

## ***Interactive comment on “Horizontal soil water potential heterogeneity: simplifying approaches for crop water dynamics models” by V. Couvreur et al.***

**V. Couvreur et al.**

vcouvreur@ucdavis.edu

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Please note that the attached pdf file contains the same answers with associated print-screens of changes brought to the manuscript.

To Dr. Quirijn de Jong van Lier:

We are grateful to Dr. Quirijn de Jong van Lier for the positive and constructive comments concerning our paper. For reading convenience, we recalled and numbered the comments as follows:

Comment1: On the fact that water potential at plant collar is considered as equal to leaf  
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water potential in the water stress function. “This assumption is equivalent to saying that longitudinal (collar to leaf) resistance is zero. Is it justified to assume this? In that case, why don’t you just substitute collar potential by leaf potential in your equations? On the other hand, it would be straightforward to implement the collar to leaf resistance by an extra term in eqs. 4, 5. The manuscript would benefit by a discussion about this question.”

Answer1: The referee is right about the fact that we neglect the hydraulic resistance between plant collar and leaf, and we think that this is justified in our study for two main reasons: (1) the relative value of this hydraulic resistance in series, and (2) the expected impact of the chosen water stress function on the results of this study.

(1) In wet conditions, the most important hydraulic resistance against water flow from soil to leaves is located between soil-root interface and root xylem (Frensch and Steudle, 1989). In relatively dry conditions, the soil hydraulic resistance may become the most important one (Draye et al., 2010). Both of these major hydraulic resistances are taken into account in the presented results. The latter entails a major loss of water potential from the bulk soil to soil-root interfaces, while the former entails a major loss of water potential from soil-root interfaces to root xylem. According to the relatively low stem hydraulic resistance of the simulated crops, we believe the additional loss of water potential due to stem xylem hydraulic resistance is of relative minor importance. We however ponder the validity of this assumption for certain plant types, as demonstrated empirically by Domec and Pruyn (2008).

(2) If we had taken into account this additional resistance, by generating an aerial hydraulic architecture in R-SWMS and using an additional equivalent resistance in the macroscopic model, water stress would probably have occurred slightly earlier in the simulated scenario, and induced slightly lower transpiration rates, but we believe this would not have had any significant impact on the results of this study. The prediction of plant transpiration rate is not a central objective in this article. The referee also asks why not substituting collar potential by leaf potential in the equations. The reason is

that we do not want to mix the assumptions of the different models used in the study. Considering that leaf water potential equals collar potential is an assumption necessary for our water stress model (Eqs. 5-6). It is not a requisite of our macroscopic root water uptake (RWU) model, which predicts collar water potential. For instance, we do not want a user intending to model RWU from measured stem flow or collar water potential to believe that he first needs to check if water potential is the same at plant collar as in leaves.

Comment2: The statement “Even though it does not appear in their units” sounds odd and seems unnecessary. “You might also refer to two plants, a dozen of plants, or a square meter. In fact, if you presume all related computations are realized “per plant”, then in eq. 5,  $K_{rs}$  is “the equivalent conductance of the root system, PER PLANT”. If you want to maintain the affirmation “per plant” on line 28, you should include it also in some other units (values of  $K$ ). On the other hand, you might omit all of these, or make a more general statement at the beginning of the section explaining the considered system is “one plant”. Observe that you already use the plant-1 unit at some places in the manuscript (e.g. p. 1218, line 19)”.

Answer2: The referee is right, as suggested, we removed the “per plant” units in the two concerned parts of the manuscript, and notified by a general statement that these variables and parameters are related to a single plant. We also warned the reader about the possible inapplicability of the model for several plants having different  $K_{rs}$ , as suggested by referee#2.

Comment 3: On the inconsistency in the units of Eq. (20).

Answer 3: The referee is right. We modified the units according to his suggestions.

Comment 4: On the replacement of “time consumption” by “computing time”.

Answer 4: We did the replacement in the whole manuscript, including tables 2 and 3, and figure 5.

