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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Stem-root flow effect on soil–atmosphere interactions and uncertainty assessments" *by* T.-H. Kuo et al.

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The reviewers' comments and suggestions are very insightful and help to improve this paper. We really appreciate the reviewers' efforts and time. In the following we respond to the reviewers' comment and indicate the corresponding text revisions in the quotation marks. All the line numbers mentioned in our reply refer to those in the revisions.

1) The methodology is reasonable, but the lack of field data guiding the selection of parameter values is a major concern I have. Data at the two sites are from bulk measurement with no process-level data to confirm or refute results from the numerical modeling study.

Reply: We fully agree that it is important to have measurements data to validate the

modeling on this process eventually. Our current study is an initial attempt, hoping to demonstrate the importance of and uncertainty in current modeling of the stem-root flow processes. We also wish to identify key parameters that lack of verification, and justify for further investigation of this issue through theoretical and observational approach. As indicated in our paper title and in the text, assessing the uncertainty in modeling this process is an important subject of this paper. We have added more discussions in the revision (see other responses below).

2) Related to 1), what kind of field measurement is needed to provide data for such modeling? That might be a real contribution the authors can make through this paper.

Reply: Most of the key parameters are shown in the equations of this paper, including the root distribution, the stemflow to leaf drainage ratio, and root flow velocity. We pointed out this in the Discussion section of the revised manuscript (lines 336-337).

3) Based on results shown, the magnitude of the maximum possible changes caused by stem-root flow is still rather small (relative to the model bias), although qualitatively it does nudge some of the model results closer to observations. This point warrants extensive discussion.

Reply: We agree that introducing this process will not solve all the problems in the soil moisture modeling. As the reviewer pointed out in Specific Comment 1, other factors, such the hydraulic redistribution, as well as the low resolution modeling that pointed out by another reviewer, may also contribute to the deficiencies in soil moisture modeling. We commented these in the discussion section of the revised manuscript. However, a comprehensive discussion on the causes of current modeling deficiencies in soil moisture modeling is out of the scope of this paper. Here, we also want to provide another supporting evidence for the possible stemflow effect for the reviewer's reference. Figure A shows the correlation between hourly changes in precipitation and soil moisture at Lien-Hua Chih (LHC) station in 2010. The correlations are higher at deeper layers during the stronger rainfall intensities. Such a relationship is a good indication of the stem-root flow mechanism. If the reviewer and editor think this is appropriate, we can include this figure as a supplement.

* Figure A: Correlation coefficient of hourly âŰşPrecipitation and âŰşSoil moisture at LHC, 2010.

4) Since most of the differences between control and experiments are rather small in magnitude, it is important that information on statistical significance be presented in the figures.

Reply: We have conducted the statistical tests for Figures 4 and 5. The soil moisture changes are statistically significant (reached 95% confidence level) in all seasons at LHC. However, at HAPEX, the responses of SM2 and SM3 to the stem-root flow are statistically significant (>95% confidence) only during late summer and autumn (the main growing season and relatively dry soil); whereas the responses in SM1 reached only the 94% confidence level. We indicated this in lines 187-188 and 201-203 of the revised manuscript. Figures 8-9 show that the differences due to different Vs are not that large, but this may be model dependent. We do not want to mislead the community that the measurement of VS is unimportant at this point, so its statistical test was not conducted.

5) If it is not feasible to collect field data to guide the explicit parameterization of this process, a more appropriate approach is probably to relate soil hydraulic conduct over vegetated land to vegetation density (therefore root density). I suspect that might be a more feasible approach that can be tested in the field. Again, this is an aspect that discussion in the paper and suggestions will make a real contribution to the field.

Reply: We have pointed out the key parameters that lacked of data. As for how to do the measurements, we incline not to get into such details as we are not experts in field experiments. Nevertheless, the reviewer's suggestion looks reasonable and is greatly appreciated.

