Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-59-AC1, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

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

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Received and published: 29 June 2016

In response to the comments by N. Jarvis:

We are thankful for you critical reading, constructive comments and suggestions that will help to enhance the paper. In the following we address the main questions and the numbered specific questions are addressed thereafter.

Regarding the conclusion that proposed models are recommend, we can make a more thorough analysis, as you suggested by applying the Akaiki information criteria to support the conclusions. Regarding the values of λ (that will be called l_m) of the proposed models, they can be greater than 1, as discussed in more detail in points 9 and 16. In applying the evaluated models in blind predictions the JMII may have more advantages over the other models as it is more physically based. This will be shortly addressed

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in the final version. One of the reason why the models PMm and JMm perform better than JMII is detailed in the reply of point 16. Another reason, as raised by you, might be the fact that the threshold type function can mimic a constant plant resistance that dominates in the early stages of soil drying. Nevertheless, as commented by the Referee#3, the fact that the proposed and the De Jong van Lier et al. [2013] reduction function are both a function of matric flux potential might be favourable to the proposed models. Yes, the uncertainty of soil hydraulic parameters might affect the outcome of the comparisons and as you agreed it is ok for our modeling exercise. We will discuss this issue according to your suggestion.

We will take your suggested titles into consideration.

Next we respond to specific questions:

1. Page 2, lines 24-25 (and line 3 in the abstract): I am not so convinced of this. I would prefer to use a physics-based model even if it did have two or three more parameters, as long as they were, in principle, measurable. The limiting leaf water potential is quite well known, at least

R.: We also would prefer using a physics-based model, but in practice it appears not to be appealing. Root water uptake (RWU) models are usually embedded in larger hydrological models, for instance the ecohydrological model SWAP (De Jong van Lier et al., 2008), and most users are unfamiliar with plant hydraulic parameters, making them to prefer the simplicity of empirical models like the Feddes et al. [1978] model, as long as empirical parameters are available. Besides, apart from the well-known limiting leaf water potential, radial root hydraulic conductivity has a strong effect on RWU distribution as shown in the paper and it is not easily available.

2. Page 4, line 5–8: the sentence starting ... “using h seems ....” is wrong, as the authors know well enough. It is immediately contradicted by the text at lines 14-21 on the same page (and by the results shown later in the paper). This sentence should be deleted. The remaining text just says that various forms have been proposed for the α

function, but that making α depend on M is physically the most plausible. This is quite sufficient

R.: The sentence is in fact misleading. It was intended to say "Comparing to θ, h seems to be more feasible..." instead of "Using h seems". It will be corrected as such and then it will not be contradictory anymore. In the text below, it only states that the use of M is more plausible than both h and θ. We hope this to become clear with this slight modification.

3. Page 5, equation 8: $h_0(z)$ is not defined, as far as I can see?

R.: Correct, it will be defined.

4. Page 5, lines 20-22: yes, it would be good if you mentioned this phenomenon by its name: hydraulic lift or hydraulic re-distribution. You could also cite Jarvis (2011) here, since he discussed and clarified the relationship between water uptake compensation and hydraulic lift in some detail (see the text in relation to equations 13 to 15 in the final version of this paper, not the HESS discussion paper that you cited: see point 4. under "Presentation")

Agree, it is important to mention the name of the phenomena as well as cite Jarvis (2011). These changes will incorporated in the text.

5. Page 6, lines 1-3: I know what you are trying to say here, but it is not so well expressed. You could replace i.) " .... is only relevant" by ".... it only needs to be explicitly addressed ...." and ii.) "... becomes less important" by "... is not necessary". This would help, but you could also add a sentence at the end of this saying that the effects of compensation can nevertheless be explicitly discriminated and identified in physics-based models. This is demonstrated in Jarvis (2011) in the text related to equations 13 and 14 in that paper (again, in the final version).

The text will be improved accordingly. However, we don't think it is always possible to explicitly discriminate and identify the effects of compensation in physically-based models. Such relation (Jarvis [2011] eq. 13 and 14) was easily found for the De Jong van Lier et al.[2008] model comparison (Jarvis, 2011). Furthermore, adding this comment would be contrary to our general reasoning when we add that “In physical models, discriminating compensation is not necessary since in such models “compensation” follows implicitly from the RWU mechanism”.

