EGUsphere, referee comment RC1
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# Comment on egusphere-2022-889

Anonymous Referee #1

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Referee comment on "Flow recession behavior of preferential subsurface flow patterns with minimum energy dissipation" by Jannick Strüven and Stefan Hergarten, EGUsphere, https://doi.org/10.5194/egusphere-2022-889-RC1, 2022

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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.

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).

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.

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.

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.

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.

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).

L 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.

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.

L 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?

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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:

$$h'(X,t) = h\_r * [\exp( -alpha*t) -1] + h'(X,0) * \exp(-apha*t) \quad (3)$$

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.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-889/egusphere-2022-889-RC1-supplement.pdf