at least the version with porosity and conductivity – is not related to minimum energy dissipation. The Cozeny-Karman relation was the first to predict such a power-law relation. **We added some paragraphs on this topic (lines 367–390).** However, the question whether real aquifers approach a state of minimum energy dissipation and how long this would take is a different story, and we are far off from being able to answer this question at the moment.

The hydraulic head h(X,t) (L) is defined with respect to a fixed reference, often the mean sea level for the country in which the aquifer is located or a reference point on the soil surface. It hydraulic head can therefore be expressed as:

$$h(X,t) = h_r + h'(X,t) \quad (1)$$

where h_r is an arbitrary reference height (L) and $h'(X,t)$ is the hydraulic head (L) expressed relative to this reference. Inserting Eq. (1) in Eq. (12) of the manuscript gives:

$$h_r + h'(X,t) = (h_r + h'(X,0)) * \exp(-\alpha t) \quad (2)$$

Solving for $h'(X,t)$ results in:

$$h'(X,t) = h_r\left(\exp(-\alpha t) - 1\right) + h'(X,0)\exp(-\alpha t) \quad (3)$$

Substituting this result in Eq. (1) gives:

$$h(X,t) = h_r \exp(-\alpha t) + h'(X,0)\exp(-\alpha t) \quad (4)$$

I do not understand why the decay with time of the hydraulic head is a function of an arbitrarily chosen reference height. This clearly should not be the case, hence Eq. (12) can only be correct if $h_r = 0$, but this is not required in the paper. It probably implies that the reference level is at the flat aquifer bottom.

Of course, unit hydrographs typically describe the deviation from a reference state and are not absolute. Probably it would have saved work just to mention that the zero reference level was not mentioned explicitly instead of repeatedly deriving errors from this missing information. **We added a section about the linearized treatment of deviations from a steady state (Sect. 2.1), which hopefully clarify why the reference level must be $h = 0$. In this context, we also added an explanation why the boundary condition must be $h = 0$ (lines 143–146).**

Comments from the annotated manuscript (only those that are not totally clear)

Lines 11–12: Difficult to understand.

Maybe, but how to write it better?

The earliest one I am aware of is: van der Molen, W. H.: Physics versus mathematics in groundwater flow – a physical explanation of the minimum theorem in finite element calculations. J. Hydrol. 109, 387-388, 1989, doi: 10.1016/0022-1694(89)90026-7.

Indeed a nice and short paper. However, it is just explaining Hamilton's principle in the context of groundwater flow. In this case even only that steady states are typically characterized by minimum energy or minimum energy dissipation. So it does not fit very well into the line of papers about optimality since it only addresses the hydraulic head distribution for a given conductivity pattern.

Line 134: Is a two-dimensional space adequate for an aquifer? It creates unphysical heterogeneities that occur over its entire depth, does it not?

We did not get the point completely. In general, two-dimensional space is adequate for horizontal flow, where the transmissivity is the vertical integral over the conductivity. Or is the comment about the D4 scheme? Typical discretizations on regular grids also involve 4 neighbors, and the anisotropy is not a big problem. Of course, we have to take care that the patterns of S and T also have to be computed with the D4 scheme in order not to interrupt preferential flow paths.

Line 144: This is a bit too fast. How do you arrive at this? Is this one of several alternatives for diffusion problems, or are exponentially decaying functions unique in this respect?

Exponential decay is indeed unique in the context of diffusion. For wave equations (basically the same equation, but with a second-order time derivative at the left-hand side), these are sin/cos oscillations. Anyway, the way to the exponential decay should hopefully be clearer now **(lines 199–213).**

Line 149: I am sure you are correct, but I would appreciate a reference to the mathematical foundations of this.

The context of the eigenfunctions should hopefully also be clearer now **(lines 213–218)**. References to mathematics textbooks would not be very useful here since readers would have to dig into an extensive framework. Anyway, readers who want to get a bit more information about eigenfunctions will probably land at the respective Wikipedia page, which is quite good.

Line 163: Does it not make more sense to start with this before you embark on the resulting solutions?

In general yes, but not here. We want the readers to understand how the approach works at first. The following part – understanding in detail why the approach can be applied although it looks like a non-symmetric problem – is more challenging.

Line 164: The fact that you need two such clarifications in one sentence indicates that you are going too fast in the theoretical development. This section needs to be expanded (perhaps in an appendix, see above), because I think you lost most of the readership already at this point.
Or you submit it to a mathematical journal.

Unfortunately, the gap in theory between mathematics and geology, hydrology, environmental science, ... has not become smaller through time. The problem is that such stuff is not scientifically new for mathematicians, but potentially useful at least for some hydrologists, although it exceeds the mathematical background of most of the readers. **We expanded this part a bit (lines 241–253).**

Line 170: How can a spring have discharge if there has never been any recharge, as you claim in the previous sentence?

