

Interactive comment on “A simple three-dimensional macroscopic root water uptake model based on the hydraulic architecture approach” by V. Couvreur et al.

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To referee#1:

We are grateful to the referee for his/her very positive comments concerning our paper.

To referee#2:

We would like to thank referee#2 for the time and thought involved in the reviewing of our paper. We highly appreciate both of the positive and constructive reported comments. For reading convenience, we reported and numbered the referee's comments

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as follows:

RC1: "At this point, however, I still wonder how the implicit model results were obtained: it is not clear to me, how the simplified root architecture was derived from the complex one."

AC1: In this paper, there are indeed two different root architectures, one which is simple and another which is complex. There are also two different RWU models, one which is complex (Doussan model, which solves physical equations of water flow in an explicit hydraulic architecture (HA)) and another which is simple (the proposed model, which is a simplified solution of water flow equations governing the RWU rates at the soil element scale). While the simple RWU model was deduced from the complex one, it is not the case of the root architectures. The simple root architecture is totally independent of (and was not derived from) the complex one. The simple root architecture enabled us to obtain an analytical solution of water flow equations in an explicit HA. Because the variables and parameters were visible in the analytical solution, we could rearrange them to make appear a common structure and create a simplified solution with macroscopic parameters at the soil element scale (the simple RWU model). The role of the simple root architecture stops here. Does the simple RWU model apply to complex root architectures? If so, its specific properties should appear in Doussan model numerical solutions for complex root architectures (checked in section 4.1). How to find the values of the macroscopic parameters of the simple RWU model for the complex root architecture? By solving Doussan equation in uniform water potential conditions (for SSD and Krs) and in non-uniform water potential conditions (for Kcomp) (this is explained in section 3.2). As this point seems not to be clear enough in the paper, we will develop it in section 3.2 and in the discussion.

RC2: "A revision, as far as I am concerned, should focus on elucidating how the simple (implicit) model is exactly dealt with."

AC2: See the last two sentences of AC1.

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RC3: "Since the implicit model still has many adaptable parameters, it should not be such a surprise that it can mimic an even more complex one."

AC3: The implicit model has one vector parameter (the SSD whose length equals the number of soil elements) and two scalar parameters (Kcomp and Krs). These parameters do not need to be adapted during a simulation as long as they are independent of the soil water potential distribution and of the transpiration rate. In comparison, the explicit model requires, for each root segment, two hydraulic properties parameters (the axial and radial conductances) and also information about how the root segment is connected to the rest of the root system architecture. Since the number of soil elements is generally lower than the number of root segments, it is clear that, in addition of being less complex, the implicit model has much less parameters. As compared with the widely accepted Feddes RWU model, both SSD and RLD vectors of parameters are defined at the soil element scale and thus have the same length. While Feddes stress function requires several parameters dependent on the transpiration rate, the implicit model only needs the Krs parameter which does not require adaptation during the simulation. Like in Jarvis function (1989), our model only needs one parameter to predict the compensatory RWU process. Therefore, the newly proposed model requires less or as much parameters as common RWU and water stress models. Finally, we think that having a lot of parameters is not sufficient to enable a model to mimic a more complex model without changing its parameters at each time step. The adapted model structure is a fundamental feature of the implicit model which allowed it to mimic the explicit model while having much less parameters. These points were already developed in the discussion part of the paper and we envisage rephrasing them to make them clearer.

RC4: (About the introduction) "I expect that quite some readers will wonder about some of the used definitions. Collar water potential being one of them. (...) What is a catenary process (...)."

AC4: Indeed, adding these definitions may be valuable to the reader. These will be

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added in the revised version of the paper.

RC5: “I wonder why the new model is called ‘implicit’. In numerical work, this term is usually reserved to solving the equations.”

AC5: The term ‘implicit’ used to characterize the proposed model is more general than that used in numerical works about schemes for solving equations. As explained in section 2.4, the proposed RWU model is “referred to as the ‘implicit model’ because it does not explicitly take into account the root system HA” (i.e. its inputs and outputs are defined at the soil element scale and no root system architecture has to be considered in a simulation using the implicit model). Nevertheless, the model structure and its parameters keep an ‘implicit’ footprint of the root system HA because they were deduced from solutions of water flow in a HA. Since the word ‘implicit’ used here has the same meaning as in the ordinary language, we estimate that no definition of this word is necessary for the reader to understand it. Yet, a clearer description of the reasons why we call the proposed model ‘implicit’ may be valuable to the reader.

RC6a: “Considering the comparison of the two model approaches in the introduction, one wonders how well the HA modeling is *a priori* parameterized.”

AC6a: We guess that the referee meant the first model approach developed in the introduction and that developed in the theory. We consider the explicit model parameterization as a good compromise between detail level and measurability of the parameters. As explained in section 3.1.1, all the information about root segment hydraulic parameters (radial conductivity and xylem conductance) and root system architecture were found in the literature, based on laboratory or field measurements.

