I think that the authors have greatly improved their paper. It must be noted though that the paper is very technical. This implies that the methods and the definition of all the parameters need to be very clearly explained so that the reader can understand the paper. To my opinion, there is still some work to improve the clarity of the paper.

Furthermore, I think that the conclusion and discussion section of the paper can be strengthened further by adding some discussion about the dependence of root water uptake parameters on soil properties and climatic conditions. This is an important issue since root water uptake parameters are normally linked to the vegetation type and tables are provided that provide root water uptake parameters for a certain vegetation type. This paper actually shows that the root water uptake parameters of the empirical models also depend on the soil type and the climate. The question therefore arises whether the empirical models can be really considered to be parsimonious compared to the full physically based model. I think that this deserves some discussion in the final part of the paper.

General comments

Abstract: If you briefly mention what is behind the 'alternative empirical models' that are proposed, I think that the abstract could be improved. How do they differ from the Feddes and Jarvis models?

P6 ln 2-3: Maybe before going to De Jong van Lier et al. (2013) where the reader can find information about the algorithm, I think it would be helpful to explain how an equation with three unknowns Ta, hl and h0 can be solved in general. First, I think you need to write that in equation 8, there are only two unknowns: either h0 and hl or h0 and Ta. In order to solve the equation for the two unknowns, additional equations are required. After formulating equations for h0's at different depths, Eq 1, making use of Eq. 6, can be used to solve for Ta (or hl) and the distribution of h0 with depth in the soil profile.

P6 Figure 2: I am afraid that I still do not understand figure 2 and its caption. What I suppose that is shown in Figure 2 is the sink term for the case that the root length density and the soil water potential are uniform in the root zone (i.e. they do not change with depth in the root zone). As a consequence, also the sink term is uniform in the root zone and the transpiration rate is simply the sink term multiplied by the root zone thickness. So I do not understand that the plant transpiration was set to 1 mm d^{-1}.

P7 and P8: Comparison of the Jarvis model and the De Jong van Lier model and Figure 5. In my previous comments on the paper, I posed the question whether the analogy between the two models relies on the assumption that when stress occurs the water potential the root surface is every the same in the root zone. I also asked whether the model of De Jong van Lier only predicts stress when the water potential at the root surface is everywhere equal to the wilting point. If the De Jong van Lier model can predict that stress may also occur even when in parts of the root zone the surface water potential at the root surface is still above the wilting point, then the De Jong van Lier and Jarvis models may also deviate under stress conditions. I think that the main problem in the description here is that the authors are not fully consistent in defining the stress conditions: Eq. 14 is not the same as Eq. 10. In Eq. 14, it is assumed that the main loss of pressure head between the bulk soil and the leaves is in the soil when stress occurs. Under this assumption, it can be stated that the pressure head at the soil root interface is everywhere in the root zone equal to the wilting point. However, when pressure head losses in the root system become important, the pressure head at the soil root interface can be in some parts of the root system well above the wilting point. To check this, another Tpmax can be defined which is the maximal uptake when the leaf water potential is equal to the critical leaf water potential, hwl and the water potentials at the soil root interface, h0, are everywhere in the root system equal to the bulk soil water potential hs. When this Tpmax is smaller than the Tpmax that is obtained assuming that the water potentials at the soil root interface are everywhere equal to the wilting point, then pressure head losses in the root system are more important.

In Eq. 14, there are no plant conductivities. Therefore, Tpmax as defined in Eq. 14 must be different from Tpmax in Figure 5. In figure 5, the effect of Kroot and Ll on Tpmax is evaluated. But it is not clear to me what exactly the boundary conditions were to calculate Tpmax. I think the authors should explain how they defined Tpmax and try to use a consistent definition.

P10 section 2.2.5: I made the comment that besides the pressure head, also the potential transpiration rate plays a role in the definition of the stress function. Instead of making an extra section about it, I would suggest to include how the different model concepts deal with the dependence of the stress function on the potential transpiration. I would propose to include in Eq. 3 also Tp in the stress function: α(h(z),Tp). In the physical model, the maximal uptake rate is calculated and this maximal uptake rate depends on the soil water potential. That means that if a stress function would be defined for the physical model as Ta/Tp(h), it would also depend on Tp since for a lower Tp, the pressure head at which the maximal uptake is equal to Tp is lower. The authors already mention for the Jarvis model, that the dependence of the stress function on Tp should be different from the dependence that is derived for the Feddes model. It is interesting that in the comparison between the Jarvis model and the physical model, they determine a stress function (Eq. 18) that is independent of the potential transpiration rate (neither M nor Mmax depend on Tp).

P 18 Eq. 32: The authors included now the Aikake's information criterion. I think this is a very suitable parameter but in this context, I am wondering whether it would not be better to investigate the performance of a model that uses the same vegetation parameters for different soils and transpiration conditions could be used. The problem now is that for the same properties of the vegetation (root density and root hydraulic conductivities) different 'root water uptake parameters' must be used depending on the soil and potential ET conditions. Therefore, although the empirical models do not have that many parameters, they need to be adjusted for different soil conditions and climate (potential transpiration). My question is therefore whether the de Jong van Lier model requires that many more parameters than the empirical models. Root length density can be measured and the root conductivities could be fitted as well. In the end, this might result in less parameters that need to be determined when the model is to be used in different soils and for different transpiration rates. This problem may be even more relevant when considering that soils often have layers with different hydraulic properties. I do not suggest that the authors refit now the models to the different cases they considered using only one parameter set for the different cases (soils and potential transpiration) but I would propose including this in the conclusion and discussion section.

P20: The authors concluded that one drying experiment would be sufficient to parameterize the model and use it to run root water uptake during a growing season. I am just wondering whether one drying experiment would be enough. How can the dependence of the stress parameters on the transpiration rate be defined then? Maybe, this dependence is not so important for simulations over an entire growing season as long as a drying period with a relevant transpiration rate for the entire growing season is chosen. But this could maybe be taken up in the discussion section. Furthermore, the authors used daily averaged transpiration rates and made the stress function dependent on the daily transpiration rates. But this means that in the model, the transpiration rates should not be resolved within one day since otherwise, different stress functions will have to be used that consider the peak transpiration rates during midday (which are about a factor 8 higher than the daily average transpiration rate).

P21: The authors concluded that for cases with low root water uptake compensation, which correspond with cases of low root length density, the Feddes models perform pretty well. Can this be explained by the fact that for low root density, the resistance to water water flow from the soil to the leaves is mainly dominated by the resistance to flow in the soil? When the soil dries out at one depth, the resistance to the flow at another depth and therefore flow will not change since this resistance is dominated by the soil conditions and is hardly influenced by changing conditions in the roots.

Detailed comments:

P1 ln 13 ‚all models that accounts‘ → that account

P1 ln 14-15: From reading only the abstract, the reader will not understand this sentence since it is not yet clear what the JMII model is.

P3 ln 29: Dimension of Ta should be L T^{-1}.

P17: Definition of coefficient of determination and model efficiency. In fact, the model efficiency is the same as the formal definition of the coefficient of determination. However, the squared correlation coefficient is sometimes called the coefficient of determination (which is very confusing of course when coefficient of determination mostly refers to a metric that is equal to model efficiency).

P17: omega_c > 1. Isn't the upper boundary of omega_c equal to 1?