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Discussions

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.

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General comments:

This paper raises interesting questions, and figures and tables are mostly clear.

The authors present a version of Doussan et al. (1998) model of the root hydraulic architecture, for which each root segment is associated to a soil element non-interacting with other soil elements, in order to emphasize the effect of simple combinations of root types (young and mature) and topologies on the dynamism of root water uptake. Then the impact of these combinations on two efficiency indices is illustrated. The first index

is called effort and corresponds to the time averaged plant collar water potential before reaching water stress. The second one is called water yield and corresponds to the cumulated transpiration until water stress is reached, divided by the total root length. Both indices are shown to be sensitive to combinations of root types and topology. Then the authors carry out a similar sensitivity analysis using a more complex model accounting for soil water flow with a single complex root system architecture, but different degrees of root maturity (e. g., 60% young root segments and 40% mature root segments). A perspective would be to use these two indices to parameterize root hydraulic properties distributions.

Regrettably, many sections of the paper are unclear due to lacking units when defining new symbols, errors in equations, conceptual inaccuracies. Many statements are also not well motivated, and some of the concepts defined in this paper are misused. Most of the background section being affected by the artefact reported in RC7, it should be considered as misleading, and removed from the paper. Many figures, the result and discussion sections, should also be corrected accordingly. The choice of the efficiency indices is not convincing, since they appear to be quite correlated to each other, sensitive to the chosen scenario, not very sensitive to root topology and maturity (especially water yield). Moreover, the indices require the plant transpiration rate to be constant until water stress is reached, which makes their calculation for real plants implausible. On total, these points make the perspective of using these indices to parametrize complex root water uptake models illusory.

Specific comments:

Since comments on the abstract are partly justified by comments made in the body of the text, I will finish with the abstract.

Introduction:

RC1, P761, L7: The meaning of the expression "these models resolve the root geometry . . ." is unclear. "Solve water flow equations within soil and root system architecture" might be more adapted.

RC2, P761, L12: The meaning of the expression “compensation of local water stress” is unclear. “redistribution of root water uptake due to local limitations of soil water availability” or “due to soil water potential heterogeneity” might be more adapted.

RC3, P761, L15-17: Redistribution of root water uptake is actually reproduced by Feddes et al. (1976) model when coupled to Jarvis (1989) model (see also Simunek and Hopmans (2009)). They are however not based on quantitative hydraulic principles, which is their drawback as compared to Doussan et al. (1998) model. The argument should thus be more balanced.

RC4, P762, L18: The statement “parameterization has to be based on intuition” is a bit strong and sounds like “that’s how it should be”, which is not true. The point that the hydraulic parameterization is based on scarce quantitative information and is thus generally complemented by qualitative information on roots anatomy might be more appropriate.

RC5, P762, L29: The point is interesting. Just for the record, Choat et al. (2012) published similar results in Nature a year earlier.

RC6, P763, L9-12: This sentence is a conclusion, not an objective or context. It should be removed or reformulated.

Background:

RC7, P763, L16-P764, L12: Even in the uniform unbranched root used in the example, there are actually an infinite number of parallel radial pathways (of the same radial resistivity), each corresponding to a different relative position to the top of the root. The consecutive axial pathways are more or less resistive depending on the position of the radial pathway (less resistive if closer to the top). For simplification purpose, the authors postulate that these pathways can be summarised as two single pathways in series (one radial and one axial). Consequently, they find out that the total resistance of

the uniform unbranched root is a function of its length. This function is first decreasing and then increasing for long root lengths. Unfortunately, the increasing part of the function is an artefact due to the simplifying assumption of the authors (see the figure attached to this document, in which "n seg" is the number of segments in which the same unbranched root is discretized). The authors might want to characterize the shape of the function for different discretizations of the same uniform unbranched root. They will first notice that the function is sensitive to the number of segments in which the root is divided, and then that the increasing part of the function tends to disappear with refinement of the root discretization. This artefact undoubtedly affects a large part of the results and of the discussion.

