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Discussions

Interactive comment on "Determination of empirical parameters for root water uptake models" *by* M. A. dos Santos et al.

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Reply to "Interactive comment on "Determination of empirical parameters for root water uptake models" " by Referee#2

In response to the Anonymous Referee #2 :

We are thankful for your critical reading, constructive questions and suggestions that will help to improve the paper. In the following we address the general questions and the numbered specific questions are addressed thereafter.

i) Regarding the dependence of the model parameters on transpiration rate, indeed this topic is explicitly addressed in the results and discussion. We can also shortly address

this topic when introducing and discussing about the Feddes and proposed reduction functions at the end of the section 2.

ii) We introduced some general advantages concerning the use of empirical models as compared to the De Jong van Lier et al. [2013] physical model in page 2, lines 23 to 27.

iii) As suggested we can enhance the conclusion section in order to include the aspects regarding the dependence of empirical model parameters on soil properties and hydraulic conditions. We will address the variation of the other parameters as suggested.

iv) It not possible to directly retrieve root water uptake from measurements. This is one of the advantages of using physical-based models. The main purpose of this paper is to evaluate empirical models that can be sensitive to and follow the variations of root water uptake due to different scenarios of soil and plant properties as well as climatic conditions as predicted by a physical model. By using root water uptake it is possible to strictly capture the root water patterns predicted by the models, whereas for instance if using soil water content the results can be "blinded" by the sensitivity of RWU on soil water content which vary with soil type. Using transpiration may lead to wrong predictions on root water uptake. Some of these aspects can be addressed more specifically and a short examination on using transpiration can be performed in order to show this.

Next we respond to specific questions.

1. P4: root length density R. Shouldn't that have dimension L L^{-3} ?

Yes, it will be corrected.

2. P4: The authors propose a stress function α which is a stepwise linear function of M. Since M is a function of h, the new stress function will be a function of h also. But the shape of the function will have a different shape than a piecewise linear function

of h. Furthermore, the relation between the new stress function α and h will depend on the hydraulic soil properties and will therefore be different in soils with a different texture. The original Feddes $\alpha(h)$ function depends on the transpiration rate as shown in Figure 1. Figure 1 suggests that the new stress function $\alpha(M)$ does not depend on the transpiration rate. I do not understand why the transpiration dependency of the stress function disappears when α is expressed as a function of M since M does not depend on the transpiration rate.

This is a very important observation. In fact the new α function will also depend on potential transpiration rate T_p. This dependency is implicitly expressed in the critical value M_c of M. Therefore, as in the case of the Feddes α function there should be two values for M_c: one for low T_p and other for high T_p. Fig. 1 will be correct to include this.

3. P5: ln 15: "Because T_a and h_l are unknowns, eq. 8 and 10 cannot be solved analytically, but an efficient numerical algorithm is described in De Jong van Lier et al. (2013)." I did not understand this. I thought that either $T_a = T_p$ is known as a boundary condition so that h_l can be calculated or $h_l = h_w$ is known and T_a is calculated. I think that the reason why the h_l (or T_a) cannot be derived directly is because the set of equations that needs to be solved (including also all $h_{0,i}$'s) is non-linear in $h_{0,i}$.

Thank you for other very important observation. As you noticed well, the sentence is wrong. Indeed the set of equations can be solved analytically (and we in fact used an analytical solution by De Jong van Lier et al. [2013]) for some special cases of Brooks and Corey [1964] soils, but not in direct way. This will be corrected.