6. Page 6, lines 11-12: “In principle, any definition of α is applicable...”. Yes, perhaps, but it does make a difference to the results of course, as you demonstrate very well later in the paper! But what is definitely not debatable is that Jarvis (1989) used a threshold type function for α based on water content (degree of saturation). The reason for adopting this approach was discussed by Jarvis (1989) in relation to the experimental evidence available at that time and no other type of function was considered. The fact that you adopt a Feddes-type function means that in the rest of the paper you cannot refer to this model as the Jarvis (1989) model. It is a modified Jarvis (1989) model, in exactly the same way that JMm is also a modified Jarvis (1989) model, where the threshold water content function is replaced by a threshold function of matric flux potential: in other words, you investigated two different modified Jarvis models and you should refer to them as such, both in table 1 and throughout the rest of the paper, including the abstract (perhaps you could call them JMm1 and JMm2?)

R. Indeed, any kind of α might provide different predictions. We agree that using the Feddes reduction function in the Jarvis (1989) model is also a modification of the Jarvis (1989) model, and will refer to it in the paper as such (it will be call JMf).

7. Page 6, line 28 to page 7, line 4: this is a little vague. You followed quite closely what Skaggs et al. (2006) wrote in this section, but since they wrote their paper ten years ago, it is now much better established exactly how the original Jarvis (1989) model departs from physicality. This was clarified in the papers by Jarvis (2010, 2011), which you also discuss in the following section. There are two aspects to this:

i.) the choice of function for α. The threshold function chosen by Jarvis (1989) doesn't make complete physical sense, as the local resistance to uptake should in principle increase continuously as the soil dries (e.g. like equation 18). Jarvis (1989) discussed this choice in terms of the overall resistance to uptake being dominated by an air gap between soil and root which might only develop after a certain critical water deficit was reached: this choice was strongly influenced by experimental studies which showed such an effect. Also, at high soil water contents, the overall resistance to uptake in the soil-plant system would be dominated by plant resistances, which may be more or less constant. Thus, a threshold function might be a good choice from an empirical point of view. In this respect, it can also be pointed out here that the authors also adopt a threshold α function in the PMm model.This model is the one the authors finally recommend, because it works best, although it can certainly be criticized on the same grounds (i.e. that it "affronts the definition of α").

ii.) Compensation under non-stressed conditions. As you point out, under non-stressed conditions the Jarvis (1989) model does give a different uptake distribution compared with the de Jong van Lier physical model. However, it is wrong to imply that the Jarvis (1989) model does not predict any compensation under non-stressed conditions (page 7, line 4). Under non-stressed conditions, water uptake is increased by a factor of $1/\omega$ in all layers (regardless of the pressure head distribution) to maintain transpiration at the rate demanded by the atmosphere during soil drying. It is also not wrong in principle to link compensation to plant stress (page 7, line 3): the onset of stress certainly does affect the nature of compensation: this is demonstrated in Jarvis (2011) in the text following equations 13 and 14 for the physics-based model of de Jong van Lier (2008).

For the above reasons, I strongly suggest that you delete the text on page 6 line 28 to page 7, line 4 and replace it by a short sentence that simply states that the Jarvis (1989) model departs from complete physicality in some respects and that this is explained in the following section. Then at the end of the next section (i.e. after equation 21) you can briefly summarize how the Jarvis (1989) model departs from physicality, based on the comparison with the physics-based model that is represented by equation 14-21. This will be very much clearer.

