Next we respond to the minor comments.

1)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.

We agree and it will be omitted

2) Page 1, 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."

OK, we will consider this

4) Page 1, Lines 13ff: When the Jarvis model is criticized it should be stated that the modifications are conceptually closer to the reference model.

Agreed, this will be discussed.

5) Page 1, 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.

Ok. We will add a statement about the performance of JMII

6) 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.

Agreed, we will correct this sentence.

7) 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.

We will consider your suggestions. Indeed, it does not always happen and depends on h_l

value. It will be corrected.

8) Page 7, Line 3: Consider another word than obscure. Compensation will certainly (and shall) enhance uptake (by the factor α_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 ω_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).

This part will be rewritten as also suggested by N. Jarvis in RC1 comment.

9) Page 7, 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).

In that case we agree with Jarvis's comment (see point 7 of RC1 comment) that this sentence is wrong since Jarvis [1989] can predict compensation by $1/\omega_c$ under wet conditions

10) Page 7, Line 14: Consider "conceptually" instead of "numerically"

OK

11) Page 8, Lines 14-15: I cannot follow: ρ and M as defined here do not occur in the Jarvis (1989) model.

This is the result of comparing Jarvis [1989] model to the De Jong van Lier et al. [2008] model. The models can be correlated for stressed conditions if α and β are given by eq. 18 and 19, respectively. For stressed conditions, substituting these eqs into the Jarvis [1989] model gives the same eq. for S for both models. For unstressed condition, however, a different equation is found, eq. 21.

12) Page 10, Lines 2 to 14: Consider using subsection header such as "3.1 Applied models"

OK

13) Page 10, 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.

This is an important point and requires a careful justification. We used free outflow close to the bottom of the root zone. Many studies of water flow in soils without roots use the unit hydraulic gradient in the entire profile as a reasonable hypothesis. Extracting roots of course change this scenario dramatically, but simulated root length densities were already very low in the bottom part of the rooted zone. Nevertheless, changing the depth of free drainage will alter the water regime, especially in the lower part of the soil profile. That may, on its turn, change the simulated uptake pattern. Other important scenario changes might also be studied, like more or different soils with distinct soil hydraulic properties. Any alteration of this kind implies in a whole new set of scenarios and simulations and a considerable job in analyzing them and possibly lead to some new discussion or insight, but will it be a crucial factor in the comparison between the RWU models? We think it will not change the conclusions and would rather like to decline from this suggestion. We may, however, include a comment/thought about the issue in the discussion part.

14) Page 10, Line 24: "Soil date..." should be "Soil data.."

OK

15) Page 10, 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."

OK

16) 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 Φ. Consider to fit also transpiration rates and use a weighted least squares scheme instead.

OK, this will be specified. We already addressed this point in point ii of major comments.

17) Page 11, Line 15: "...the objective function to be optimized..." Consider "...the objective function to be minimized..."

OK

18) Page 11, 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.

For most cases the same minimum was found whatever was the starting point. In some cases we found different values and then used the "the lowest minimum".

19) 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.

This sentence was meant to emphasize that the optimizations were performed only in the drying-out scenarios and by optimizing the parameters the best fit to De Jong van Lier et al. [2013] model was reached.

20) Page 12, 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?

SWAP was run until the initial soil pressure head set values were equal to the end pressure head set values.

21) 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?

The RWU predictions for sand soil are very close from what you inferred. We will make a short discussion to make this clear.

22) Page 13, Line 14: "... increases the reduction of. . ." consider "... leads to faster reduction of..."

OK

23) Page 13, Line 15: " assumes a parsimonious relationship..." do you mean "assumes a direct relationship..."

We tried to say a simple relationship when compared to other empirical relationships, ex. Fisher et al. [1981]

24) Page 14, Line 23ff, Tab. 5 and Fig. 6: For Sand with $Tp = 1mm/d$ and $R = 1cm/cm^3$ 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 T_p for a prolonged time: Is it due to a very small reduction of α_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.

Indeed, this is due to the small reduction of α. In fact, fitting the models to transpiration will improve these two models performance regarding transpiration, but reducing RWU predictions. It might not be worthy forcing these models to mimic transpiration, whereas other models can mimic well both variables by fitting to only RWU. We can fit the models to transpiration in some scenarios to show this.

25) Page 15, Lines 5 to 6: h_s cannot be lower than h_4 if only transpiration but no evaporation is considered.

Agreed. We will correct this. In fact, h_s becomes close or equal to h_4.

26) Page 15, 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.

We will add this.

27) Page 15, Lines 21ff: Here fitted models are compared by statistical measures like E and r^2. 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).

We will consider this as also discussed in the reply of RC1 comment.

28) Page 15, 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.

We think it does make sense. See point 14 of RC1 comment.

29) Page 16, Lines 16 to 17: "The optimal h_3 and M_c values (Table 5) for FM and FMm, respectively, increase as R or Tp increases, contradicting their conceptual relation to R and T_p levels" I see the contradiction only with respect to increased R but not to increased T_p.

Yes, we will correct this sentence.

30) Page 16, Lines 31ff: I assume that parameters h_3 and ω_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.

We will analyse this correlation and discuss it.

31) Page 17, Lines 1 to 2: What are l-values? L_m and lambda respectively. Please unify.

They all will be referred as to l_m.

32) Page 17, 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.

We will add transpiration information.

33) Page 17, 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.

We will analyse this more carefully. The Akaike information criteria measure, to be include, will help in this analysis.

34) Page 17, Line 24: This is a very daring conclusion, since the reference model and the proposed models have partly a similar structure (see above).

As commented above, we will discuss this more.

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

If required, we can alter the writing stIf required, we can alter the writing style of this section.

36) Page 17, Line 32: ". . .especially under wet soil conditions and high potential transpiration." Why do the simulations yield insight especially under wet soil conditions?

For high T_p and low R under wet conditions it was shown that T_p can not be achieve and also how plant hydraulic parameters relate to this.

37) Page 19, Lines 21 to 22: This paper is certainly not in press.

It will be corrected

38) 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.

We will follow your suggestion

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

Yes, it will be corrected.

40) Table 5: In the text root length density is R here it is R_d.

It will be corrected.

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

42) 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 T_p=1 mm/d is different from M_c for $T_p = 5$ mm/d, this should be indicated in Fig. 1b using $M_{c,l}$ and $M_{c,h}$, similarly to $h_{3,l}$ and $h_{3,h}$ in Fig 1,a.

We will improve this accordingly.

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

Because we chose to set $b = 2$, it would be good to graphically see how b affects the curve.

References

Q De Jong van Lier, J C Van Dam, K. Metselaar, R. De Jong, and W H M Duijnisveld. Macroscopic root water uptake distribution using a matric flux potential approach. *Vadose Zone Journal*, 7(3):1065–1078, 2008.

Q De Jong van Lier, J C van Dam, A Durigon, M. A. Santos, and K Metselaar. Modeling water potentials and flows in the soil-plant system comparing hydraulic resistances and transpiration reduction functions. *Vadose Zone Journal*, 12(3), 2013.

M J Fisher, D A Charles-Edwards, and M M Ludlow. An analysis of the effects of repeated short-term soil water deficits on stomatal conductance to carbon dioxide and leaf photosynthesis by the legume macroptilium atropurpureum cv. siratro. *Functional Plant Biology*, 8(3):347–357, 1981.

N J Jarvis. A simple empirical model of root water uptake. *Journal of Hydrology*, 107(1): 57–72, 1989.