This paper compares different root water uptake models. In a theoretical part, the relations between the different models, their parameters and differences between the models are derived. Simulations for a dry out scenario and for a growing season in different soils, for different transpiration rates and for different root densities were carried out. The benchmark model that was used was the de Jong van Lier 2013 model, which describes root water uptake in a mechanistic manner. The root water uptake distributions that were simulated with this model were subsequently used to parameterize the empirical models.

Despite the fact that root water uptake is one of the key processes in land surface models and crop models, it is one of the processes about which there is still a lot of uncertainty on how it should be represented in models. This paper makes an important contribution to this problem by making a detailed comparison between different approaches and by presenting new empirical models that could be used to represent root water uptake. Therefore, I think that this paper will make an important contribution to root water uptake modelling.

In general, the paper is well written and is technically sound. Yet, given that it is a very technical paper, I think that it can still be improved at some points so that it becomes clearer. Many of my detailed comments are questions for clarification. One point that needs to be clarified is the dependence of the model parameters on the transpiration rate. The authors address this topic in the discussion of the results. But, also in the theoretical part, I think that this point should be addressed.

The authors introduced a new empirical model for root water uptake. I would propose to include also some arguments why such an empirical model would be needed or beneficial as compared to the mechanistic model. Is this an issue of computational time?

In the conclusions section, I think that a general discussion on the parameters of the empirical model could be included. The problem with empirical models, which is not addressed, is that the 'root water uptake parameters' also seem to depend on the soil properties and the climatic conditions. The authors addressed the dependence of h3 on the transpiration rate and found that it was opposite to what is expected. I would suggest to address also the variation of the other parameters a bit.

Since the parameters of the empirical models depend on soil properties and boundary conditions, it means that these parameters have to be estimated for each specific case. In the paper, the authors parameterized the models based on simulated root water uptake distributions. The problem is that it is not possible to retrieve root water uptake distributions directly from measurements since there is also considerable water redistribution in the soil. I would propose to include a strategy how to deal with this problem.

Detailed comments.

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

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 α(h) function depends on the transpiration rate as shown in Figure 1. Figure 1 suggests that the new stress function α(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.

P5: ln 15: 'Because Ta and hl 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 Ta=Tp is known as a boundary condition so that hl can be calculated or hl=hw is known and Ta is calculated. I think that the reason why the hl (or Ta) cannot be derived directly is because the set of equations that needs to be solved (including also all ho,i 's) is non-linear in ho,i.

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.

P7 ln 21: 'where Tpmax is the maximum possible transpiration rate attained when M0 = 0'. This assumes that the minimal water potential at the soil-root interface is hw (wilting point). But, doesn't this minimal water potential depend also on the critical leaf water potential hl?

P7 Eq. 17: Why is M0 constant with z? The soil root interface water potential can depend on the depth, can't it?

P 8 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 ρ 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 M0 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 M0(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.

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'.

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 $\alpha(z)^2$?

P9 ln 16: Same comment as above.

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.

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.

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.

P 12: ‘3.2.1 Growing season simulation’ This is not a sub section of the optimization section

P13 ln 8: ‘hw(= -200 m)’. I am confused here because at p 10 it is written: ‘The value of the parameter h4 was set to -150 m.’

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

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