4. P5 ln 17 and p 29 Figure 2: There are several things I do not understand about Figure 2. The figure caption says that the plant transpiration was set to 1 mm d^{-1} . Shouldn't for a fixed rooting depth the root water uptake or sink term S be constant and independent of the root length density R until a threshold soil water potential is reached? This threshold will depend of course on the leaf water potential and the root length density. Can it be that the curves shown in Figure 2 shown the maximal possible

sink term as a function of the soil water potential for different leaf water potentials and root length densities? But, when the root water uptake goes to zero, why doesn't the soil water potential then go to the leaf water potential? Now there seems to be a 10 m difference between them. Second, why doesn't the root water uptake for a certain soil water potential then not increase with decreasing leaf water potential. For sufficiently large (small absolute value) soil water potential, the root water uptake becomes independent of the leaf water potential. I do not understand this since the water potential difference increases with decreasing leaf water potential and therefore the root water uptake should also increase with decreasing leaf water potential.

i) For a fixed rooting depth, the root water uptake (RWU) is physically given by the soil water flux at the root surface $q = -K\partial h/\partial r|_{r=r_0}$ integrated over the root surface area divided by the soil volume exploited by the root. In the De Jong van Lier et al. [2013]) model this is represented by eq .6. Therefore, root water uptake depends on root length density R, as root surface area increases with R. Leaf water potential will also affect S as shown in Fig 2 since it will affect the soil pressure head at the soil root interface.
Then, your question is very pertinent since in the empirical RWU models we assume a reduction curve of the type you mentioned in your question (a threshold-type reduction function), whereas the physical RWU model shows this curve is different: there is a continuous reduction ever since the soil starts to dry. This reduction curve is even more complex since the water uptake in one layer is dependent also on the whole rizosphere uptake, i.e. the uptake in one layer is influenced by the uptake in other layer which is overall controlled by the h_l value. In that case it might happen that h_s in one given layer decreases while total uptake increases. There is no α function that can account for this, but we hope that introducing a compensation factor in such approach this overall RWU distribution can be closely mimicked. Thus, physically the reduction function given by eq. 18 is more suitable (see the discussion in the reply of RC1, topic 13). These aspects and more thorough analysis on this will be included in the revised version.

ii) The RWU is zero when the pressure at the soil-root interface is equal to bulk soil pressure head h_s. In that case, the h_l must be lower than h_s because of the water head loss from the root to the leaf.

iii) Fig. 2 does show RWU increases with decreasing water potential for a certain h_s value. However, for less negative h_s, RWU becomes less sensitive to high negative h_l values.

5. P7 ln 21: "where $T_{p_{\mathsf{max}}}$ is the maximum possible transpiration rate attained when $M_0 = 0$". This assumes that the minimal water potential at the soil-root interface is h_w (wilting point). But, doesn't this minimal water potential depend also on the critical leaf water potential h_l?

Yes, it depends on h_l. The limiting pressure head at the soil-root interface (called h_{ws} to avoid confusion) is less negative than the limiting h_l (called h_{wl}). Although h_{ws} depends on h_l and on plant and soil hydraulic parameters, for the sake of simplicity we considered it as constant and equal to -150 m. The h_{ws} value was the limiting value used in the empirical models that depends on M and it will be listed in Table 2.

6. P7 Eq. 17: Why is M_0 constant with z? The soil root interface water potential can depend on the depth, can't it?

We take advantage of your question and correct eq 6 to explicitly make $M_0 = M_0(z)$. However, De Jong van Lier et al. [2008] did assume M_0 constant with depth in order to solve the problem of the two unknowns: T_a and M_0. They made a justification for that, and we refer to their paper (**?**) for more detail. With this assumption it was possible later on to Jarvis (2011) make a comparison with the Jarvis (1989) model.

7. P8 ln 15: "The Jarvis (1989) model predicts RWU by a weighting factor between ρ and M throughout rooting depth". This is not very clear to me. What do you mean with a weighting factor "between $\rho$and M"? Do you mean a weighting factor that is equal to the product of ρ and M?

An interesting feature of the analogy between the Jarvis model and the De Jong van Lier et al. [2008] model is that the analogy is derived based on the assumption that stress only occurs when everywhere at the soil-root interface limiting conditions are reached. It is assumed that M_0 is zero everywhere in the root zone. But, I am wondering whether the De Jong van Lier et al. [2008] only predicts stress under these conditions. Can it be that stress occurs even though $M_0(z)$ is not zero everywhere in the root zone? If this is the case, then the analogy between the Jarvis and the De Jong van Lier et al. [2008] models is not given always when stress occurs.