RC8, P763, L21-P764, L14: None of the variables in this section is presented to have units. For the sake of clarity, when new variables or parameters are defined, in any section of the paper, their units should also be given.

RC9, P764, L14: The expression is reported to have “m” units, which is wrong. That expression has “mˆ(3/2)” units.

RC10, P764, L15-19: Following RC7, “axial limitation” as defined by the authors does not exist. It would however be interesting to discuss the sensitivity of the total root resistance to axial and radial resistances, according to root topology and to the distribution of hydraulic properties.

RC11, P765, L1-2: The fact that the root water uptake dynamics is sensitive to the ratio of radial to axial hydraulic conductances was already reported in the introduction with different references (see P762, L2-5). These should be grouped.

RC12, P765, L4-5: Following RC7, this statement is wrong. It would however be interesting to balance interests and drawbacks of extending roots in the introduction. The access to water and nutrients, as well as the carbon cost to build roots, should be mentioned. Lynch (2013) wrote an interesting paper on the topic.

Material and methods:

RC13, P765, L23: The sentence is unclear. The word "altered" seems misused.

RC14, P766, L6-7: It is not clear whether the authors mean "water content" or "water potential" was uniform.

RC15, P766, L7-8: I guess that the authors mean that there was no soil water flow between soil layers. Water redistribution might also be due to root hydraulic redistribution. This point should be clarified.

RC16, P766, L8-9: It is not clear whether the authors mean that the soil "hydraulic properties" are uniform or that the soil "hydric state" was permanently uniform (bulk approach). Also, in case of a bulk approach, is the soil hydric state uniform around each individual root or on the whole soil profile? This should be clarified. Also, the volume of the soil buckets should be given.

RC17, P766, L15: Resistance units are missing. According to other units, they should both be "s mˆ-2".

RC18, P767, L2: Units for the radial resistivity are thus wrong. They should be "s".

RC19, P767, L14: Equation 3 does not cover the case of root branching. This should also be accounted for.

RC20, P767, L24-26: This type of water stress boundary condition is typically referred to as "isohydricity". It would be good to mention it.

RC21, P768, L7-8: I understand that this assumption was made for reasons of simplicity. I would add that it was made to explain in simple terms the impact of soil water potential distribution on root water uptake distribution for different root hydraulic types. It totally makes sense. However, when stating that this assumption is "suitable when root water uptake velocity dominates soil water dynamics", the authors should define the concepts involved (i.e. domination; comparing distributed velocities to dynamics)

and referring to literature detailing in which conditions this statement would possibly be true. I would actually remove the second part of the sentence of the paper.

RC22, P768, L8-10: Unless I missed a point in the recommendations of HESS, I think that results of the current paper are not supposed to appear in the methodology section. The methodology should have its independent justifications. Furthermore, stating that "results of the simple model are in good agreement with those of the complex one" does not make sense per se, especially since they represent different types of root systems and are used to simulate different scenarios. The authors probably meant that they expected redistribution of root water uptake to occur in both models results since they are both based on root hydraulic principles.

RC23, P768, L22-24: Same comment as RC22.

RC24, P769, L6: The reference seems inappropriate. Richards (1931) or a reference to a 3-D solver of Richards equation (such as SWMS-3D or Wave) would probably fit better.

RC25, P770, L20-25: The authors explain criterions defining whether root segments are young or mature. As far as I understood, the given criterions are not sufficient to isolate one possible distribution of root properties for each percentage of maturity. Did the authors use a threshold root age as additional criterion to discriminate between young and mature root segments? This part should be clarified.