It was not said that there was never recharge. The solution starts from a given initial distribution of head values $h(\vec{x}, t = 0)$, which is, of course, due to recharge in the past ($t < 0$) or an instantaneous recharge at $t = 0$.

Line 175: Is that word necessary here?

We explained in more detail why a non-orthonormal basis would also work in principle, but an orthonormal basis has a huge advantage (lines 260–270).

The next paragraph too moves too fast through the mathematical fundamentals. HESS is not a mathematics journal.

Have you ever seen scientific papers in mathematics journals? **We tried to provide some more background (lines 271–307).** Nevertheless, we have to go beyond the mathematical of the majority of the readers.

Line 215: According to Eq. (32) you already have these fluxes because you already have q_0 and q'. You appear to contradict your equations here.

No, when moving downstream, $h_b(t + \delta t)$ is still unknown. This is hopefully a bit clearer now.

Line 218: The entire paragraph is confusing. At the start of the paragraph you claim to calculate the fluxes, for which you presumably need the hydraulic heads. You then calculate the hydraulic heads, begging the question how you found the fluxes. Then you state you are going to calculate the fluxes in a second sweep, although you just told us you already have them. Once you calculated the fluxes (for a second time?), you compute the hydraulic heads, even though you stated just before that you need the value of h_b before you can calculate the flux, which implies you already have the hydraulic heads.
I have no idea what you try to convey here.

This specific numerical scheme was also a challenge to the reviewers when it was introduced in the context of fluvial landform evolution modeling. In your case, however, the problem is presumably just that you assume that $h_b(t + \delta t)$ was already known, which is not the case. This is why the second sweep is needed. None of the properties are computed twice during the scheme. **We tried to provide some more information throughout the section.**

Line 249: There was talk about spatially variable storativity and transmissivity. It is not clear how you generated their fields. Even if you make them proportional to the catchment size of their pixel, do you not need some initial heterogeneity to get the process started?

Right – simulated annealing starting from a field of random flow directions. We think that it would not make much sense to describe the algorithm here in detail, **but just refer to Hergarten et al. (2014) (line 428).**

Line 267: I am thinking that by having single target nodes you are lining up in series catchments hat each behave more or less like linear reservoirs. Therefore you are approximating a Nash cascade. The more elements in the cascade, the slower its response, and hence the larger its e-folding time. With short cascades, the effect is not as pronounced.

Right, except that the cascades are not lined up, but arranged like a tree. Your explanation is, of course, why smaller catchments have a shorter recession time. However, it does not explain the difference between single and multiple flow direction. This is an immediate effect of inhibiting flow. Anyway, this paragraph does not contribute much to understanding at this point, **so we removed it (lines 447–450).**

Line 279: If I understand correctly, any point at a drainage divide by definition cannot have any donor points. Is that correct?

Not correct in this strict form. By definition, they only drain towards different points at the boundary (springs). Theoretically, even be two parallel channels might be separated by a drainage divide. Practically, however, points at drainage divides have small catchment sizes (most of them ≤ 3 pixels). **Anyway, we reworded the sentence for clarity (lines 458–460)** (independent of your question).

Lines 282–282: Why is this the case? It makes little physical sense to me because it represents neither hydrostatic equilibrium nor a steady-state flow. Both would represent more realistic and/or practical initial conditions.

It must be like this for uniform, instantaneous recharge (see end of Sect. 2.4). But since the readers cannot have all theory available at each point, **we tried to explain it a bit better (lines 463–469).**

Fig. 3: Please have on legend entry for every line. This way of doing it is confusing.

This looks a bit like your personal preference. **We removed the two entries from the legend** since the dashed lines were already explained in the legend, but we will not introduce a legend with 6 long(!) entries.

Fig. 3: I have no idea what this is.

Hard to believe. Probably you know it under a different name. Equally-sized bins on the logarithmic x-axes, and then ten bins for a factor of 10.

Fig. 3: I thought this was exp. decaying recharge. What term are we talking about here?

We added an explanation to the caption.

Fig. 3: I had to read this three time before I understood. If a reader has not read the main text before this is incomprehensible.

At this point, it may indeed be challenging for readers who only look at the figures and read the caption without reading the main text.

Line 290: Only in the first step. Darcian flow will lead to a much more effective dissipation of gradients in the hydraulic head than dendritic flow, and the resulting smaller gradients will reduce the fluxes in subsequent time steps.

Qualitatively true, but this is just the faster decay. Four neighbors is just like a four times higher T at the same S and this just four times faster.

Lines 299–301: I have not seen this type of dimensional analysis before, where the differential operators in a PDE are used to scale parameters in the solution. Interesting.
The explanation in lines 301-303 is very useful. More of these would be helpful in the theoretical part of the paper.

