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Comment on hess-2021-14

Anonymous Referee #2

Referee comment on "From hydraulic root architecture models to macroscopic representations of root hydraulics in soil water flow and land surface models" by Jan Vanderborght et al., Hydrol. Earth Syst. Sci. Discuss.,
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Title: From hydraulic root architecture models to macroscopic representations of root hydraulics in soil water flow and land surface models
Authors: Jan Vanderborght, Valentin Couvreur, Felicien Meunier, Andrea Schnepf, Harry Vereecken, Martin Bouda, and Mathieu Javaux

The paper presents a model of root water uptake based on a distributed root architecture system and tries to perform upscaling to make the proposed approach suitable for land-surface models.

This interesting paper focuses on a topic of great interest to the hydrological community. However I have a number of reservations on the current manuscript and thus I suggest some revisions that I consider necessary for a better collocation of the research.

Here is the (unordered) list of comments that should be addressed by the authors.

1. Notation. I do not like the non-standard Matlab-like notation. I think it is confusing and misleading, taking away the attention from the essential components of the model. I had a hard time reading through it. The paper feels more like a cut-and-paste from the matlab code (see supplementary information) rather than the description of a model. For example, if we read through the indices of eq. [5], this is nothing else than a weighted graph Laplacian defined on the graph with which the root system is discretized. It took me a long time to understand this, also because of the uncommon wording (e.g., the connectivity matrix is typically called the incidence matrix in graph theory).

The use of standard mathematical (linear algebra) notation is welcome.

(in line 160 the product between $IM^T \text{diag}(K)$ is not diagonal. May be the authors refer only to $\text{diag}(K)$. Please correct.)

2. The authors describe a discrete model without ever looking at the continuous counterpart. Thus, one is forced to wonder how the discretization affects the parametrization and the solution. There is no answer to this question and it should be discussed at least in the numerical experiments.

3. The distinction between parallel/big root systems and the proposed model really boils down to parametrization of the same model: all of them are based on a linear diffusion-like equation, making the assumption that a potential function exists, and then proceed to upscaling in order to find the K-Q relationship. For example, the parallel root system makes the assumption that resistance inside the root system is negligible with respect to resistance at the soil-root interface and use it throughout to solve (exact upscaling) the related mass conservation equation (i.e. the diffusion-like equation). In this case, the approach is exactly the approach proposed in this paper, as argued also by the authors themselves in a simpler case. Isn't then the difference only related to a different parametrizations of the same model?

Note that the assumption of existence of a potential is reasonable in the linear regime but is prone to fail in a nonlinear regime, not addressed here. The authors at some point comment on linear vs nonlinear models, but they should elaborate more on this. In addition, it is linearity that allow the upscaling, which can be done equivalently (from the mathematical point of view) using a "series/parallel resistance" analogy or inverting the resulting weighted graph-Laplacian (the diffusion-like equation enforcing mass conservation of the system).

In addition, linearity is the main limitation of the proposed approach, as it cannot be extended to the nonlinear case since there is no analytically expressible upscaling and numerics (Newton method) has to be used everytime parameters are changed.

4. Appendix: I don't understand the wording "distal" and "proximal" that have a relative meaning. Eq. [A1] is just the sum of the fluxes entering/exiting node i , i.e., $\text{div } q = 0$. Also I do not understand the change in sign convention for the first term. Also eq. [A2] has a different sign convention. Then, one has to be overly careful in assembling all the fluxes.

Again, I think it wouldn't be bad to use standard linear algebra (graph theory) notation and call IM the incidence matrix of the graph instead of the connectivity matrix. Then it becomes obvious that [A4] is just Darcy's (Ohm) law and [A3] is the mass balance ($-\text{div } k \text{ grad } h = q$).

The developments starting after equation [A5] seems just an application of Gaussian elimination. Is this needed? I am in favor of summarizing the model with some basic equations and then describe the steps used to solve it (finding the K-Q relationship) giving some physical meaning to intermediate steps is necessary only after the full algorithm description is reported. Or the authors could add to all this lengthy (and to me useless) equations a summary of the basic idea (solve for Q when H is known to get the effective conductivity).