

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 (which are shown in blue fonts) and indicate the corresponding text revisions in the quotation marks. All the line numbers mentioned in our reply refer to those in the revisions.

Anonymous Referee #2

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In this paper, a new process about vertical water movement along with plant root was incorporated in a land surface model. Sensitivity tests were conducted at two experimental observation sites, and the impacts were investigated. The results showed better agreement with both sites, and the impacts on the atmosphere-soil interaction were different between the two sites depending on the climatological characteristic.

I liked this paper because the message is straightforward and the method is well targeted. The parameterization of indicated process seems applicable to macro scale models, such as climate models. So there is potential benefit to introduce this scheme to the community by publishing this paper. Here are some major and minor comments for the authors to further improve the manuscript.

Major comments:

1. There have been quite a few studies about partitioning of water transport recently (Jasechko et al. 2013; Good et al 2015; Wei et al., 2015; etc.). The parameterization proposed in this manuscript should the partition of latent heat (E or T) and runoff (surface or subsurface). In considering those studies, what is additional information/constraint that this paper proposes?

Reply: First, we thank the reviewer for providing these references. They are now cited in the revised manuscript and added relevant comment on lines 286-290: "Regarding the partition of water transport, recent studies (e.g., Jasechko et al., 2013; Good et al., 2015; Wei et al., 2015) explored the dominant role of transpiration in ecosystem evapotranspiration. The results of this work partially concur with these studies. In other words, the stem-root flow in the plant-soil system could enhance the transpiration, and reduce the soil evaporation, which regulated the partition of evapotranspiration."

2. Similar to the first comment, but there are lots of model-intercomparison studies (PILPS1,2, GSWP1,2,3, etc.) in land surface schemes. What is significance of this paper among these intercomparison studies? In particular, in PILPS, experiment at HAPEX was conducted (Boone and Wetzal, 1996).

Reply: We cited Boone and Wetzal's paper and added the following comment in the revised manuscript on lines 290-296: "A number of PILPS studies, including the PILPS-HAPEX experiment (Boone and Wetzal, 1996; Henderson-Sellers, 1995; Shao et al., 1996; Xue et al., 1996) consistently demonstrated that the current land model parameterizations have the weakness in simulating the soil moisture in the dry season. This study, by introducing a parameterization on the stem-root flow mechanisms, wish to help solve this deficiency. With the stem-root flow mechanism, the soil moisture will redistribute in vertical, leading to better simulated results in each layer, which is important for the evapotranspiration partition."

3. Detailed experiment specification is missing. What kind of atmospheric data in which time interval is used to run the model? How long is spin-up period? Is the experiment setup typical offline simulation

setup for land surface model?

Reply: Sorry for missed these details. We have revised the text and indicate in lines 117-126 that “Following typical offline simulation procedures for single-column land surface model, in situ atmospheric data were applied to drive the SSiB model in 30 min time resolution. These specified variables include pressure, temperature, humidity, wind speed, net radiation and rainfall. Soil conditions were initialized with each site’s measurement data. Fully coupled land surface model typically require a couple of months to over a year to spin up the model, but the spin up time can be shorter when running in off-line (single column) mode and with good initial soil conditions (de Goncalves et al., 2006; Yang et al., 2011; Lim et al., 2012; Angevine et al., 2014). Our simulations applied measurement data for model initialization, and the results show that the soil conditions reached physical balance within a few weeks. So, at the last 10 months results of our simulations are reliable.” Note that we also repeated the annual simulation initialized with the end results of the original run. The results are very similar except for the first 2 months of the HAPEX case. But the main discrepancies are not caused by differences in spin-up time but rather the underestimation of deep-soil moisture (which lasted to the end of December) in the original run. This is the reason that we chose to use the original run instead of the repeated annual run for discussion (but we do not want to bother the readers with such details).

Minor comments:

1. Equation (3): what is the relationship between q_0 , q_z and q_x ?

Reply: We have indicated in lines 90-91 that “ $q_0 = q_{x,1} + q_{z,1}$ according to the mass conservation principle.”

2. 790L25: I don’t understand “SLR; i.e., q_0 LD”. It’s better to use SLR in Equation(2).

Reply: Sorry for the typo. It should be “i.e., q_0/LD ” (line 164)

3. P11792L24: Isn’t it so obvious that larger the P, stronger the stem-root effect? Throughfall would be stronger too.

Reply: The stem-root flow effects also depend on other conditions such as soil moisture conditions. We simply want to indicate that the difference between LHC and HAPEX is mainly due to rainfall intensity, not other factors. We modified the sentence (line 221) as following to avoid confusion: “This is simply because LHC has more intense rainfall than HAPEX.”

4. P11793L27: Is the vapor density changeable in this experiment setup? Humidity is forced to run the model, right? Some sort of nudging (relaxation) method was applied? Please specify the detail of the method in the method section (see Major 3 comment).

Reply: The specific humidity is read in from the meteorological forcing data, while the canopy air temperature is prognostic, so the vapor density is changeable within the canopy. Nudging was not performed. We have indicated this in the method section (lines 109-110).

5. Figure 4: Too small characters to read. (same as Figure 5-9).

Reply: We have modified these figures with larger fonts.

6. Figure 6: Is air temperature changeable? (Similar to above comment about humidity)

Reply: Canopy-top air temperature is read in from the forcing data every 30 minutes, whereas air temperature within the canopy space is a prognostic variable from the SSiB model. We have indicated this in the method section (lines 109-110).