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Discussions

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Interactive comment on "Disentangling temporal and population variability in plant root water uptake from stable isotopic analysis: a labeling study" *by* Valentin Couvreur et al.

Anonymous Referee #3

Review of Manuscript HESS-2019-543

Disentangling temporal and population variability in plant root water uptake from stable isotopic analysis: a labeling study by Couvreur et al.

This manuscript compares two alternative modeling strategies for deriving the sink term (root water uptake) in a controlled ecotron experiment. Strategy 1 uses a simplified root water uptake model which however incorporates the main features of the three dimensional soil water flow, including hydraulic redistribution. The unknown model parameters are calibrated based on isotope data in the tiller and leaf water potentials. Strategy 2 derives root water uptake based on isotope data using Bayesian inference.

The authors find that the results between the two strategies diverge. They show that Bayesian inference yields unphysical fluxes. Based on the model results they conclude that spatial variation ("swarm-like") in tiller isotopic signal is misinterpreted as a strongly fluctuating time series, whereas it actually reflects the different rooting depths of plant individuals. Additionally, they argue that both the root water uptake model and the soil moisture time series suggest hydraulic lift, which cannot be captured by the Bayesian inference based on isotope data alone. Therefore, they conclude that the results obtained based on Bayesian inference could be due to an artifact.

This is a valuable contribution illustrating how sampling choice may affect the interpretation of isotope data. Especially the application of a straightforward process model for comparison with the Bayesian inference together with the dense measurements are extremely helpful to explain the shortcomings of deriving uptake profiles based on isotope data alone. The case is well argued and the methods are sound. I feel the manuscript has potential to making an impact and will find strong interest in the readership of HESS. The paper is mostly well structured, although I have some concerns with the Abstract and Methods section, as well as with some formulations (see below).

I have two general concerns, and a number of editorial remarks (below): The investigated case is a particular one, e.g. with a strong labelling pulse added below the rooting zone. This needs to be explicitly stated and the manuscript should discuss in which other situations such a strong influence of spatial variation is to be expected (and where it is not a concern).

The study suffers from lack of opportunity for validation: The heterogenous rooting depths cannot be measured in situ and therefore it remains a hypothesis. This is ok. But it requires diligent consideration of other assumptions of the model that may have had a similar effect on the model result. How about the inherent assumptions of a big leaf? Could individual differences in leaf development incur similar results? Those considerations need to enter more than now into the discussion. I propose adding a section dealing with the effect of inherent model assumptions.

Detailed comments:

Abstract: Line 18-19: This sentence sounds vague, e.g. "semi-controlled" and "such variables", please formulate more specifically. Line 23-24: "results underlined the discrepancy .. " At this point unclear what is meant Line 29-30: The sentence starting with "The physical model .. " is difficult to understand, please reformulate Line 33: "local increase . . ." this results is not stated earlier and at this position confusing.

Lines 35-62 List of variables Some variables are missing, please complete. Also I propose erasing all the repetitions of "units of". Later in the paper, it will be useful to express volumetric water content as vol-% and I propose adding it here.

Introduction Line 75-76: The description of the "mean value of . . . weighted by" is confusing, please rephrase

Material and Methods Line 114: Please mention that CS616 is a time domain sensor (TDR). Also, reflectometer is a correct, but awkward term for soil moisture sensor. I propose using the latter, just to avoid confusion.

Line 123: The description of the soil is confusing. Is District Cambisol a typo for "Dystric Cambisol". Otherwise, I am not aware what a District Cambisol would refer to, please explain. Besides, Cambisol refers to a soil in situ and after specific pedogenesis which is completely removed in your experiment. Maybe say "The soil originates from a xx Cambisol". Also, does the bulk density refer to the original soil, and is it required to be mentioned?

Line 125: Add "layer" before "by"

Line 128: sols-PST55 sensors are missing above, where installation depths were mentioned. Add there. Please add where they are installed.

Line 130: "between its position and measured soil water content" unclear, please rephrase

Lines 132-136: Please shortly state: Were the plants watered? What was the lower boundary condition?

Line 139: Do you mean "sides" instead of "slides"

Line 144: "three plants were sampled" - does it mean the entire plant or some leafs?

Line 148-149 I believe you mean "from the atmosphere surrounding the rhizotron". Also, I am assuming the latter means the ambient air in the lab? Would be good to specify. Line 162: "60 tall festucae root systems .." Why 60 plants?

Eq. (1) Personally, I do not find this equation obvious. Please motivate the origin. Are there any other assumptions involved besides the big root one?

Line 173-174: "dimension of the domain . . . " I do not understand this statement

Line 176-177: Sentence starting with "The averaged distance .." seems wrong. Maybe erase the last words?

Eq (4): Se is not part of the List of variables, plus the S stated there refers to the sink term not saturation. Please use a different abbreviation.

Line 188: Could not the measured root length density profiles be used?

Line 191: "were derived" unclear how this was derived? Also, where was k_axial in Eq. (7) taken from? Please explain. Ok, I learn later this was calibrated. Maybe mention this here.

Line 191: "root system class" Unclear what is meant with "class".

Line 194: standard sink distribution is not a standard term and requires a bit more explanation to be convincing.

Line 195-196: "potential difference between soil and leaf": You are dealing with a soil profile and a leaf canopy. Thus, where in the soil and leaf are you referring to. Please also translate to what this means for your experimental setup either here or in the

discussion.

Line 196: "assuming negligible stem conductance": Does this imply that all conductance / resistance happens in the root system? Is this a reasonable assumption? ĂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂă Âă Line 202: "class" unclear

Line 205: "where axial conductances" this comes too late, please move up.

Line 218: I propose moving the inverse modeling procedure out the the appendix and add it to the main text. It is important information.

Line 226: Not sure what is meant with "ten identified potential water sources" .. "10 distinct soil layers" Can you be more specific?

Results and discussion: Lines 335-252: Small issue: Please add some paragraphs in this section.

Line 281: Do you mean "and" instead of "et"

Line 304: With "all the population" do you mean all individuals?

Line 311-313: Sentence is difficult to understand, please rephrase.

Lines 329ff: Since there is repeatedly reference to increasing by xx% this may be strongly confusing. Better use vol-% to be on the safe side.

Line 340 Lambda is not in the list of variables. Also, this information is very compact, and difficult to understand. Please elaborate.

Lines 345ff: I strongly recommend bringing Fig E into the main text. It is discussed and seems therefore sufficiently important. Also, because this is one of the two alternative water uptake profiles which comparison is the main motivation of the manuscript.

Line 360: Replace "first" with "top"

Line 367: Not sure what is meant with "broadcasted"

Figure 2: I was confused about the positive water potentials. Since they were named psi I was instinctively assuming to see matric potential, but plotted are the water potentials. I propose renaming to h. If you want to stick to psi_soil because of psi_leaf (although I seriously think it would not be an issue), please obviously state the reference elevation to avoid this type of confusion.

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