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Comment 5: On the fact that soil capillary flow is a process seen as dissipating soil water potential heterogeneity, but also leveling out differences between root water uptake models (p. 1221, lines 22-25 and p. 1223, lines 23 and further): “This is a plausible and interesting finding. If it is correct, wouldn’t this imply in the fact that the scale of modelling is too detailed to be of practical use? In other words: if simulated differences in root water uptake are masked by compensatory water flows that level out the pressure head differences, wouldn’t that be an indication that a simulation at a less detailed scale would be an improvement in terms of modelling efficiency?”

Answer 5: We agree with the conclusion of the referee. To some extent, high soil water redistribution rates may justify the use of coarser soil grids, but also of less accurate RWU models, and increase modelling efficiency (as shown with the wheat simulations in 1-D, or maize in 2-D). An extreme but didactic example is that of a glass full of water containing a straw. Whatever the position at which water is taken up with the straw, the resulting water content distribution in the glass will always be the same (water disappearing on top), because the redistribution rate of water in the glass is extremely high. In such condition, any water uptake model provides the right water distribution, but also the system can be represented in 1-D because no horizontal heterogeneity persists. Back to RWU: A person simulating soil-plant hydrodynamics in 1-D can either consider that the associated assumption is (i) that the RWU function is horizontally uniform, or (ii) that soil water horizontal redistribution rate is unlimited. Both assumptions are equifinal and may provide accurate results as compared to a 3-D reference in case horizontal redistribution rate of water is high enough in 3-D.

Comment 6: On the conclusion of Faria et al. (2010), which already stated that rooting heterogeneity implied the use of a correction factor for calculation of  $R_{ho}$ .

Answer 6: The referee is right. We apologize for this unintended mistake. It is now corrected in the text.

Other typos and technical clarifications reported by the referee were taken into account.

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To anonymous referee # 2:

We are grateful for the positive and constructive comments of anonymous referee # 2. For reading convenience, we recalled and numbered the comments as follows:

Comment 1: "I agree with referee de Jong van Lier on the issue of plant collar-leaves water potentials. Assuming that they are identical implies to assume zero collar-leaves resistance and it is necessary to add a discussion on the implications of such an assumption. In addition, the authors neglect the cavitation processes that may occur in the plant xylem even though they have been shown to play an important role in regulating plant transpiration (e.g. Domec et al 2012). If the authors decide to follow the referee's comment and implement the collar to leaf resistance by an extra term, they should also consider introducing a "vulnerability function" to account for cavitation (e.g. Daly et al. 2004)".

Answer 1: Most of this comment was addressed in Answer 1 to referee de Jong van Lier. We understand that for plants whose stem hydraulic conductance may limit transpiration rate, the implementation the cavitation process and associated vulnerability function would make sense in order to obtain more realistic estimates of xylem limitation on plant transpiration under water stress. We also believe in the interest to implement such function for future studies concentrating on quantifying the transpiration rate along with water redistribution rates by soil and roots for other plants, such as trees. However, in this study, the objective is to simplify horizontal soil water flow calculation within the root zone, and a refinement of the chosen water stress function of the modelled crops will not affect our conclusions.

Comment 2: "Secondly, since the study addresses the issue of upscaling but focuses on the plant scale only, it would be interesting to add a discussion on the implications of these results to larger scales (e.g. field or watershed). In other words, what happens in terms of model predictions when the model grid is in the order of 10 m instead of 10 cm? And what about the overlapping of root systems? I personally think that the

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manuscript would benefit from a discussion on these topics".

Answer 2: As requested by the reviewer, an additional point on the applicability of the model at larger scales (for several plants) was added (see answer 2 to referee de Jong van Lier). As explained in Sects. 3.1.1 and 3.1.3, root system overlapping is accounted for in the simulation through the use of a periodic domain for root architecture, water flow inside roots and soil water flow. Using such a periodic domain is equivalent to modelling hydrodynamics in a field in which overlapping root systems are all identical and regularly spaced. That is why we focused on SWP heterogeneity within the root zone of single plants (there is no heterogeneity at scales larger than the plant scale in our simulations). We made these points clearer in the manuscript and added the larger scale study as future prospect in the outlook.

