Authors' response to interactive comment of the anonymous Referee #2

Black text: Referee comment

Blue text: Authors' response

We thank the reviewer for the valuable comments and suggestions to improve our contribution. We provide point-by-point reply below.

General Comments

This study compares two crop and root growth models with measured data that were previously published. The models differ mainly in their representation of root water uptake. One uses the standard approach (Feddes), the other considers the flow process in the roots (but not to the roots) and is hence more mechanistic. The paper is fairly well written. It is a valuable contribution to soil/crop science, but would benefit from a few extensions and corrections, as outlined below.

Thank you for your comments. The measured data has not published before. However, the soil parameters and RWU parameters from two RWU models were published in the previous studies (with the same experimental set-up but in 2014). The published the soil parameters and RWU parameters from two RWU models were used in this study.

It is a severe shortcoming of the measurements that the field plots do not involve replications. Replicates would be very desirable to enable assessing the variability, but their omission should not prevent the manuscript from being published. It should be frankly stated that and why there were no replicates (too expensive?). The experiment should be described better (e.g., the plot size is not given).

Yes. We agreed that the field plots did not involve replications.

The construction and experimental designs were described in detail in Cai et al., (2016); Cai et al., (2017) and Cai et al., (2018). The authors also referred the readers for the detail explanation of field trial these papers. As suggested from the referee, we added three sentences in the section 2.1 (Location and experimental set-up) between line 93 and 94:

"Each treatment was 3.25 m wide and 7 m long. The treatments bordered each other along 7-m-long side. There were no replicates for plots due to the complex and expensive construction of underground minirhizotrone facilities".

In model-measurement comparisons, it is good practice to present the measured data with standard deviations or errors and the modeled data as lines; if uncertainty is considered, with uncertainty bands. This is not always the case here (Figure 3 no, figure 4 yes; why?)

Figure 4 showed the transpiration by two models versus the measured sap flow. The measured sap flow was achieved with 5 sensors in each plot. Thus, the variability of transpiration from different stems could be shown via error bars. The Figure 3 showed the simulated root length density versus the observed root length in 06 soil depths in different treatments. For one observation depth and one treatment, roots were counted in 120 images of 13.5 mm x 18 mm. This dataset represents a sample of the population of all possible root counts at this depth. Cai et al., (2016) estimated and analyzed the standard deviation (error) and spatial correlation of root counts along the minirhizotube. The standard deviation was small and no spatial correlation of root densities in the horizontal direction was observed for the investigated **winter wheat** crop. That is a reason that Figure 3 we did not show the standard deviation for the root measurements.

The paper should give a few more details about the calibration of the soil-crop model. The role of Penman's ETP should be discussed.

The calibration was mentioned in line 283-284. Following the suggestion from the referee, the calibration sentences was revised and is added more details.

"Before comparing these modelling approaches, we calibrated the original LINTULCC model using the data from the rainfed plots in the silty soil (F2P2). The model is firstly calibrated to make sure the model properly described the phenology. Two parameters (minimum thermal sum from sowing to anthesis and thermal sum from anthesis to maturity (°C d)) were used for phenology calibration based on information of sowing, anthesis, and maturity dates. The model was then calibrated using time series of LAI, biomass, and gross assimilation rate through the change of maximum carboxylation rate at 25 °C (VCMAX25), critical leaf area index (LAICR), and relative growth rate of leaf area during exponential growth (RGRL) parameters."

Regarding the potential evapotranspiration (ETP), our study used the Penman-Monteith equation from Allen et al., (1998) (See Equation 3, Chapter 2 in the FAO Irrigation and Drainage Paper No. 56, Allen et al., 1998). The ETP was calculated with crop canopy resistance under optimal water condition (fwat = 1) (see the Appendix B). The hourly net radiation, soil heat flux, and aerodynamic resistance were estimated based on Allen et al., (1998) (see also the replies for comments at line 559, 561, and 565). Following suggestion from the referee, some sentences were added after line 404 to describe the roles of ETP.

"The method that we used for modelling the canopy resistance used in the Penman-Monteith has been reported for both short and tall crops (Dickinson et al., 1991; Kelliher et al., 1995; Irmak & Mutiibwa, 2010; Perez et al., 2006; Katerji et al., 2011; Srivastava et al., 2018). The fair agreement of RWU to sap flow in our study indicates the proper estimate of ETP based on the crop canopy resistance (with fwat = 1) in winter wheat. The direct calculation of crop canopy resistance in our work allows to capture physiological responses of the crop (stomatal conductance) to solar radiation, temperature, and vapor pressure deficit (Eqn. A5). In addition, this approach also avoids calculating grass reference evapotranspiration based on a constant canopy resistance."

