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

Anonymous Referee #3

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The manuscript presents (i) an evaluation of the performance of some well-known conceptual models for root water uptake (RWU) and some modified versions of them and (ii) the determination of their paramters. As reference they use simulations with the more physical model of de Jonge van Lier et al. (2013).

RWU for a simple drying out scenario and a complete season with daily data for precipitation and potential evapotranspiration is simulated with the reference model. The drying out scenario is used to fit the conceptual models to the data of the reference model. With the fitted parameters the complete season is simulated with the conceptual models. RWU patterns and temporal course of transpiration of the conceptual models is then compared with the RWU patterns and temporal course of transpiration of the reference model.

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Discussion paper

The authors show that most of the conceptual models cannot reproduce the simulations with the reference model but that the modifications greatly improve the performance.

The paper is partly well written and structured. However, to my point of view, especially the results and discussions as well as the conclusions need improvements.

Generally, the topic of evaluating conceptual RWU models by comparison with more physically based models is important, fits well in the scope of HESS and needs consideration. However, as will be outlined below, to my point of view, there are several points, which should be clarified before.

Major comments

The manuscript compares some well-known conceptual models with the reference model, suggests for each of them (except JMII) a modified version and suggests two new models. This is very ambitious. The authors should consider to use less models and go therefore deeper into the discussion, which seem to me rather sketchy.

RWU models must be able to simulate transpiration and local uptake well. However, the conceptual models are only fitted to local uptake data (Eq. 29) and not to temporal course of transpiration. To me this seems to be problematic since (i) for most applications of simple RWU models a sound prediction of transpiration is more important than uptake distribution, (ii) in reality transpiration is much easier to quantify as local uptake so that fitting RWU models to real data (which is the ultimate model test) will probably use transpiration rates for fitting and (iii) even if local uptake at different depth is fitted badly, transpiration can be met well if too low uptake in one depth is compensated by too high uptake at another depth. Therefore, I suggest to fit the models to local uptake and transpiration simultaneously. I know that this will make the weighing scheme more problematic but a solution could be to fit relative transpiration and relative uptake and use weights in a way that transpiration and uptake are equally weighted, e.g. w_t = n*w_u, where w_t and w_u are the weights for the transpiration and uptake data and n is the number of depths for which uptake is fitted.

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Discussion paper

The comparison of the established conceptual models with the proposed modifications seems a bit “unfair” to me since local stress reduction in the modified versions and in the reference model are based on matric flux potential, thus the modifications are closer to the reference model than the other models. Note, that the reference model is still a model and not a representation of reality. This should at least be discussed.

One of the critical points concerning the Feddes stress response function in combination with the Jarivs (1989) compensation approach, the authors mention, is that the models fail to predict compensation under wet conditions, where alpha is 1 for different matric potentials. The modification using martic flux potential with distinct critical point (M_c) will perform alike. This is ok but should be discussed.

Model PM mixes stress reduction described by pressure head and compensation calculation based on matric flux potential. I am not sure whether this is a conceptual reasonable model. Please reconsider using this model.

I wonder why model fitting was only performed for the drying out scenario with constant boundary conditions. Under such simple conditions, the information content of the “measurements” might be too low to find parameters for conceptual models, which shall then be used to simulate (extrapolate) dynamics under variable boundary conditions. I would suggest to use the first half of the time with variable boundary conditions for model calibration and the second part for model performance test. This is the usual way for model test in hydrology.

It should be discussed that other physical models do exist, where local RWU is based on energy status instead of matric flux potential, see e.g. Doussan et al. (2006), Javaux et al. (2008) and the simplified model of Couvreur et al. (2012). In this context it can also be discussed that in the de Jonge van Lier model no other parts of the energy density, like gravimetric potential or osmotic potential, can be accounted for. Additionally, although the Feddes stress response function seems to be “out of fashion”, it does account for oxygen deficit, which is important at least for the fine textured soils in the growing season, whereas the matrix flux potential based stress function cannot account for that. These limitations of the reference model should be discussed in the introduction section. Since none of the models (neither the reference model nor the calibrated models) account for oxygen stress, I can imagine that RWU in the clay under variable boundary conditions is not well described by any of the models. This could also be briefly discussed.

To my point of view, a physical model for RWU, which accounts also for the magnitude of potential transpiration (Tp), should be solved with boundary conditions accounting for the daily course of Tp (as done by e.g. Couvreur et al. (2012)).