i. The proposed models are also based on a threshold-hold type function for α. However, the fact that Jarvis [1989] model affronts α definition is related to how the model functions, and not the α definition itself. That is, the model has a reduction function accounting for RWU reduction due to soil water resistance. For a given soil layer i, it follows that $S_i < S_{p_i}$ if $\alpha_i < 1$. It means RWU is reduced by soil water resistance, accounted for by α. However, a situation occur when $\alpha_{2_i} > 1$, causing $S_i > S_{p_i}$ even if $\alpha_i < 1$. In other words, there should be a reduction due to α ($\alpha < 1$), but in fact an increase in RWU might occur. We therefore state that the Jarvis [2011] model is in conflict with α definition, as first noticed by Skaggs et al. (2006). On this regard, the proposed models function differently: RWU is first partitioned by weighting factor between M and R^{λ}, then it is reduced by α. Anyhow, we agree that just citing Skaggs et al. (2006) should be enough in this part.
ii. We agree that this needs to be corrected. The Jarvis 1989 model predicts compensation under non-stressed conditions. Regarding the linking of compensation to plant stress, it depends on how compensation is defined or understood. Following the-Javaux et al. 2013 definition, compensation is an independent process of plant water stress, driven only by soil hydraulic re-distribution. However, in this paper as discussed in section 2.1 we stress that the term "compensation" is not relevant in physical-based models, since it follows implicitly from RWU mechanism. Furthermore, we stressed that compensation term was used to interpret the difference in results predicted by empirical models. Interpreting such results from a physical model, there is no need in referring to "compensation", as it is an implicit RWU mechanism.
Nevertheless, we agree that text will be become clearer by deleting the mentioned part.

8. Page 7, lines 5-12. The parameter h_3 does not exist in the Jarvis (1989) model (see lines 10-11 especially). I think this paragraph can be deleted (or perhaps moved to the results and discussion section). At the very least, readers should be reminded that the

original Jarvis (1989) model does not use a Feddes-type α function.

OK, this will be moved to a proper location in the text

9. Page 8, line 22: you should add the limits for λ here. If compensation means that water uptake increases from sparsely rooted layers, then λ must lie between zero and 1. Also, you should replace "deeper soil layers" by "more sparsely rooted layers" to be strictly correct.

We agree that an explanation is missing about the limits for λ as well as how λ values affect RWU and compensation. In fact, λ is not restricted to the domain between 0 and 1. The λ values were originally based on experimental works, but essentially it changes the shape of RWU over depth by giving more weight to R or α in partitioning RWU. For $\lambda = 1$, RWU is partitioned by a simple weighting factor between α and R. For $\lambda > 1$ ($\lambda < 1$), R (α) becomes more important on partitioning RWU.

10. Page 9, lines 23 to 26: I wonder what it is about your modification to the Li model (the use of the matric flux potential in a threshold function) that resolves the conceptual difficulties with the original formulation that you described earlier on page 9 at lines 3 to 8. As far as I can see, the same objections should be equally valid for this modified version as for the original model. This should be clarified and the text modified accordingly.

The main objection regarding the Li et al. [2001] model is the use of α in ζ (eq. 22). Thereby, "compensation" taking place before transpiration reduction (when $\alpha = 1$ for all soil layers) can not be computed: RWU is distributed over depth only by R^{λ}. By using M instead of α, "compensation" before transpiration reduction can be computed. As M integrates both the effects of K and h, it might be a better soil hydraulic function than K or D [Molz and Remson, 1970; Selim and Iskandar, 1978] to account for the effects of soil water in partitioning RWU. Such comments will be added into section 2.2.4.

11. Page 11, lines 13-28: As I understand it from table 3, you only have a maximum

of two parameters to calibrate for all the models, while each parameter is constrained within known limits. This means that a "brute force" grid search for optimum parameter values would be preferable to the method you chose, since you could be sure of avoiding risks of finding local minima (although it might be slower). I am sure there is no need to repeat the calibrations, but maybe you could mention this?

R.: The "brute force" grid search is a very slow method. As there are many scenarios and some models to evaluate, we dot not think it is interesting to mention it since it would not be applicable in practice (a very small grid would also be required to avoid finding relative minimum).

12. Page 13, lines 23-24: yes, this may be why a constant value of ω_c often seems to work quite well. Maybe you could add a comment to this effect, and also refer to your equation 20 and cite Jarvis (2011), where this aspect is discussed in detail.

R.: As eq. 20 gives an expression for ω_c derived from the De Jong van Lier et al. [2008] physics-based model [Jarvis, 2011], it indeed helps in accounting for some aspects relating RWU phenomena. A constant ω_c might be quite robust as can be inferred by eq. 20 and from common field observations. However, adding such a comment in this part (page 13, lines 23-24) might get out of the context of the paragraph.

