

## ***Interactive comment on “A thermodynamic formulation of root water uptake” by A. Hildebrandt et al.***

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**Abstract:** The root water is most efficient if water is extracted uniformly from the soil. In what way does this have implications for forests (with significant lateral redistribution of water through stem flow), or any other systems with non-uniform distributions of water? Whatever the distribution of water in a soil is, this is a given for the root system. The root network does not drive infiltration patterns, so in what way does its desire for a uniform water content have implications?

In fact, perhaps one can reason the other way around: co-evolution of species in natural ecosystems in all likelihood has led to an optimal root network in terms of energy-

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efficiency of root water uptake, effectiveness of capturing water in times of water stress, and energy-efficiency of root development to populate the entire root zone with roots. Despite heterogeneity of soil properties as well as spatial distribution of infiltration this led to root systems that take up water most efficiently in terms of energy use when the initial water distribution in the soil is uniform.

In the last few lines, are you really claiming to predict in which direction evolution will go? That seems a bit bold. Also, plants have been evolving in heterogeneous soils (and probably co-evolving as they influenced the trajectory of the soil genesis) for the past few hundred million years. They seem to have done quite well so far, yet your phrasing seems to suggest this challenge is a relatively recent evolutionary driver.

p. 133856, l. 25 The soil water retention curve may not have a resistance analog, but the matric potential is a direct analogue to the electrical potential. The potential difference between the atmosphere and the soil provides the driving force for transpiration, and thus for root water uptake. Gradient-driven water flow and soil drying both have an effect on the soil water potential that can be readily compared, I would say. One obvious difficulty is the non-linear relationship between the water loss and the drop in water potential. I doubt if this poses a fundamental objection against comparing the effect of different processes leading to water loss at the same location in the soil.

p. 13387, Equation (1) I believe the second flux needs to be summed over all  $j$  not equal to  $i$ . Also, I do not understand the limitation to fluxes between neighboring reservoirs in a conceptual model. It implies that the outer two reservoirs have only one neighbor and the inner two have two neighbors. But their arrangement is arbitrary. Why not permit fluxes between all reservoirs, in which cases my suggestion to sum over all  $j$  unequal to  $i$  applies. According to your text and figure you need to consider one value of  $j$  for the two outer compartments, and two values of  $j$  for the inner compartments, which is inconsistent.

p. 13391, Equation (5) I disagree with the integration over the entire soil volume that

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is implied here. This integration should only take place over the water-filled portion of the soil volume, and thus the volume of integration will change with the water content. The integral here is similar to that of Zehe et al. (2006), which I compared with other averaging methods in de Rooij (2011). I do not like having to put my own paper forward here, but it covered the various methods of averaging potentials presented in the literature in some detail, and therefore has direct relevance to this paper.

Equation (5) and (6) in combination are wrong if the water content is not only a function of  $z$  but also of  $x$  and  $y$  (the horizontal coordinates). By including the volumetric water content, but only permitting it to vary with  $z$ , you implicitly establish the requirement of a horizontally uniform water content. But in reality, you in fact require the average water content at elevation  $z$  multiplied by the area at elevation  $z$ . The latter permits you to use soil volumes of arbitrary shape. These complications can be avoided by integration over the water-filled pore space, as suggested in the previous comment. Doing so amounts to multiplying the intrinsic average of the binding energy (or any other property of the soil water) by the water volume, and thereby links the paper more clearly to the considerable body of literature that studies scale transitions like the one you are interested in here. See for instance the series of papers by Quintard and Whitaker that appeared in *Transport in Porous media* in the 1980s and 1990s. The reference below (Quintard and Whitaker, 1988) elaborates on upscaling through volume averaging most extensively, I trust you can trace the rest if you start your search there.

p. 13392, l. 14-17. Please rephrase. I believe 'replace' means 'to be transferred to the soil'. Can energy be negative?

l. 18-22. I believe this applies to non-uniform wetting of a uniform soil. A heterogeneous soil should be heterogeneously wetted to minimize the binding energy. The distribution of the water will clearly conform to the soil heterogeneity, and as such is a deterministic function of the configuration of the heterogeneous pore architecture, but it will definitely be non-uniform.

