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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #3

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This paper presents a parameterization to represent the impact of stem-root flow, a process that has not been considered by land surface models in the past. While this is an important topic, I have several major comments as follows: 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. 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. 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 ex-

tensive discussion. 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. 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 room 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.

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

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