13. Page 15, line 18: You should replace "either R or M" by "both R and M". But this sensitivity to M is in principle also present in the empirical models that include M. Why is it more important for JMII? Is it because this model is not calibrated? Or is it because of the different type of function? I can believe that predictions of JMII are, in comparison with the empirical models, more affected by the value of M_{max} , which must be a very uncertain parameter, not least because the Mualem-van Genuchten model of soil hydraulic properties is known to have an incorrect form close to saturation (since it does not allow for a maximum size of pore in soil). These questions should be clarified.

R.: Comparing the models JMf (earlier abbreviated as JM) and JMII it is clear that Jarvis 1989 model type is affected by the defnittion of α. This becomes more evident analyzing Fig. 1 (see its caption below and figure at the end) which shows α of JMII (eq. 18) as a function of soil pressure head h and ω_c (eq. 20) for different soil types, expressed by M_{max}. Focussing first on the α function, it can be seen that despite the fact the soil resistance should increase continuously as soil dries as you point out in point 7, defining α by eq. 18 does not seem very realistic. In this case α is suddenly reduced even close to saturation. When $h = 1$ m, for instance, α is much lower than 0.5. Such a behaviour does not correspond to the α definition. Another interesting point is the values of ω_c which are also extremely low. The low α values are, however, balanced by high α_2 values (due to low ω and ω_c values), leading to suitable values of RWU in a given soil layer. Nevertheless, the magnitude of α and ω_c are physically questionable. SOme interestinh points that can be drawn from the above: i) the ω_c value which sets the compensation level depends on the α definition. For instance, Jarvis 1889 stated that $\omega_c = 0.5$ is a moderate level of compensation. Surely, it does not hold if α is defined by eq. 18. ii) Comparing Jarvis 1989 to De Jong van Lier et al. [2008] model led to an unrealistic α function, and its behaviour does not properly represent the α concept. The threshold type functions like the other ones evaluated in this paper seems to be more feasible.

The fact that JMII is more sensitive to both R and M when compared to the other M–based models is eventually attributed to the α function and related equations derived to express their parameters (eq. 19 and 20). It can be seen from Fig. 1(c) that β defined by eq. 18 (β of JMII) tends to be higher when R increases and tends to be lower when R decreases compared to β of JMf and JMm. Thereby, for the first days of simulations when the soil hydraulic conditions tend to be rather uniform over depth, JMII overestimates RWU compared to VLM predictions. This becomes more important for the high R–low T_p scenarios. For such conditions, the RWU over depth predicted by the VLM tends to be more uniform, which is reasonable since the low transpiration demand can be met by any small root density that can be found in deep soil depths. After some period of time, the discrepancies between VLM and JMII tend to increase, since the higher uptake in the upper layers reduces h and because of α shape of JMII

RWU in the upper layers are suddenly reduced towards zero. These are the main reasons why JMII does not predict well in high R–low T_p scenarios. (Part of) this discussion may be included in a future version.

Fig 1 Caption: (a) α of JMII model (eq. 18) as function of soil pressure head h, (b) ω_c parameter (eq. 20) for different soil types, expressed by M_{max} and (c) the normalized root length density β computed by the eqs. 4 (JMf) and 19 (JMII) as function of root length density R considering R over depth given by eq 28 with R_{avg} and b equal to 1.0 cm cm^{-3} and 2, respectively.

14. Page 15, line 25: it could also be noted (perhaps by referring to equation 20) that $\omega_c > 1$ is not physically unrealistic.

R.: Yes, $\omega_c > 1$ is not physically unrealistic and it is implicitly stated in line 25, from which can be inferred that if we set $\omega_c > 1$, both JMII and JMm can predicted $T_a/T_p < 1$ for the low R-high T_p scenarios as VLM did.

15. Page 16, line 3: This is misleading. The Feddes function for α is not part of the Jarvis (1989) model.

R.: As it was commented in point 6, it will be referred to as JMf.