RC6b: “For instance, are the resistances & conductances for the two models related with each other, and how?”

AC6b: In the implicit model of the root system HA, two conductance parameters appear. Both of them depend nonlinearly on the set of axial and radial conductances of

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the explicit model (see illustration in Fig. 5 for Krs) and on the root system architecture (trivial for Krs, cf. Thevenin theorem). In most of our observations, Kcomp is close to Krs but not equal to it (except when there is no xylem resistance to water flow, cf. Tab. 2). These points were already developed in the paper and we envisage rephrasing them to make them clearer.

RC7: “For the presentation, I propose that eq. 2 & 3 are also moved to the appendix. As it is now, only for $Q_r,1$ the result is in the main text for no obvious reason. Furthermore, the reading is unnecessarily complex if you refer to a term (Phi), used in the rest of the paper, without telling ‘which second term you mean’. Just replace (2) by the expression involving Phi, and some function $f(R)$ and refer for details to the appendix.”

AC7: The referee is right. This type of presentation is unnecessary complex, it will be simplified in the revised version of the paper.

RC8: “Three interesting features are observed in relation with eq. 2, 3, and the appendix. I wonder why they are so interesting. As presented, the impression is given that (first) being a sum of two terms multiplied with a dimensionless factor is interesting. As such, it is not. The interesting thing might be that the expressions concern an easily separable Tact, compensatory term and a reference uptake fraction. It is obvious, after recognizing the compensatory term, that it is zero if soil water potential is uniform. That the sum of dimensionless factors is one for the entire root architecture is not surprising or interesting, but a consequence of what it is: a relative uptake rate for each of the pathways of water uptake (if soil water potential is uniform). In short, I think that this discussion of p. 4948 and 4949 should be turned around: (1) analytical solutions can be given for the simple architecture of figure 1, (2) these solutions can be cast in the general form $Q_r = (Tact + Phi_i) \cdot (f(R))$, (3) where we recognize a compensatory and a reference uptake fraction term, (4) the first one zero at uniform soil potential and the second one summing up to unity.”

AC8: The first interesting feature about Eqs. (2), (A1), (A2) and (A3) could indeed

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be explained more clearly. About the second and third features, we had no a priori knowledge about the terms that would appear in the analytical solution of water flow equations in the HA. The noticed properties of these different terms allowed us to recognize what they actually were: a compensatory RWU term and a “reference uptake fraction term”. The clues allowing recognizing them were considered as interesting points. This part will be rephrased in the revised version of the paper and the structure suggested by the referee will be considered in the creation of the new simplified structure of section 2.1.

RC9: “Whereas RWU is a well-known acronym for root water uptake, the newly introduced vector SUD is not. I would call this vector a ‘uptake fraction’, because it is a fraction, and I would not give it the symbol SUD (which is more as a computer code identifier), which later could be interpreted as multiplying S, U and D: I would give it a real symbol, as you do also in the other equations. E.g. f_U . I admit that it is a matter of taste.”

AC9: We understand the reviewer concerns. However, we think that the expression ‘uptake fraction’ would not be suitable to describe what this vector is, because the uptake fraction changes with the soil water potential (due to compensatory RWU), which is not the case of this vector. In contrast, the word “standard” reminds the reader of the fact that the vector gives the uptake fractions in “standard conditions” only (i.e. uniform soil water potential). About the computer code identifier “SUD”, we think that it is not a problem if the acronym already exists as long as the probability that both of them are used in the same paper is negligible. Finally, we think that the reader will not interpret SUD as the multiplication of three variables because we used the “.” mark as multiplier identifier in all of the developed equations. Nevertheless, the word “fraction” would make it clearer to the reader that the elements of the vector are fractions and that their sum equals 1. Therefore, we propose to use the acronym SUF for the “standard uptake fraction distribution”, $SUFI$ being the “standard uptake fraction at root segment i ”. In parallel, we propose to use the acronym SSF for the “standard sink

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fraction distribution”, SSF_k being the “standard sink fraction at soil element k”.

RC10: “I believe that (9) has been derived under the constraint that alpha=1 although line 5-6 p. 4953 says differently, but if correct (otherwise, I am lost somewhere), to avoid ambiguity, this should be mentioned in line 7 of p. 4951, or, alternatively, line7 should not start a new paragraph.”

AC10: We guess that the referee wanted to mean p. 4951. Indeed, (9) has been derived under the constraint alpha=1, which is equivalent to $Rx,i \ll Rr,j$. The referee is right, for clarity, it will be mentioned that the constraint still applies in the new paragraph.

RC11: “The potential difference of line 8 should be better explained, i.e., to what this flux refers (the index j is not sufficient).”

AC11: The referee is right. The interpretation of (6) ranging from line 7 to line 9 will be better explained.

RC12: “The authors are of course free to call their conductance (line 8) a compensatory conductance, but I doubt that this gives much understanding to the readers. Their case is presented ‘too economically’ to my taste.”

