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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Root growth, water uptake, and sap flow of winter wheat in response to different soil water availability" *by* Gaochao Cai et al.

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This paper compares the performance of two root water uptake models against a field dataset of soil water contents/potentials and sap flow measured in two contrasting soil types for three different watering regimes. The dataset is comprehensive, the model application has been performed carefully, and the results are also very interesting. The paper should make a valuable contribution to the literature on this important topic.

One concern I have is that the methods are not fully described. Firstly, the water uptake models themselves are not well explained. The equations are given, but the readers are given no indication of how they have been derived. The authors should

explain that although it is physics-based, the C model is an approximate solution to a 3D root architecture model that does involve some assumptions and simplifications. For completeness, these should be stated. For the empirical FJ model, the authors should give some background information on what the main functions and parameters in the models are supposed to reflect (there is actually some physical basis to the model).

Reply: we have the description of the FJ model in the introduction part (first paragraph of Page 2). As for the simplifications and assumptions, we added a paragraph after Eq. 4 on Page 5 (line 31 to line 41) and Page 6 (line 1 to line 4).

Similarly, although a detailed description is not necessary, the authors should at least mention the basic principles of the method they used to calibrate the model parameters.

Reply: a brief description of the method we used was added in the second paragraph on Page 7 (line 13 to line 15).

The authors emphasize that one important advantage of the physics-based C model is that it accounts for the effects of total root conductance (or root length) on uptake, whereas the empirical (phenomenological) FJ model only considers a relative root distribution. This is certainly true of the way the FJ model was originally formulated and is still mostly used. However, I think the authors should mention in the paper that the analysis in Jarvis (2011) shows that the compensation parameter omega_c in the FJ model should be dependent on the ratio of the potential transpiration rate to the total root length/conductance. From this point of view, it would have been better to calibrate omega_c separately for each combination of soil type and watering treatment. The derived values could then have been compared with the measured LAI/root length ratios. With a smaller LAI/root length ratio, the covered treatment (especially in the stony soil) should have smaller omega_c values. This could also have given better simulations of the sap flow data. This lumping of the treatments might also explain why the calibration of the FJ model seemed to suffer from poorly defined parameters (equifinality) and also

why the overall calibrated omega_c values were 0.95 at both sites, which implies that virtually no compensation occurred.

Reply: thanks for the suggestions! An additional paragraph was added to discuss the relation between the value of omega_c and the ratio of total root length to LAI on Page 10 (line 20 to line 31).

This result should also be discussed in the paper in light of the above points, because otherwise it might seem very surprising to the reader given the drought conditions that were induced in the covered treatment. Of course, ideally, model parameters should be constant! But in this case, I think it could help understanding to explore and discuss why omega_c might not be constant.

Reply: here we disagree with the statement that model parameters need to be constant. If the system properties change, the parameters that represent these properties need to change as well.

Specific (minor) comments Abstract, Line 15 (and Page 10, lines 4-14): this result is only shown in the supplementary. If it is important enough to mention in the abstract, then it should be shown in the paper itself.

Reply: thanks for the suggestion. The figure was put in the paper as Figure 8.

Page 4, line 10: how close? Please give the exact distance.

Reply: it was added in the second paragraph of section 2.1 on Page 4 (line 13 to line 14).

Page 4, line 18: Are these rainfall totals, not precipitation? You need to be careful about the choice of words here, because of the irrigation supplied to some plots.

Reply: they were the total amount of rainfall in the two soils. It was changed accordingly in the last paragraph of section 2.1 on Page 4 (line 21).

Page 6, line 28: some brief details of the method are needed here.

Reply: the method was briefly described in the first paragraph on Page 7 (line 13 to line 15).

Page 7, lines 25-32: you could also discuss the effects of water treatment on LAI here. LAI may be more directly related to potential transpiration than above-ground biomass?

Reply: thanks for the suggestion! The discussion of the effects of water treatment on LAI was added in the first paragraph of section 3.1 on Page 8 (line 14 to line 20).

Page 8, line 22: “above-ground”

Reply: it was replaced accordingly in line 9 on Page 9 (line 9).

Page 8, lines 30-31: better to replace “stimulated” by “restricted” and swop “silty” and “stony”

Reply: thanks for the suggestion! They are changed accordingly in line 17 and 18 on Page 9.

Figure 10: perhaps this should be split into two figures?

Reply: it was split into two figures (Figure 11 and 12) accordingly.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-711/hess-2017-711-AC2-supplement.pdf

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