It is probably because this kind of scaling argument is often considered too sloppy. We used it since we wanted to get around the full scaling theory, which is now included in Sect. 2.6.

Line 302: This only holds for the homogeneous term. r is scale invariant.

Right – although $r = 0$ should be clear for recession scenarios. **We added it (line 488).**

Lines 310–312: Preferential flow also leads to longer tailing, because water stuck far away from the preferential flow paths takes longer to discharge. Did you see that as well?

This depends, of course, on the properties of the homogeneous aquifer used for comparison. In an earlier stage of this study, we transferred α to an equivalent length of a homogeneous aquifer. This length could be seen as an equivalent distance toward the preferential flow system. However, we found that it did not bring much and skipped it before writing the manuscript.

Fig. 5: The darker colors are not easy to distinguish.

We reorganized the colors and the legend.

Lines 321–329: Would it not be more interesting to test a wider variety of heterogeneity structures? Sandy and gravelly aquifers often have lenses in a preferred direction, although that will be difficult to represent in 2D. I think it would also be worthwhile to have alternatives of the single relationship between the transmissivity and storativity you introduced in this paper.

It might be interesting to make a study on lenses in a preferred direction. But as discussed in the general section, it would be very challenging to obtain systematic results. And what should other relations between transmissivity and storativity should look like?

Line 330: This is a long paragraph, but from the text is does not become clear why the topics you address here deserve so much scrutiny.

Perhaps because it is the property that makes the biggest difference between the models/aquifer types discussed in the introduction?

Line 336: What water are you referring to?

We added an explanation (line 531), although it should be clear in the context of instantaneous recharge.

Fig. 6: The legend that declares colors and line types separately is not helpful. Simply explain what each line represents.

This looks a bit like your personal preference. **We removed the two entries from the legend** since the dashed lines were already explained in the legend, but we will not introduce a legend with 4 long(!) entries.

Line 363: This can be interpreted in multiple ways, please rephrase.

We have no idea how it could be interpreted. **Anyway, we rephrased it (line 558).**

Fig. 8: Over which time period?

Of course, up to $t \to \infty$. It is hopefully clearer now.

Line 403–404: Perhaps this is related to my earlier comment about increased tailing caused by preferential flow: the dendritic flow pattern limits access of many nodes to a rapid discharge conduit, and the slow-flow component necessarily grows if the probability of access to a dendritic branch decreases.

See comment to lines 310–312.

Line 405: You only treat this in a rudimentary way. I was thinking about rain showers falling in different areas of the aquifer, perhaps moving over the aquifer.
I am not sure what we are learning from this that gives us a better appreciation of the contribution this paper makes to the existing body of knowledge.

We added a some more interpretation (lines 625–636) and hope that it is clearer now.

Line 413: Eq. (7) represents an aquifer of which the thickness is not dependent on the hydraulic head. Therefore it is unclear what you mean by a completely filled aquifer. The governing PDE does not allow for anything else.

Of course, "completely" does not refer to an amount since there is no limitation for a model with a given storativity. It refers to the spatial coverage. **We rephrased it (lines 610–611).**

Reviewer 2 (Erwin Zehe)

Summary

This is an interesting and well-structured manuscript, investigating the role of energetically optimized preferential pathways, which minimize total dissipation in the network, on the aquifer response to recharge events. Strong emphasis is the recession behavior of such system is in line with the one of karst aquifers, and more generally on its scaling. The study is based on a very solid and partly innovative numerical simulations, comparing full 2d Darcy flow, where each cell potentially drains in all its neighbors, to preferential scenarios where water flows along the path of the steepest descent in hydraulic head. By using a finite volume approach and a function set where the pressure head h declines exponentially with time, the authors obtain a symmetrical eigenvalue problem (for constant S and recharge), which implies that they can express their solution as sum of orthonormal eigenvectors, which have different characteristic decay times (inverse of recession coefficient). Moreover, they derive organized patterns of transmissivity T and storavity S by

minimizing total energy dissipation according to the work of Hergarten et al. (2014).
The authors show that the recession coefficients obtained with the full Darcy model and the dendritic flow patterns are well aligned over the range of investigated catchment sizes, in case of organized T and S patterns. In case of a homogeneous S and T, they find in contrary a strong mismatch. They also show that recession coefficients show power low decline with catchment area, when the system is a strongly preferential, while they find a much stronger dependence on catchment a homogeneous domain. A main conclusions is that the recession of Karst systems is not in accordance with a self-organized preferential flow network. This for the latter the slowest recession component controls 90% of the water that leaving the system, while this fraction is clearly smaller in Karst systems.

Evaluation:

The proposed manuscript deserves without doubt publication in HESS. Yet, I made a few observations which relate to the realism of a few underlying assumption and their implication for real world system that should be addressed within a round of revisions.