The title is misleading. I would suggest to use a title, which shows that the paper deals with a performance test of different simple empirical models for RWU using a more complex physical model accounting explicitly for water flux in the soil-plant-atmosphere continuum.

Minor comments

Page 1:

Lines 7 to 8: “The simulated scenarios give more insight into the behaviour of the physical model, especially under wet soil conditions and high potential transpiration rate.” This statement seems not to be important for the abstract and can be omitted.

Lines 10 to 11: “. . .for the scenarios of low RWU “compensation”. Better: “. . .for the scenarios for which RWU “compensation” is expected to be low.” or “. . .for the scenarios for which the physical model predicts low RWU “compensation.”

Lines 13ff: When the Jarvis model is criticized it should be stated that the modifications are conceptually closer to the reference model.

Lines 13 to 14: “Incorporating a newly proposed reduction in the Jarvis model. . .” Consider: “Incorporating a newly proposed reduction function in the Jarvis model. . .”

I did not find a statement about the performance of the Jarvis (2010) model in the abstract.

Page 2:

Lines 17 to 18: Models that do not account for compensation are under some circumstances (not all) less accurate, e.g. for coarse to medium textured soils and high root length density.

Page 5

Line 24: “non-homogeneous” consider “heterogeneous”. “For non-homogeneous conditions, RWU for lower R can be the same for higher R depending on the stress level. . .” Consider: For heterogeneous conditions, RWU for lower R can be the same as for higher R depending on the stress level. . .” Maybe I am mistaken but I do not see this in Fig. 2: For a certain leaf pressure head (for example -110 m), the RWU for R=0.01 is always lower than for R=0.1 and RWU for R=0.1 is always lower than for R=1.

Page 7

Line 3: Consider another word than obscure. Compensation will certainly (and shall) enhance uptake (by the factor alpha_2) in some depth compared to the value given by alpha. To me the specific problematic issue is that in case of homogeneous alpha smaller than 1 and omega_c smaller than 1, these models lead to uptake greater than given by the homogeneous value of alpha or, more generally, that relative transpiration can be higher than given by the highest value for alpha in case of heterogeneous alpha distribution with depth (see e.g. Skaggs et al., 2006, Simunek and Hopmans, 2009, Peters, 2016).

Lines 3 to 5: If I understand it right, this holds only for the combination of the Jarvis model with the Feddes stress function for which alpha is 1 for different pressure heads (i.e. between h_2 and h_3).

Line 14: Consider “conceptually” instead of “numerically”

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Discussion paper

Page 8

Lines 14-15: I cannot follow: rho and M as defined here do not occur in the Jarvis (1989) model.

Page 10

Lines 2 to 14: Consider using subsection header such as “3.1 Applied models”

Lines 19 to 20: A free drainage boundary condition is usually used for the case with very deep groundwater level so that groundwater cannot influence the soil. Then the assumption is that at a reasonably deep layer below the root zone the hydraulic gradients are close to unity. This is certainly not the case at the bottom of the root zone. I would suggest to set this boundary condition at a depth of at least 1 m or 1.5 m.

Line 24: “Soil date. . .” should be “Soil data. . .”

Line 26: “These soils are identified in this text as clay, loam and sand (Table 3).” Consider “These soils are identified in this text as clay, loam and sand.”

Page 11

Line 12ff: Please specify in this section at which depths and which time interval the data for S and S* were taken and used to minimize Phi. Consider to fit also transpiration rates and use a weighted least squares scheme instead.

Line 15: “. . .the objective function to be optimized. . .” Consider “. . .the objective function to be minimized. . .”

Line 25: For a nonlinear problem with a model error, i.e. with models that do not fit the data well, there might be several local minima. Did all fitting runs lead to the same minimum? If not I would try to use more starting points to be sure or even a global minimization scheme.

Page 12

Lines 1 to 2: “This guaranteed that RWU predictions from SWAP corresponded to the best fit of each empirical models to the De Jong van Lier et al. (2013) model.” I do not understand this sentence and how it refers to the statement that parameter fitting was only applied for the drying out scenario.

Lines 19 to 20: “Initial pressure heads were obtained by iteratively running SWAP starting with the final pressure heads of the previous simulation until convergence.” I do not understand. What converged to which values? And why was the initial condition optimized?