The Conclusion section should be improved. The first paragraph is misplaced. I would like to hear a bit more about the rhizosphere conductivity under drought and see the work (at least one paper) of Andrea Carminati cited in this context. Model testing is important but how could the model be improved? Is it not a severe shortcoming that the drop in root length density in the topsoil is neglected? And, similarly, the increased root growth under drought stress? How could this be represented better in the model? What models are already out there that are capable of handling such situations?

The conclusion section was revised which should answer the mentioned objectives in the Introduction (see comments from Referee # 1) and will be added some suggestions from Referee #2. The first paragraph is shortened down to conclude for the first objective. The second graph concluded the model ability in simulating plant hydraulic conductance (which is for the second objective) with more insights of rhizophere conductance and citation from Andrea Carminati. The third paragraph concluded for the results from sensitivity analysis (third objective). The last three small paragraphs mentioned on the model limitations and outlooks. Some root growth modelling approaches will be added that we can lean and improve the models.

"We evaluated two different root water uptake modules of a coupled soil water balance and crop growth model. One root water uptake model was the often used Feddes model whereas the other, the Couvreur RWU model represents a "mechanistic" RWU that explicitly simulates the continuum in water potential from soil to root, and to leaf based on the whole plant hydraulic conductance. Overall, the measured biomass growth, LAI development, soil water contents, leaf water pressure heads, and transpiration rates were well reproduced by both models. But, the Fe model incorrectly predicted more water stress and less growth in the silty soil than in the stony soil whereas the opposite was observed. The Fe model does not account for the higher plant conductance in the silty soil where more roots were simulated than in the stony soil. In addition, the Fe model does not consider root water uptake compensation which reduces water stress. In other words, the Feddes approach did not possess the flexibility as compared to Couvreur model in simulating RWU for different soil and water conditions.

Based on the absolute root length, the Co model was able to simulate K_{plant} in different soils and treatments. The simulated K_{plant} followed the root growth and reached a maximum at around anthesis. However, the observed K_{plant} was lower in the sheltered plots although the observed total root lengths in these plots were almost similar (stony soil) or larger (silty soil) as compared to the irrigated and rainfed plots. Moreover, the higher simulated K_{plant} in comparison to the observed values in the sheltered plots suggested that the newly coupled model needs to consider the declined hydraulic conductance of the root-soil interface due to decreased soil water pressure head. The formation of air gaps at soil-root interface due to the root shrinkage of roots and root-soil contact loosening (Carminati et al., 2009) could induce a strong increase of hydraulic resistance to radial water flow between soil and roots.

A mechanistic model that is based on plant hydraulics and links root system properties to RWU, water stress, and crop development can evaluate the impact of certain crop properties (change of root segment conductance, specific weights of root, or leaf pressure head thresholds) on crop performance in different environments and soils. The Co model could capture the positive feedbacks between the aboveground biomass, the root length, the total root system hydraulic conductance, and finally K_{plant}.

In this study, a higher total root length was simulated in the silty soil than in the stony soil because a higher specific root length was found for root growth in the silty soil. This can be considered as an extra relationship that requires attention in crop modelling. Crop growth models will need to consider soil specific calibration to account for differences in specific root length with soil. Alternatively, a more mechanistic description of root growth that predicts root specific length would reduce the amount of calibration in crop growth models. Another aspect in demand of improvement is the prediction of the root distribution with depth. In our simulations, highest root densities were simulated in the top soil whereas the observations showed higher densities in the deeper soil layers. Examples of detailed 3D root growth models that could improve the simulation of root distribution are given by Dunbabin et al., (2013). The coupling of a shoot model with a 3D root growth model that represents root system architecture simulated more accurate root distributions (at both top and subsoil layers) under drought conditions (Mboh et al., 2019). Nevertheless, simulating the third dimension of root growth would largely extend the parameter requirements which makes them more difficult for testing under the field.

Finally, the model did not consider changes in carbon allocation to the root system that are triggered by stress. Therefore, the model simulated less roots in the water stressed sheltered plot of the silty soil whereas more roots were observed in this plot compared with the other plots in this soil. A more mechanistic description of root: shoot partitioning of both carbon and nitrogen (Yin & Schapendonk, 2004) or carbon allocation as a function of soil water conditions (i.e. soil water potential in Kage et al., (2004) and Li et al., (1994)) would be needed to refine the prediction of responses of root development to water stress.