AC12: To be complete, Kcomp is interpreted as being “the effective conductance of the compensatory RWU process”. We have the feeling that, in the field of RWU modeling, “compensatory conductance” recalls the conductance of the compensatory RWU process. Moreover, we think that giving a parameter a name which is short, intuitive and in adequacy with its “role” will make it easier for the reader to remember its signification. We thus consider that the name “compensatory conductance” is appropriate to this parameter and would like to keep it as it is in the revised version of the paper.

RC13: “Lines 13-15 are obsolete, if the constraint made regarding alpha is presented better.”

AC13: The referee is right. The development of the equations in section 2.2 will be better structured in order to avoid the presentation of obsolete equations.

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RC14: "Again, p. 4952, line12, interesting features are announced. (...) The first feature is not so surprising to me and I propose that the various terms in 10 are renamed SUDj, for brevity."

AC14: For brevity, we could replace the various terms in 10 by SUDj. Nevertheless, it is far from trivial that the exact analytical expressions of the SUDj would appear as weighting factors of the soil-root interfaces water potentials in the prediction of the plant collar water potential. Eqs. (9) and (10) were developed independently and even we did not expect that a common parameter would appear in both of them before demonstrating it. In consequence, we consider the 'first feature' of high interest and will more clearly emphasize that point.

RC15: "p.4953: line 10: for a very special root system, is that what you want to say?"

AC15: No, we mean that the root system architecture domain (composed of root segments delimited by root nodes) is different from the soil domain (composed of soil elements delimited by soil nodes). An equation developed to predict RWU at the root nodes locations cannot be used directly at the soil element scale. This sentence will be rephrased for clarity.

RC16: "Line 14-18: I find your way of formulating this quite difficult to follow. This can be said more straightforwardly. The text is ambiguous for me. Why is information transferred from the soil grid to the root architecture? Basically your aim is to go from architecture to soil grid, right?! What is the root node? (...) Since now the soil element becomes more important (than in the introduction), it is now time to define exactly what you mean."

AC16: The referee is right, this paragraph is difficult to follow. Actually, in an explicit model of the root system HA coupled to a soil water flow model, information needs to be transferred in both directions: from the soil domain to the root architecture domain in order to know what are the values of the water potential at the soil-root interfaces (needed to predict the RWU rates with Doussan equation); and from the root architec-

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ture domain to the soil domain in order to know what are the sink terms (needed before solving Richards' 3D soil water flow equation). The root node is a point element of the root system architecture domain. Two adjacent root nodes form a root segment. All the root segments together form the full root system architecture. A soil element is a volumetric element (for instance, a cube) of the soil domain delimited at its corners by soil nodes. All the soil elements together form the full soil domain. The soil and root nodes/elements can be defined more explicitly in the introduction if necessary. Lines 14-18 will be rephrased.

RC17: “Concerning eq. 12: as I understand it, Q is determined by (9). The delta factor is only 1 for one value of k , for each i value, but for each k value, more than one i can give a delta that is 1. Please indicate so. Now, all RWU together (all k) should not be larger than the transpiration demand. How is that ascertained?”

AC17: Indeed, Q is determined by (9) and for each i value (i.e. each root node), the delta factor is 1 for only one value of k (# of the soil element). In other words “each root node is contained in only one soil element”. Indeed, for each k value, more than one i can give a delta that is 1. In other words “a soil element can contain several root nodes”. Note that for some k values, no i give a delta that is 1. In other words “a soil element can be empty of root nodes”. The SSD vector is thus just a different partition of the elements of the SUD vector and we consider as obvious the fact that their sum is the same (i.e. 1). Consequently, all RWU together (all k) always equal Tact. For clarification, the properties of the SSD can be detailed after presenting Eq. (14).

RC18: “Please delete eq (13): it is trivial.”

AC18: This equation can indeed be removed without altering the clarity of the development.

RC19: “p. 4954: you should rephrase line 9-11:...is the value of... should become ..., which we call...”

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AC19: The referee is right, this will be done.

RC20: "The approximation that you make: how big is the error that you make, for the considered examples later in the paper?"

AC20: Estimating the weight of each individual approximation made to obtain the proposed model was not an objective in this paper. Only a global error was estimated and it showed to be negligible (<1% and <2% of relative difference respectively for the soil water content and for the sink term) at any time of any of the tested scenarios (see section 4.2.1).

RC21: "p.4955: explain line 1-2. What do you mean with 'not explicitly into account', but 'sensitive'?"

AC21: See AC5.

RC22: "Further, I wonder whether you really wish to use the word 'verify' in line 21. I suggest that you use a more neutral term."

AC22: The word 'verify' will be replaced by the word 'check'.

RC23: (About typo's/English).

AC23: The authors want to thank the referee for taking the time to report typo's and English errors. All of these errors will be corrected; commas will also be inserted at many places.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4943, 2012.

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