Comment 3: About the M soil elements mentioned below Eq. (3): "Do you account for all the soil elements or only the soil occupied by roots? Obviously it depends on how SSF is defined but this is not clear here".

Answer 3: The definition of SSF is now clarified. Indeed we account for all soil elements, but those containing no root have a null standard sink fraction. In consequence, they have a null weight in the average SWP.

Comment 4: About the fact that boundary conditions at the edges are not explained.

Answer 4: It was actually explained in Sect. 3.1.1 instead of 3.1.3. We corrected the location of this explanation (periodic domain).

Comment 5: "Tpot is calculated on the basis of potential EvapoTranspiration ETref by the FAO methods. However, ETref accounts for both plant transpiration and soil evaporation. The latter should not be accounted for in the RWU term. Can you please discuss this point".

Answer 5: In this study, we did not model evaporation at soil surface in order to concentrate on RWU and soil capillary flow as processes generating and dissipating SWP

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heterogeneity. For simplicity, the part of evaporation in the ETref was considered as negligible; transpiration rates were thus slightly overestimated (we made this clearer in the manuscript). However, the choice of ETref was arbitrary (approaching the ETref of the region from when and where the root system was characterized) and could have been 10% higher or lower without impacting the conclusions of the study.

Comment 6: “The authors refer to “eight scenarios” but Table 1 and Fig. 4 illustrates 6 scenarios, please be consistent”.

Answer 6: We did not consider simulations using different soil discretisations as different scenarios. The 8 scenarios are combinations of the following features: Maize or wheat; Silt loam or sandy loam; high or low Tdaily. We made this clearer in the text.

Comment 7: “Consider to move the sentence “Equations . . . Appendix C” somewhere else in the text. Not sure it is related to the “Comparison with Ref scenarios” section”.

Answer 7: These equations allow quantifying water redistribution rates from reference and 1-D simulations, which are then compared. We kept this sentence at the same location but clarified the text.

Comment 8: Clarification of the sentence “Values . . . by soil”.

Answer 8: In this paragraph we quantitatively compare simulated redistribution of root water uptake to soil water redistribution. The former reaching increasingly high values with time. In other words, by modifying its uptake distribution (towards wet regions), the plant “helps” the soil keep SWP homogeneous, by allowing it recharging dry areas of the profile at higher rates (than if uptake would occur in zones that are rather dry). Both processes are quite complementary in pushing SWP towards a state of homogeneity. An additional reference was also used to illustrate that idea (Gardner and Ehlig, 1963).

Comment 9: “Equal to minus infinity”, why? Add reference to an equation to clarify.

Answer 9: Our statement was actually not accurate. We clarified this point in the text.

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Comment 10: “when the model was further coupled to Richards Eq.”, it was not clear that the model was not coupled before, please add comments in Section 3.2.

Answer 10: We clarified this point in the manuscript.

All other typos, technical clarifications and modifications to figures requested by the referee were accounted for in the new version of the manuscript.

References:

Domec, J. C., and Pruyn, M. L.: Bole girdling affects metabolic properties and root, trunk and branch hydraulics of young ponderosa pine trees, *Tree Physiology*, 28, 1493-1504, 2008.

Draye, X., Kim, Y., Lobet, G., and Javaux, M.: Model-assisted integration of physiological and environmental constraints affecting the dynamic and spatial patterns of root water uptake from soils, *J. Exp. Bot.*, 61, 2145-2155, 2010.

Frensch, J., and Steudle, E.: Axial and radial hydraulic resistance to roots of maize (*Zea-mays-L*), *Plant Physiol.*, 91, 719-726, 1989.

Gardner, W. R., and Ehlig, C. F.: The influence of soil water on transpiration by plants, *J. Geophys. Res.*, 68, 5719-&, 1963.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/11/C560/2014/hessd-11-C560-2014-supplement.pdf>

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Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 11, 1203, 2014.

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