Future research should focus on testing the newly coupled model (HILLFLOW–Couvreur's RWU–SLIMROOT–LINTULCC2) for other wheat genotypes and crop types (isohydric like maize) and for a wider range of soil and climate conditions. Further improvements should particularly be targeted leaf area simulation. Improving the modelling of leaf growth should result in better simulations of LAI and more accurate estimates of energy fluxes at canopy level."

Does the fact that P1 receives less water but is exposed to the same weather situation in regard to all other weather variables (e.g., air humidity) as P2-P3 might have biased (in the sense of an artifact) the reaction of crops in the field as compared to the simulations?

We agreed and understood that in the field, the rainout shelters might influence the P1 itself and nearby P2 and P3 plots with regard to air circulation (and thus air humidity and canopy temperature). We also expected that there could be a difference with regards to microclimate conditions and crop reactions amongst the plots. We tried to minimize as much as possible the effects of shelter application on climatic conditions and crop growth. The rainout shelter was used when it rains and removed directly when rain stops to minimize the effects of the shelter within the plot P1 and on plot P2 and P3. Water was collected by the shelter from P1 was drained out that did not pour on the P2.

Detailed comments

line 41 function of

It was corrected

44 correct: are lost

It was corrected

46 make clear that you use the terms water potential and hydraulic potential coherently. Better, define it for the readers from different fields. There is a problem because traditionally for plant scientists water potential does not contain the gravitational component, for soil scientists it does. What is root zone water potential? Is it the hydraulic or matric potential in the rhizosphere? probably not.

It was corrected. One sentence was added to clarify the terms in the introduction part and used consistently for the whole paper.

58 computation of

It was corrected

228 In both models, delete "for each model"

It was corrected

235, 240 in a given layer

It was corrected

238 delete "sufficient"

It was changed

239 I recommend deleting "which is based on a mechanistic description of water flow in the coupled soil-plant system," because you are her in the technical part.

It was deleted

252 delete "the"

It was deleted

287 UTC is more confusing than local time

We do not understand clearly this comment. It is important to match time of weather input data and output data from the models with the time of the measured data. The conversion of the local time (CEST and CET in Germany) to UTC time aims at avoiding the time confusion since the UTC is standard time which might help the paper targets to “broader readers”.

292 better "characterizes the difference" or "is a measure for the difference"

It was changed “characterizes the difference”.

297 "are uncertain"

It was changed.

304 seminal roots

The range of value is for the lateral root. This will not be changed.

310 units missing

Unit was added.

311 threshold (index)

We agree, threshold was added.

316 better reverse: kplant explicitly simulated by...

This was changed.

317 we present and discuss the results of the sensitivity analysis

It was revised.

324 in fair agreement (at best)

It was revised.

358 Grammar: this should not be emphasized too much Content: This cannot be emphasized too much because it shows a clear and important shortcoming of your modeling approach and gives a point of leverage for the next step of improvement.

It was revised.

366 better: transpiration rates simulated by the Fe/Co model or simply transpiration rates by the Fe/Co model

It was revised.

376 less dry

It was revised.

379 "from" end of May

404 I do not understand how you define adequate. I would rather write fair.

It was revised.

407 Pg is not defined. For the reader, it is better to write it out.

Thank you. The Pg is defined.

443 the sheltered plot with the silty soil (the field is the same - according to figure 1) if this is not true add the field borders in figure 1

This is in line 433 not line 443. The two sites were in the same field (around 200 m long). There is no field borders however the two sites were situated in two different soil types. The text was revised.

444 comma before based

It was revised.

445 delete "in the measurements" (perhaps: observed in the field)

It was revised.

446 and elsewhere see above 443

It was revised.

450 must have causes not considered in the model ("other causes that" is wrong here)

It was revised.

455 The sensitivity analysis is, frankly speaking, a bit boring (sorry). It destroys the flow of the paper and feels like a "lost part". The reader should be left off the hook after Figure 9, but (recommendation) after a better discussion of what he or she can learn from all that.

Thank you very much for your suggestion. The couple root: shoot model with such the mechanistic RWU Couvreur model with considering two ways coupling has not been done before. This is the first study to evaluate the performance of the coupled root: shoot model. Thus, the sensitivity analysis is strongly necessary because of several reasons (i) to understand the roles of important parameters of Courveur model itself (critical leaf water pressure head and plant hydraulic conductance) and other root hydraulic conductance parameters which are rarely available at field scale (see Cai et al. 2017 and Cai et al., 2018) or the root parameters which are often used in crop models that might contribute to plant hydraulic conductance (ii) to understand the feedbacks and effects of aboveground biomass, root growth, root system hydraulic conductance, whole plant hydraulic conductance, and leaf water pressure head threshold on RWU and biomass (iii) the feedbacks of aboveground crop growth to belowground can be only analyzed with Courveur model (iv) by doing this analysis, the important roles of hydraulic conductance and necessity of two-ways couple can be emphasized. Thus, the authors would like to keep this section (together with Fig.10). However, the section is shortened down. Line 479 to lines 487 will be deleted (see also the replies to comments from Referee #1 on this paragraph).