Specific comments: 1. Abstract is extremely confusing, due to the inappropriate use of terminology “soil water” and “vertical redistribution of soil water”. This terminology has specific meaning: In the context of “hydraulic redistribution” (including hydraulic lift and hydraulic descent), water becomes soil moisture before it gets redistributed via plant root, a rather slow process. In this manuscript, it refers to flow through preferred conduit (root channel) during infiltration process and happens during or immediately after precipitation events. The abstract led me to expect something totally different than what the authors end up talking about.

Reply: We are not aware that “soil water” has specific meaning. So, we changed all “soil water” to “soil moisture” (except in “soil water potential”) in the abstract and in the text to avoid confusion.

2. Line 84: Deff: either in the main text or in the Appendix, a much better explanation is needed for what Deff represents. Not in mathematical terms, but rather, a physically meaningful explanation.

Reply: Agree. We have added a schematic diagram and brief explanation at the beginning of the Appendix to make the D_eff concept easy to apprehension physically. As shown in figure A1 below, the part of soil next to the root flow absorbs water and form a thin, saturated boundary of width "lambda". A gradient of soil moisture is formed in the transition zone (of width "delta"), with soil moisture potential decrease from the saturated state, Ψs, to that of the bulk soil, Ψw. Diffusion of soil moisture toward the bulk soil is directly proportional to this gradient.”

* Figure A1: Schematics of the root flow-soil boundary and soil moisture transition for the parameterization of horizontal water flux qx. The red-dashed line represents the analytical solution, and the black-solid line represents the parameterization. Soil moisture is saturated in the root-soil boundary (width "delta"), and decreases linearly in the transition zone ("lambda") before reaching that of the bulk soil.

3. Lines 142- 146 and in other places: The potential role of plant uptake in causing the

dynamics of soil moisture in the middle layer is not considered or discussed.

Reply: Yes. We clearly indicated that plant uptake was not considered in this study in lines 208-209 “Note that SSiB does not consider the potential role of plant uptake, which might be potentially important in the middle layer” and 324-326 “On the other hand, the overestimation of the middle-layer soil moisture at HAPEX may be partly contributed from the plant uptake process which was not considered in this study” of the revised manuscript.

4. Lines 160- 167: on comparison between model and observation: Due to the lack of process-level data, there is no way to gage whether this improvement is truly due to improved model physics or due to error compensation related to other model deficit. This point has to be made clear.

Reply: Theoretical studies in many occasions develop in advance before the observation and use the available data to indirectly evaluate their theory. For instance, the development of cloud convective scheme and radiative transfer scheme are developed much earlier than the satellite data are able to make some direct measurements to validate the theory. Otherwise, there will be no scientific development at all. In this paper, we have indicated that in 11796 L15 (lines 330-332 in the revision) that Henderson-Sellers (1996) indicated that a full evaluation of land surface model’s simulation against observations can be established only when the initial conditions and all soil parameters are known precisely. We also add that “because this study lack of process-level data to validate, so the improvement should be more prudent to represent” (lines 332-333 in the revision). We wish this paper could stimulate more theoretical study and field measurements which can further evaluate the hypothesis proposed in this study.

5. 1st paragraph on Page 13: what about transpiration increase due to deep soil moisture increase? Should be factored in in this discussion.

Reply: We believe the reviewer was referring to the consideration of deep soil moisture in determining energy-limited versus moisture limited regimes. The original Budyko’s

curve considered only the top-soil conditions, but the deep soil moisture certainly is very important too. We have added the following statement to stress the role of deep soil moisture (lines 284-285): “Note that this regime separation needs to take into account the contribution of deep soil moisture to transpiration.”

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C6376/2016/hessd-12-C6376-2016-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11783, 2015.