Setting the factor of proportionality to one in Eq. 49 is physically not meaning full, because it must be related to recharge of the aquifer. When choosing a catchment area of 1km^2 we end up with a transmissivity of $(10^6)^{(2n/n+1)}$, for $n = 2$ this is larger than 10^7 m^2/s. What does a recharge coefficient of 1 mean, 1 m/s, 1 m/y)? I appreciate when the authors make their equations dimensionless, but this requires to normalize by characteristic areas and recharge rates, to assure that the connection to the physical world remains clear.

We introduced a new section (Sect. 2.6) for the nondimensionalization and provided an example how to transfer the nondimensional properties to physical values in Sect. 2.7 (lines 405–419).

I think it would strengthen the manuscript, when the authors discuss what a non-linear increase in transmissivity with catchment area does actually reflect. For river networks similar to hydraulic radius grows with catchment area, which implies a reduction specific dissipation and thus an increase in energy efficiency of the stream or the rill network (Schroers et al. 2022). Does the power law like increase in transmissivity with an exponent > 1 imply the hydraulic radius of preferential flow paths is growing at this rate (due to confluence in the network)? Or do you think that the extra-growth beyond the trivial linear increase with A comes from a related growth in K? There is work discussing positive feedbacks between dilution and precipitation in aquifers on K (Edery et al., 2021). Maybe this can be related to catchment area as well?

Quite interesting aspects to think about. There seems to be a difference towards rivers (at least at large scales. For the minimum energy dissipation in porous media, only the increase in K with A is stronger than linear, while that of ϕ with A is weaker than linear ($\sqrt{\phi k}$ even increases linearly with A). As a consequence, the speed of the water ($\frac{q}{\phi}$) still increases with A. For rivers, the downstream increase in cross section area (analogy to ϕ) seems to be so high that the speed rather decreases downstream. Concerning the hydraulic radius vs. K we are, however, not sure whether we got your point correctly. The nonlinear increase in K with ϕ could be interpreted in terms of hydraulic radius r. If all pores are equal in size, $\phi \propto r^2$ and $K \propto r^4$ ($n = 2$). In this case, $\phi \propto A^{\frac{2}{3}}$, $K \propto A^{\frac{4}{3}}$, and $r \propto A^{\frac{1}{3}}$. So there would be an increase in r with A, but much weaker than linear. Anyway, this stuff would go deeper into the theory of energy dissipation and drift off from the recession topic. **So we added some more explanation about the relation between K and ϕ (lines 367–377),** but did not dig deeper into this topic.

Closely related to that I wonder about the meaning of the power law dependence of K on the Darcy velocity q (Eq. 38). When solving this for q this implies $q \sim K^{\frac{n+1}{2n}}$, in case of $n = 2$, $q \sim K^{\frac{3}{4}}$. To me this seems to be inconsistent with Darcys law ($q \sim K^1$). So is this in fact a manifestation that Darcy flow is at best approximately valid, when dealing with preferential flow in least energy structures? I would expect that there is a little bit of the head difference/potential energy difference goes into kinetic energy of the preferential flow. So maybe Darcy-Weisbach and a dendritic pipe network would come closer to this?

We have to be careful with the (admittedly quite challenging) theory developed in the 2014 paper. The relation between K and q is not valid at constant hydraulic gradient ∇h, but only among all points in the domain in the state of minimum dissipation. These have not the same ∇h, but $|\nabla h| \sim q^{-\frac{n-1}{n+1}} = q^{-\frac{1}{3}}$ for $n = 2$. So the increase in K with q ($q^{\frac{4}{3}}$) is so strong that points with a higher q even have a lower hydraulic gradient. In total,

$$K|\nabla h| \sim q^{\frac{2n}{n+1}} q^{-\frac{n-1}{n+1}} \sim q^1.$$

The entire theory is only linear Darcy's law. It might be possible to extend it to Darcy/Weisbach or Forchheimer's law might be interesting, but this would be a different story. **We pointed out more clearly that ϕ and K are static properties that do not change at the event scale (lines 392–397).**

Best regards,
Erwin Zehe

References

Edery, Y., Stolar, M., Porta, G., and Guadagnini, A.: Feedback mechanisms between precipitation and dissolution reactions across randomly heterogeneous conductivity fields, Hydrol. Earth Syst. Sci., 25, 59055915, https://doi.org/10.5194/hess-25-5905- 2021, 2021.

Schroers, S., Eiff, O., Kleidon, A., Scherer, U., Wienhöfer, J., and Zehe, E.: Morphological controls on surface runoff: an interpretation of steady-state energy patterns, maximum power states and dissipation regimes within a thermodynamic framework, Hydrol. Earth Syst. Sci., 26, 31253150, https://doi.org/10.5194/hess-26-3125-2022, 2022.