I. 26-28. I can imagine a scenario where the opposite applies, and which makes more sense from the plant's point of view. The curves in Fig. 2b are themselves based on volume averaging: they represent the relationship between the average water content and the binding energy integrated over the volume of water in the soil volume for which the average water content was calculated. Around these graphs are clouds of dots of local water contents with their local binding energies. The root hairs of a plant experience these local values, not the average. The population of root hairs of a single plant will therefore experience at any time a range of bounding and gravitational energies among its root hairs. I argue that plants either take up the water only from those spots where the least amount of energy needs to be spent to get it, or (more general) that the local magnitude of water uptake is a function of the local energy status of the water that is such that the water uptake decreases if the local potential energy of the water is lower.

The first hypothesis implies that the root hairs that are in wet spots take up the water there until the energy of the remaining water equals that of root hairs in drier spots. Initially, a soil with a markedly heterogeneous water distribution will have a considerable amount of water that is more easily taken up by root hairs than one would expect based on the average curve. An uneven water distribution will therefore result in less energy expended for water uptake than in case of an even distribution of water, contrary to your statement. This is the case because the water uptake will take place preferentially from those spots where the sum of the gravitational and binding potential is highest. As root water uptake progresses, the water remaining in the initially wet spots will reach energy levels comparable to drier spots, and the root hairs tapping those locations then also start taking up water. This process continues to ever drier soil spots with root hairs in them until the water demand of the plant is met, water stress stops the water uptake altogether (wilting), or rainfall/irrigation resets the water status in the soil profile and the root hairs adjust their uptake accordingly.

Thus, root water uptake itself is a major factor in making the soil water distribution more

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uniform, while simultaneously reducing the overall water content. Thus, the blue arrow in Fig. 2b should not point downwards, but has a trajectory that moves downward and to the left simultaneously in vegetated soils in dry periods.

In short: when discussing root water uptake I think it is crucial to not only look at average soil water properties, but also to account for the distribution of the local variations around these average water contents and binding energies since these drive the root water uptake strategy of a root system with a large population of root hairs that each can only experience one of many local values.

p. 13393-13394, Eqs. (8) – (12) The energies of the water in the compartments is kept constant in these equations. This implies that steady state conditions are assumed. But for both root water uptake and large scale applications, both key to the paper, such a limitation would be too strict to yield anything of relevance. You apply your model to transient conditions, which leads me to believe that you also derived the equations for transient conditions. If believe these involve expressions that indicate how water contents and energies are updated between time steps. This basically amounts to presenting discretized versions of the conservation laws of energy and mass. (I expect that the use of the intrinsic average as suggested above will prove beneficial in formulating these equations.) Please present the full set of equations for transient conditions to give us a complete overview of the model.

Eqs. (13) – (14). This are energy balances. For clarity and completeness, it would perhaps be good to include the heat term to show explicitly what the dissipated binding and gravitational energy is converted to, and to add the corresponding mass balance equations. The latter would be useful to address my next comment.

One thing that makes Eqs. (8) to (15) a bit hard to interpret is the fact that they combine two contributions to the change in gravitational and binding energy: a change in the gravitational and/or matric potential (integrated over the water content in the soil volume of interest), and the change of the volumetric water content (integrated over the soil

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volume of interest). I believe more can be learned about energy dissipation if these terms are separated out in the equations.

l. 13395, l. 10-14. I agree with the first part of the statement (the collar xylem potential would need to be lower when water is taken up from a drier soil), but disagree with the second (. . . and also when soil water potentials are more heterogeneously distributed). Similar to my argument above, I believe plants take up water preferentially from areas where it is energetically advantageous to get (Adiku et al, 2000). Your statement is based on an interpretation of Fig. 2, and thereby implicitly assumes that plant roots obtain water indiscriminately from the entire root zone irrespective of the local energy status of the extracted water.

