Hydrol. Earth Syst. Sci. Discuss., 11, C204–C210, 2014
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Hydrology and Earth System Sciences Discussions

HESSD

11, C204–C210, 2014

Interactive Comment

Interactive comment on "Parameterizing complex root water uptake models – the arrangement of root hydraulic properties within the root architecture affects dynamics and efficiency of root water uptake" *by* M. Bechmann et al.

Anonymous Referee #1

Received and published: 27 February 2014

I am globally not satisfied with the answers from Bechman et al. This is explained in the following lines.

On major issue # 1:

When introducing a new index, it is good to clearly put forward its general definition and equation before developing a mathematical definition only valid in conditions that may be interpreted as unrealistic by the reader.

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Interactive Discussion

Discussion Paper

In the discussion paper, the effort "Psi∼" only appears in two equations (6 and A4) which are both subject to the constant flux assumption. Also, several sentences give the reader hints about a hypothetical definition of effort:

- Index "of root water uptake (. . .) used to quantify (. . .) overall resistance to root water uptake".

- "Effort estimates overall resistance to root water uptake".

- "Effort uses xylem water potential at the root collar to estimate the efficiency of root water uptake"

- "Efficiency criterion Psi∼ called effort"

- "We relate effort to the average work necessary to take up water before entering water stress at time t∼"

- "The effort relates to the time evolution of water potential and gives a measure of the total resistance to root water uptake of a root system"

- "We introduce (. . .) effort (. . .) as a proxy for overall resistance to water uptake under unstressed conditions"

- "Effort is assumed to be proportional to the water-normalized work w(t)"

The authors explain what it is used for, what it estimates, what is its symbol, what it relates to, and what it is proportional to. But none of these sentences says what it actually is. Rather than evoking a misunderstanding from the referee, the authors should acknowledge a "misdefinition" of what effort possibly is.

On top of that, in their answer to referee # 1, the authors do not only change the nomenclature of effort (now "w"), but provide a definition of effort that does not correspond to its previous characteristics. According to equation (A4) in the manuscript, effort equals the water-normalized work at the onset of water stress (Psi∼=w(t∼)). There was thus one single value of effort for one scenario. In their answer, effort becomes the time-variable water-normalized work (w(t)), with no reference to the onset of water stress. In figure 1 (c) of the answer to referee # 1, the calculation of effort is indeed not limited to the period before water stress, for the time variable flux condition. However, in the same figure, but for the constant flux condition, water potentials after the onset of water stress are not used in the calculation of effort (otherwise effort would continue increasing after the onset of water stress). The authors should try to be consistent with the definition of newly introduced concepts. Also, the conceptual error in the definition of water-normalized work reported in RC28 is a major issue to be treated in priority.

The definition of Water Yield (V∼) was not subject to such issue since its general equation (Eq. 5 in the manuscript: V∼=V_total(t∼)/l_total) is rather clear. For convenience, the authors might want to separate the last term in an additional equation, since it is the only one subject to the assumption of constant flux condition. Water yield is also said to be "related to the amount of water that can be taken up by the root until the onset of water stress". If this is true, the water yield of the young root system under variable flux condition (see its collar water potential in Fig. 1 (b)) must be less than 0.075 times the water yield of the same root system under constant flux condition. However, the authors say in the manuscript and repeat in their answer to referee # 1, that the variable flux condition does not affect the results as compared to the constant flux condition. How is that even possible?

I must underline that already in the manuscript abstract, the authors affirm that "the average uptake depth is not influenced by parameterization", which is also obviously wrong according to the results displayed in figure 8 of the manuscript.

On major issue # 2:

In their answer to referee # 1, the authors mention that effort is a "new measure", while water yield is a "previous measure" (of the efficiency of plant water uptake I suppose). I admit I didn't know the latter index (which I think is different from "water productivity"), and didn't notice references to such index in the introduction. However, in the abstract and the section 2.4 of the manuscript, the authors mention that they introduce or define two indices, which suggests that they are both new.

The authors also mention that water yield is strongly related to overall transpiration, while effort is more related to whole plants function, which I suppose would make them complementary. They are actually both strongly related to overall transpiration (power in equation A3 also equals the flux exponent 2 divided by root system hydraulic resistance, even though the authors used a different, but equivalent, expression for power). Regarding water yield, in addition to be strongly related to overall transpiration, higher values of water yield must occur for lower root system hydraulic resistance (because of earlier water stress). The fact that both indices are so correlated is thus not a surprise.