A weighting factor was meant as equal to the product between ρ and M divided by the integral of this product over the rooting zone. As M_0 in the De Jong van Lier et al. [2008] model is constant over depth, stress occurs when M_0 over the root zone.

8. P 8 ln 22: "The smaller λ, the more water is taken up in deeper soil layers" I would reword this to "... the more water is taken up from layers with a low root length density".

We will change the sentence accordingly.

9. P 9 ln 1: "RWU is calculated by substituting eq. 23 into eq. 3, following the Feddes approach." This implies that you multiply Eq. 22 again by $a(z)$. So in the nominator, you get α^2?

Yes, that will be the case.

10. P9 ln 16: Same comment as above.

No, in this case it will not happen.

11. P 9 ln 18: "In drier soil layers, Γ is reduced, whereas in wetter soil layers Γ is increased, thus increasing RWU in these layers before the onset of transpiration reduction." I do not understand this. If the soil dries out but faster in the upper layers where the root length density is higher than in the deeper layers, the deeper soil layers will not get wetter so Γ will not increase in the deeper soil layers, which are still wetter than the upper soil layers. But, $\zeta(z)$ will increase in the deeper soil layers that remained

wetter.

Indeed this sentence needs to be rephrased. As you put out well, in fact Γ in wetter soil layers will not increase, but ζ will do because Γ in these layers will be less reduced compared to Γ in the upper dryer layers.

12. P9: Proposed empirical model. Is in this model also the $\alpha(z)$ factor of the Feddes model used?

General question on the used models: The Feddes stress function $\alpha(z)$ is besides a function of the soil water potential, also a function of the potential transpiration rate. How is this considered in the different models? It should be noted that Eq. (20) suggests that ω_c in the Jarvis model is a function of the transpiration rate but the $\alpha(z)$ used in the Jarvis model is according to Eq. 18 not a function of the transpiration rate. Furthermore, the modified version of the Feddes model shown in Figure 1b suggests that there is no dependence of the α_m function on the transpiration rate and that α_m depends only on the matric flux potential. When looking at table 4, it seems that there is no transpiration rate dependence of the Feddes parameters.

The proposed root water uptake models are obtained by incorporating ζ_m into eq. 23, then into eq. 3. The PM uses Feddes reduction function whereas PMm uses the proposed reduction function α_m as shown in Table 1. We will add more information in this part in order to it get more clear.
The dependence of the models on potential transpiration are implicitly built-in in the values of their empirical parameters that were optimized. For instance, in the Feddes reduction function there are two values for h_3: one for low T_p (h_{3l}) and another for high T_p (h_{3h}). The dependence of T_p in the other models are accounted for similarly. We then optimized the models for two levels of T_p (1 and 5 mm d^{-1}, therefore the optimized parameters are derived for low and high T_p.

13. P11 ln 26: "For high non-linear problems as the one in eq. 29 GLM depends on the initial values of b." This needs to be reformulated. The GLM does not depend on the

initial values of b but the optimized parameter set may depend on the initial value of b since the GLM is a local optimization algorithm that may converge in a local minimum instead of the global minimum.

We agree with your observation. We will reformulate this sentence accordingly.

14. P 12: "3.2.1 Growing season simulation". This is not a sub section of the optimization section.

Yes, it will be changed.

15. P13 ln 8: "$h_w(= -200$ m)". I am confused here because at p 10 it is written: "The value of the parameter h_4 was set to -150 m.".

We discussed this above in point 5.

16. P15 ln 30: "showed by the presence of an outlier and lower medium. "→" "shown" and "median"

Thanks for noticing. It will be corrected.

17. P17: Growing season simulations. It would be good to have more background about the potential transpiration and the precipitation during the considered growing season.

We will provide these data in Table 6 or add other table or figure.

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