We did some work some time ago with measuring root water uptake in a root zone (from the local water depletion) while at the same time measuring the matric potential (van der Ploeg et al, 2008). The gravitational potential was known of course at each measurement location. We saw root water uptake start in the top of the profile (where irrigation caused relatively wet conditions initially). Under water-stressed conditions, we saw evidence of root water uptake activity moving deeper into the profile. Clearly, the root water uptake had a 'strategic' element in it, although we suspect it was not strictly energy-related, but also seemed to be largest in locations where the slope of the retention curve was small, i.e., a small drop in xylem potential energy could yield a sizeable amount of water delivered to the roots. This is the only piece of direct experimental evidence that I am aware of, but there is a lot of literature on this topic.

p. 13395, l. 21-23. Ignoring soil water redistribution is a rather dangerous assumption in the case of root water uptake. Roots only take up water during daytime, and the redistribution of water around the roots during night time is crucial for next day's uptake. Indeed, evidence is beginning to mount that roots excrete substances that facilitate water flow towards them, perhaps to increase the hydraulic conductivity in their immediate surroundings, where the radially converging flows create need high gradients to get the water through to the root hair. Furthermore, roots acting as passive but con-

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ductive conduits for water at night allow water within the root zone to be redistributed more uniformly in the root zone during the night (although there still is some debate about this process).

The day-night rhythm in root water uptake also makes the constant-flux boundary condition debatable, doesn't it? Do you really need this assumption? It seems to me that it compromises the validity of your study considerably. That being said, for a proof of principle, this should not be major problem. But for any quantification of energy dissipation of root water uptake in ore natural systems, it will be.

#### Result section

Unfortunately, since I believe that the basic assumption about the nature of root water uptake being independent of the local soil water energy status is invalid, I do not consider the model runs and their outcome valid. I think this assumption needs to be replaced (or compared) to one that takes into account energy-sensitive root water uptake strategy before the model can yield meaningful results.

Also, both the model in its current state and the example problem that is examined are very simple. I believe the authors can be a bit more ambitious in their model development. The underlying equations are not wildly complicated. There is no need for a proof of principle on a problem that is too simple to have any relevance for realistic systems to be published separately before the model can be applied to more realistic scenarios. At this time, this second stage is entirely missing, and that clearly weakens the paper.

When a more comprehensive modeling exercise has been performed the advantages of this approach should be much clearer. I would welcome a paragraph explaining what the main contribution of this approach is. I expect it will be in bringing soil hydrology and thermodynamics closer together. The relevance for more application-oriented research may be less clear. If this is indeed the case, than what is missing is a connection with other works striving to do the same: the work of Gray, Hassanizadeh, and Miller on

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the pore scale and larger scales (with most paper appearing in the 1990s but still ongoing I believe in *Advances in Water Resources*), but also some very useful work by Groenevelt and Bolt in the 1960s already.

I would also like to see simulations of continuous root water uptake vs. day-night cycles with redistribution at night to see how that affects the energy needed.

On a different note: does the plant actively expend energy when taking up water? It does not need to actively lower its water potential, but instead can rely on the generally very low water potential in the atmosphere. Through its capillaries and its tissues that potential is passed on to the roots. All it needs to do is to regulate its stomatal resistance and possibly other resistances that plant physiologists know better than I do to moderate that potential to the degree best suited for its purpose and then let the resulting potential drop draw the water in. There is a biological advantage of minimizing the energy dissipation when the plant is water-stressed because the plant that can generate the lowest water potential in its root hairs can get more water in than less efficient plants with roots nearby. Under non-stressed conditions, is there an advantage to minimizing energy dissipation that I am overlooking?

Overall, I think the paper presents a fresh way of looking at root water uptake that merits attention. On analyzing the paper I found several assumptions that I find unconvincing, but that do not appear to be crucial to the model. Most of these assumptions were implicit. I would recommend a more rigorous mathematical derivation of the model equations based on fewer assumptions and simplifications. I think this is feasible. Those assumptions that remain should be stated explicitly, and their validity tested if possible.

More fundamentally, I believe the way the authors used and interpreted Figure 2 is incorrect. I do not think one can use a representation of the average features of a non-linear system to derive the behavior of the system when that behavior is the sum of local processes driven by the local values of the averaged quantities.

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Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/12/C6334/2016/hessd-12-C6334-2016-supplement.pdf>

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Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 12, 13383, 2015.

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