If the authors like their index (effort) to be strongly related to the scenario, they should not repeatedly state that it is a measure of root system overall resistance, but mention that it depends on both the chosen scenario (daily potential transpiration, daily fluctuations of potential transpiration, initial soil water potential, soil hydraulic properties, etc.) and root system hydraulic resistance. I acknowledge that the choice of an index is also a matter of personal taste, but at least the authors need to be clear about which variables it is sensitive to, which is absolutely not the case in the manuscript.

Also, when stating that "On total, these points make the perspective of using these indices to parameterize complex root water uptake models illusory", I accounted for the fact that both indices are not very sensitive to parameterization (especially water yield). Such perspective seems far from being credible to me.

On major issue # 3:

The misuse of the concept of axial limitation is explained in RC35. In the background, the authors define the concept of axial limitation as the fact that "the overall root resistance decreases due to an excessive root length as compared to the optimal length". Beyond that optimal length, the overall axial resistance would increase more than the overall radial resistance decrease, and thus provoke an increase of the overall resistance. In their results, the authors never verified if the overall resistance increased beyond a root length, for the simple reason that they never calculated the overall root resistance (effort is not the overall root hydraulic resistance). Moreover, at page 777, L17-18, the authors use the term “axial limitation”, but not to explain that the root is too long and its overall resistance increased. They mention that the fact that “the maximum uptake occurs at root ends is counterintuitive (. . .) because the uptake at root tips should suffer from axial limitation”, which is the common use of the term axial limitation, but does not correspond to the definition of axial limitation introduced by the authors in their manuscript. The authors should avoid using the same term for two different concepts.

Two basic principles of hydraulic architectures were also completely overlooked by the authors.

Firstly, in a hydraulic architecture seen as a succession of root segments, each having a radial and an axial conductance, adding a root segment (i.e. increasing the total root length) is equivalent to adding a conductance in parallel (even if the root is unbranched). Adding a conductance in parallel in a hydraulic network never provokes a decrease of the overall hydraulic conductance of the network (unless radial conductances would be null). This invalidates the whole background section and the newly introduced idea of axial limitation, which must be considered as very misleading.

Secondly, in that kind of hydraulic architecture, the overall hydraulic conductance of the network depends only on the values of local hydraulic conductances and on the way they are connected (just like in electric circuits). In no way the distribution of soil water potentials or water flow in the architecture will change the overall conductance as long as local hydraulic conductances and their connections do not change. The arguments developed by the authors in page C151 of their answer to referee # 1 are a pure nonsense, the moving uptake front does not impact the overall hydraulic resistance of the root. In case roots of the same length also have the same architecture, local hydraulic properties and number of segments in figures 2 (a) and 2 (c), then they also have the

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same overall hydraulic resistance.

The figure included in my previous comment illustrates overall hydraulic resistances calculated from Thevenin theorem (Thévenin, 1883), but could as well be calculated by dividing the water potential difference between root collar and soil by the transpiration rate (which can be done from figure 2 (a)). It is indeed convenient to calculate it in conditions of uniform soil water potential, but this value of overall root conductance is of course also valid when the soil water reservoir is limited and soil water potentials become dynamic.

In figure 2 (a) the authors show that for chosen transpiration rate and soil water potential, root collar water potential does not increase with root length, which is the direct confirmation that root overall resistance does not increase with root length (for sufficiently fine root discretizations). Interestingly, in figure 2 (c), the authors show that effort increases with root length (even though root hydraulic resistance does not change). I would be curious to see the evolution of collar water potential for these scenarios, but seemingly, effort is a misleading estimate of the overall root hydraulic resistance. A possible explanation is that a longer root (let's choose the 5 m root in figure 2 (a), as compared to the 4 m root in the same figure) distributes its uptake in smaller portions, which provokes a slower decrease of local soil water potentials. It would thus take more time to reach the critical collar water potential. In case the plant with longer root spends this additional time at collar potentials that are relatively close to the critical collar potential, its effort might be higher than the effort of the plant with shorter root. In other words, even though the collar water potential of the plant with long root is always lower than the collar potential of the one with short root, its effort might eventually be higher. This would contradict another statement of the authors about effort : "lower effort is tantamount for lower xylem water potentials over the course of time". In the case of water yield, it should be even clearer that the longer root does not stress before the shorter root. The water yield of the long root should thus always be higher.

Reference:

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Thévenin, L.: Extension of Ohm's law to complex electromotive circuits, Annales Télégraphiques, 10, 222-224, 1883.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 757, 2014.