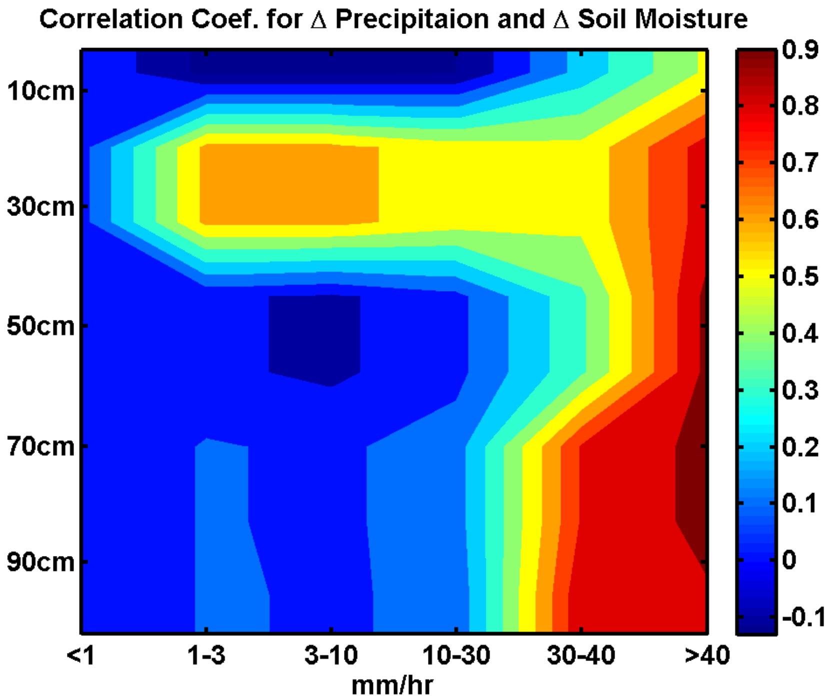

Fig. 1. Figure A: Correlation coefficient of hourly âŰşPrecipitation and âŰşSoil moisture at LHC, 2010.

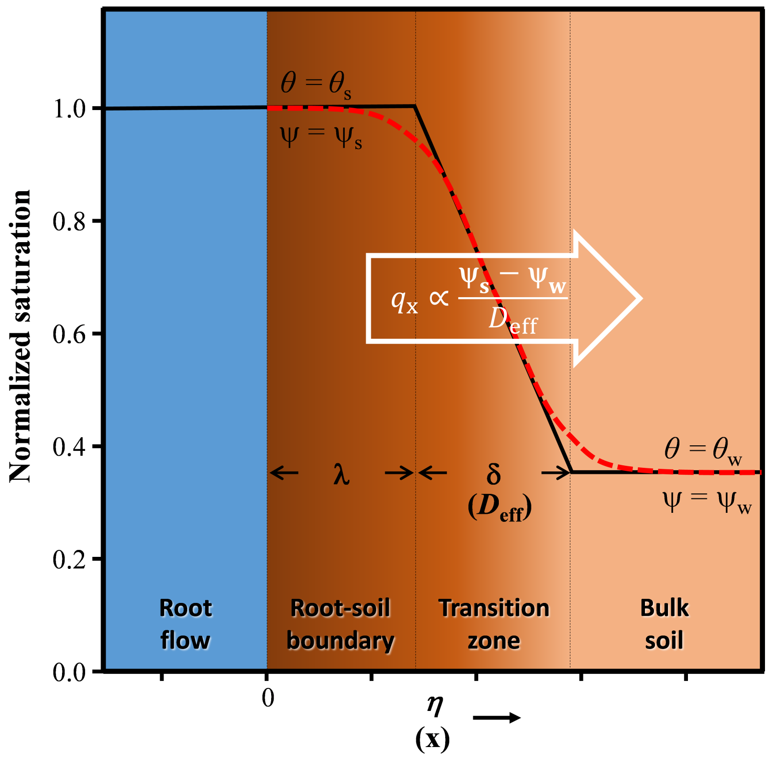

Fig. 2. Figure A1: Schematics of the root flow-soil boundary and soil moisture transition for the parameterization of horizontal water flux qx. The red-dashed line represents the analytical solution, and the