RC26, P771, L23-24: Effort was first presented as an "index for overall plant resistance", or an "index used to quantify the overall resistance to root water uptake", but surprisingly, it turns out that it is an average collar potential. When reading these lines, I first questioned myself about why making this index so vague and mysterious, why not directly saying that your first index is the average plant potential before stress, symbolized as "Psy$\sim$", which you expect to be sensitive to overall plant hydraulic resistance? My second reaction was to think that "Psy$\sim$" is also very sensitive to the chosen scenario (i.e. to the transpiration rate, to the initial soil water potential, to the critical leaf water potential, ...), but you essentially want the index to inform you on the plant property (overall plant resistance), not to inform you on the chosen scenario. Then why not making it simple and straightforward by using the plant overall resistance as index? This index would also have the advantage to be measurable on real plants, in opposition to effort, which would require a real plant to transpire at constant rate to be estimated.

RC27, P771, L24: As presented, it is not clear what kind of average the authors used when defining w(t). The term "ratio of cumulated work to cumulated water uptake" might make it clearer. I actually found it misleading to use the symbol "w" for an average work which actually does not have units of work (same remark for "V∼", which doesn't have volume units). And again units are not defined with the new symbol, which makes it even harder to catch the exact signification of the variable.

RC28, P785, L17-P786, L17: Here I jump to the Appendix A to continue commenting on "w". Again in this section, units would be helpful to the reader. The perspective to quantify water transfer in terms of work is interesting. However, equation A2 contains an error. In the electric analogy, the voltage corresponds to a potential difference between a region of high potential and a region of low potential. Applied to soil-plant water dynamics, the region of low potential is indeed the collar, while the region of high potential is the soil. The soil water potential is thus missing in equations A2, A3 and A4. Under constant transpiration rate, "w" thus equals the difference between time averaged collar and soil water potentials. See Gardner and Ehlig (1963), Lhomme (1998) and Couvreur et al. (2012) for more information on such electric analogy.

RC29, P772, L1-5: The mathematical definition of effort should be corrected according to RC28. The sign of effort will thus be positive instead of negative.

RC30, P772, L6-7: Figure 2 should be updated according to RC7 and RC28, except if the authors want to keep the time average collar potential as index. Then only the sensitivity of the results to root discretization should be verified (see RC7). Also grad-

uations of the vertical axis are not always visible.

RC31, P772, L7-8: The authors state that in figure 2, "it can be seen that in this case water yield is proportional to "t∼". The only evidence for that in figure 2 is the equation of water yield, which was already presented (Eq. 5) and told to be proportional to "t∼" earlier. This sentence should thus be removed. Also the equation for water yield in figure 2 is wrong; the root length term is missing.

RC32, P772, L16-17: As discussed in RC26, the effort is not a measure of the total resistance to root water uptake of a root system. Effort is sensitive to the total resistance, and extremely sensitive to plant transpiration rate. If the authors wanted to give a measure of the overall plant hydraulic resistance, they should have given the overall plant hydraulic resistance.

RC33, P773, L7: Figures 3 and 4 should be updated according to RC7, and possibly to RC26. Same for figure 5 and RC7.

RC34, P773, L12-14: Here and in several other parts of the paper, optimal lengths given by both indices are very similar. This is not surprising since water yield is proportional to "t∼", which is sensitive to the plant overall resistance, to the transpiration rate and to the initial soil water potential (in the same way as effort). Then why making a second index? They are quite correlated; the main difference being that the second index is less sensitive to the properties of interest.

RC35, P777, L17-18: Here the authors mention the classic concept of axial limitation (i.e. root water uptake is expected to be reduced at the outer ends of the root due to the longer, and thus, more resistive axial pathway to reach the collar), which is different of the definition of axial limitation given in the background section (i.e. the overall root resistance decreases with length due to the axial resistance). They should be careful not to use the same expression for different concepts.

RC36, P777, L19-22: The effect of axial limitation is said to "become apparent in the overall higher resistances which lead to the increased effort". Again, the overall resistance decreases not due to an increased root length (see definition of axial limitation given in the background), but to an increased axial resistance. For the record the overall resistance always increases with both axial and radial resistances (and is generally more sensitive to the radial resistance).