16. Page 17, lines 1-2: it is confusing that different symbols are apparently used for one of the parameters in the Li-type models. In equation 25, λ is used, whereas in the text here and in table 5, l is used, while in table 4 l_m is used. I believe they are all the same parameter?

If I understood it correctly, I don't see how you can write that the optimal values of λ follow a logical relation to R and T_p (line 1). In many cases, and especially for low root densities, values of l (i.e. λ?) in table 5 are larger than 1, which implies to me that compensation is working incorrectly in these scenarios (it is decreasing uptake in the more sparsely rooted layers). Also, in table 4, it is stated that l_m (i.e. λ) was constrained to take values less than or equal to 1. If I understood it correctly, the results in table 5 suggest that this was not actually the case in practice.

R.: All symbols refer to the same parameter and we will correct this by changing them to l_m. Regarding the l_m parameter limit constraining values, the upper values for l_m were constrained to 3. Conceptually, there is no inconsistency in taking $l_m > 1$. Indeed, $l_m = 1$ means no compensation at all and $l_m < 1$ implies compensation. Values of $l_m > 1$ simply indicates that the upper soil layers are more important for RWU distribution. The limits values for l_m will be corrected as well as such a comment will be added explaining these features at a suitable location in the manuscript.

17. Section 4.2, table 6: can you give the total precipitation and potential transpiration here? It's good to get a rough idea of how much stress occurred in these simulations.

R.: We will try to insert this information into the table or making a plot of cummulative transpiration over time

18. Page 18, lines 10-12: you did not test the Jarvis (1989) model (see earlier comments).

R.: Yes, it will be corrected

19. Page 18, line 12: I did not get a good understanding of why the JMII model does not work so well for high R–low Tp scenarios (i.e. high compensation). I would have thought that, in principle, it should work OK. Please briefly explain what you think the reasons are for this.

R.: This is explained in point 13 and will be added at the end of section 4.1.2.

20. Lines 16-18: I think this is too optimistic, as this test was not a very tough one. You had the same plants (identical roots) and the same three soils. How would it look if you had simulated different scenarios (soils, plants)? I think you would need to re-calibrate the empirical models. How useful is that?

R.: Yes, the results would be different. However, it is useful to show that the methodology used to calibrate the models is robust and can be used to assess empirical models and sensitivity of the empirical parameters in order to provide a full calibration of the empirical models in a next step.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-59, 2016.

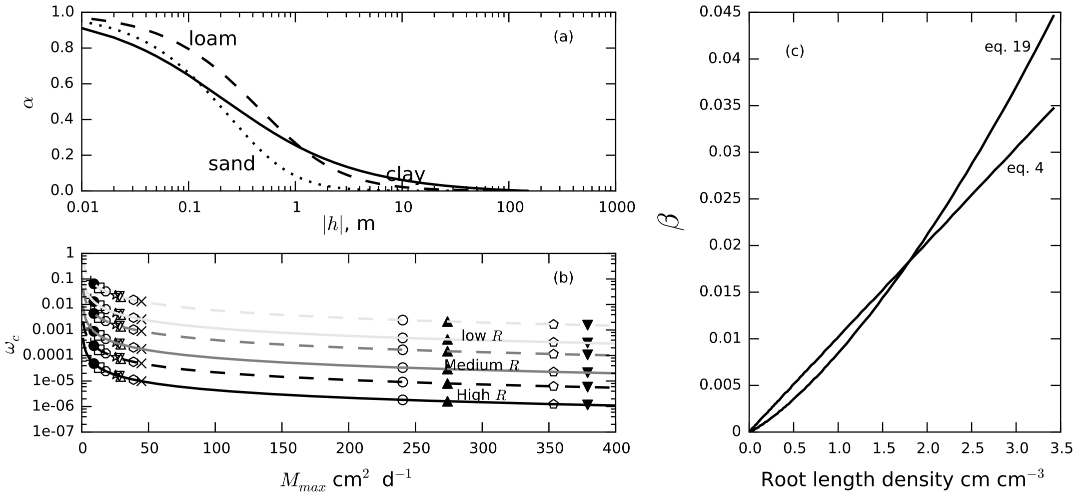

Fig. 1. Caption in the text