479 are lower than those of old cultivars (not were)

It was revised.

481 indicates that

It was revised.

490 potential

It was revised.

495 more mechanistic, then you can drop the quotation marks

It was revised.

496 no comma

This in line 499. It was revised.

549 How were xj and wj determined? Should it not read LAI(xj)?

The formulation was revised by adding the multiplicative symbols. An integral from [0, LAI] needs to be changed into integral over [-1, 1] before using the Gauss –Legendre quadrature (Stoer and Bulirsch, 2002). The estimate of x_j and w_j can be derived from page 178, Chapter 3.6. "Gausian Integration methods". The x_j and w_j can also be found from https://en.wikipedia.org/wiki/Gaussian_quadrature.

554 "thus there was no Gaussian integration over time degree" - this cannot be understood

It was revised

559 better write "grass reference evapotranspiration (FAO, give the reference)"

Please see our above reply for the role of ETP calculation with Penman-Monteith equation. This is not grass reference evapotranspiration. The potential evapotranspiration (ETP) is calculated by hourly Penman-Monteith equation from Allen et al., (1998) (See Equation 3, Chapter 2 in the FAO Irrigation and Drainage Paper No. 56, Allen et al., 1998). The ETP is calculated from non-stress crop canopy conductance (fwat = 1, see Figure 2).

561 reference needed

Reference was added

565 How were surface resistance and aerodynamic resistance calculated?

The surface resistance was corrected. The surface resistance is crop canopy resistance (Eqn. B5). The hourly aerodynamic resistance (r_a) is calculated as Equation 4, Chapter 2 in the FAO Irrigation and Drainage Paper No. 56, Allen et al., (1998).

578 verb missing matric potential, not matrix potential matric potential head should have a unit, here m Please make clear in the whole paper if you talk about the soil matric potential or soil hydraulic potential. Otherwise it is confusing. Here, for example, I feel that you mean soil matric potential. Actually I would avoid using the expression soil water potential.

This term of was changed in the paper.

Figures

All figure captions should be formulated more carefully and with more empathy for the reader.

The captions were revised.

Figure 1 Indicate what kind of rock.

The rock type was indicated.

Figure 7 You should try to explain the systematic deviation in the deeper soil layers. 998: The should be better described in the text.

The Caption was revised. Thank you for your suggestion. The systematic deviation in the deeper soil layers was explained from Line 419 to 425.

Figure 6, 8 Rephrase the confusing caption. You should start with a statement about what the reader can see. Include the top graphs in the enumeration. Psi_leaf and Pg should be defined in the caption (as RWU).

The caption was revised. The enumeration is added for the top graph. The full names of RWU, Pg, and ψ_{leaf} were defined.

Figure 10 should be deleted

This Figure 10 was kept, please see reply Line 455 for the "sensitivity analysis" section

Additional references:

Carminati, A., Vetterlein, D., Weller, U., Vogel, H.-J., & Oswald, S. E. (2009). When Roots Lose Contact. *Vadose Zone Journal*, *8*(3), 805–809. https://doi.org/10.2136/vzj2008.0147

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Kage, H., Kochler, M., & Stützel, H. (2004). Root growth and dry matter partitioning of cauliflower under drought stress conditions: Measurement and simulation. *European Journal of Agronomy*, *20*(4), 379–394. https://doi.org/10.1016/S1161-0301(03)00061-3

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Srivastava, R. K., Panda, R. K., Chakraborty, A., & Halder, D. (2018). Comparison of actual evapotranspiration of irrigated maize in a sub-humid region using four different canopy resistance based approaches. *Agricultural Water Management*, *202* (February), 156–165. https://doi.org/10.1016/j.agwat.2018.02.021

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Yin, X., & Schapendonk, A. H. C. M. (2004). Simulating the partitioning of biomass and nitrogen between roots and shoot in crop and grass plants. *NJAS - Wageningen Journal of Life Sciences*, *51*(4), 407–426. https://doi.org/10.1016/S1573-5214(04)80005-8