RC37, P778, L7-11: The statement on heterogeneous root systems compensating root water uptake more efficiently is not well motivated. For that, compensation should have been quantified and a clear trend presented. A modelling study of Couvreur et al. (2012) recently demonstrated that compensation of root water uptake is proportional to the root system overall hydraulic conductance. The fact that heterogeneous root systems generated by the authors have a higher overall conductance than their homogeneous root systems would explain better why they do more compensation. Also, in order to justify their statement, the authors should verify if heterogeneous root systems with high overall resistance do more compensation of uptake than homogeneous root systems with low overall resistance.

Discussion:

RC38: Most of the discussion should be adapted according to the comments on the background, methodology, results and appendix A.

RC39, P780, L9-11: The simplified model is said to be "sufficient" because its results are similar to those of the complex model. Sufficient for what? To observe redistribution of root water uptake as well, yes. To quantitatively model soil water dynamics accurately as compared to the complex model, no, such quantitative comparison was not carried out. The authors should clarify this point.

Additional comments on tables and figures:

RC40: For convenience, I would have expected table 1 to be included in the "Root properties" section of table 2.

RC41: For convenience, figure 5 should also present results for n=4 and n=6.

RC42: In figure 8, the units of z50 are missing.

RC43: In figure 9 (right), the label of the y-axis indicates "fraction of bleeding from transpiration", which is misleading since the bleeding flux is not a part of the transpiration flux. "Ratio of bleeding to transpiration" would be more suitable.

Abstract:

RC44: The abstract should be adapted according to the comments on the background, the methodology and the results.

RC45: It is also mentioned that the average root water uptake depth is not influenced by parameterization, which is in contradiction with both figures 8 and 6.

Technical corrections:

P770, L8-9: The use of the word "therefore" seems inappropriate to me since the sentence is not a direct consequence of the previous one(s). Probably a few elements are missing.

P770, L22: I would remove the coma.

P773, L3: I would add commas before and after the expression "water yield and effort".

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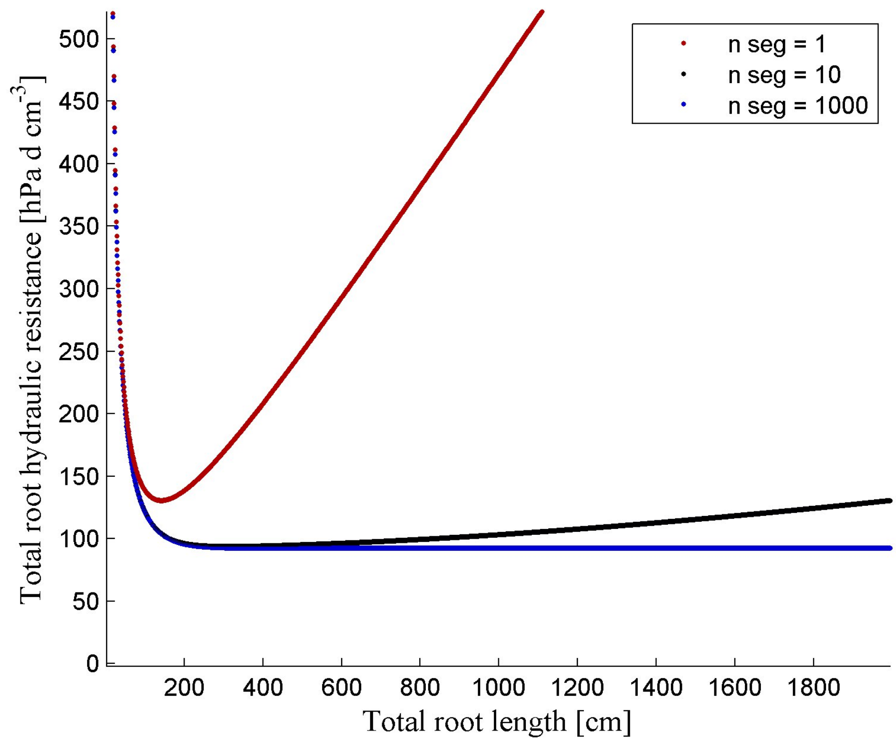

Fig. 1.