Page 13

Line 3: “The patterns for the sand and loam soil (not shown here) show very similar features.“ This is not immediately clear to me since matrix flux potential (M) for the sand is very different from M of clay. In a sand most of the water is available under very low energy densities and thus I would expect that for sand, transpiration is prolonged much longer at potential rates and the drop of T_a to be much steeper after onset of transpiration reduction. Could you discuss this briefly in 2 or 3 sentences?

Line 14: “. . .increases the reduction of. . .” consider "**...** leads to faster reduction of**...**"

Line 15: “. . .assumes a parsimonious relationship. . .“ do you mean “. . . assumes a direct relationship...”

Page 14

Line 23ff, Tab. 5 and Fig. 6: For Sand with Tp=1mm/d and R=1cm/cmˆ3 using the JM: omega_c=1, h_3=0 means that transpiration must be reduced from the beginning, since h >0 from the beginning and compensation cannot take place. I cannot see this in Fig. 6, where transpiration is equal to Tp for a prolonged time: Is it due to a very small reduction of alpha_f, so that T_a is smaller than but still close to T_p? Please discuss briefly.

The discussion of Line 23ff makes it clear to me that fitting not only the uptake pattern but also actual transpiration (see major comments) would increase model performance of the conceptual models. Then compensation would be most likely predicted.

Page 15:

Lines 5 to 6: h_s cannot be lower than h_4 if only transpiration but no evaporation is considered.

Lines 16 to 20 and general: “performs better”, “overestimates RWU”, . . . Please discuss the performance of the conceptual models always with respect to the VLM since you compare models. A comparison with real data is still the best benchmark.

Lines 21ff: Here fitted models are compared by statistical measures like E and rˆ2. Since the fitted models use different numbers of adjustable parameters such a comparison is not justified: More free parameters mean more flexibility and thus a better “chance” to fit the data. Please consider using other measures, which account for number of fitted parameters, like AIC (Aikaike, 1974).

Line 25: “. . .models (except for JM and JMm by setting omega_c > 1) are. . .” This can be omitted since omega_c > 1 makes conceptually no sense.

Page 16

Lines 16 to 17: “The optimal h3 and Mc values (Table 5) for FM and FMm, respectively, increase as R or Tp increases, contradicting their conceptual relation to R and Tp levels” I see the contradiction only with respect to increased R but not to increased Tp.

Lines 31ff: I assume that parameters h_3 and omega_c for JM are highly correlated. Can you give information about parameter correlation? Moreover, such parameter correlation might be due to model structure but also due to data used for fitting the model. Therefore, I repeat my suggestion to use not only the drying out scenario for model calibration but the scenario with changing boundary conditions. This might reduce correlations.

Page 17

Lines 1 to 2: What are l-values? L_m and lambda respectively. Please unify.

Line 4ff: A figure with the cumulative transpiration over time would be interesting to see if there are under-/over-estimations for specific time intervals in the complete season.

Line 23: The statement that JMII is poor in performance should be discussed with more caution since it was not adjusted to the reference model. Thus, this finding can be expected. The same holds to a less extend to the models for which only one parameter was adjusted.

Line 24: This is a very daring conclusion, since the reference model and the proposed models have partly a similar structure (see above).

Conclusions section: I could not find a single conclusion. This is rather a summary and not a conclusion.

Line 32: "...especially under wet soil conditions and high potential transpiration." Why do the simulations yield insight especially under wet soil conditions?

Page 19

Lines 21 to 22: This paper is certainly not in press

Tables and Figures Table 3: Although the Mualem/van Genuchten model is well known the equations should be stated in the text to make it easier to assign the parameters. What, for example, is lambda? I guess the so-called tortuosity parameter in Mualem's model, but i am not sure. Alternatively, Tab. 3 can be completely omited and the functional relationships of theta(h) and K(h) might be plotted in an extra figure.

Table 4: I cannot find l_m for PM and PMm in the text. Do you mean lambda instead of l_m?

Table 5: In the text root length density is R here it is Rd.

Table 6: For comparison: what was the value for potential transpiration

Fig. 1: a) since h_1 and h_2 are set to zero in all simulations, Fig. 1,a should account for that and start with alpha=1 at h=0. b) since M_c for Tp=1 mm/d is different from M_c for Tp = 5 mm/d, this should be indicated in Fig. 1b using M_c,l and M_c,h , similarily to h_3,l and h_3,h in Fig 1,a.

Fig. 3: Should only contain the three root distributions used in this study.

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