<u>**Answers to Anonymous Referee #2**</u>

Reviewer's comments are in **bold**.
Author's answers are in regular.
Author's additions/modifications in the text are in *italic*.

<u>Major comments</u>:

1. I believe many land surface models need some parameter tuning work before being comparable to the observations, but we have to avoid tuning some parameters that have some kind of physical meanings. For instance, the authors are trying to tune the minimum and maximum LAI to make better soil moisture simulations (Figs. 6 and 7). The rational is that higher LAI in the summer season will increase the possibility of larger amount of transpiration, and the incepted canopy water that will return to atmosphere more easily than soil evaporation; consequently it will alleviate the model's overestimation of soil moisture. However, the LAI is a physical variable that can be determined by field measurement at local scale or remote sensing at large scale. We cannot treat it as tunable parameters, though the satellite data are not always credible. In the Midwestern United States, the LAI from AVHRR is more accurate than recently MODIS product; where the former has larger summer LAI than the latter, which is preferable to the model that have wet bias of soil moisture. The authors use temperature controlled function to calculate LAI seasonal variation by constraining it between minimum and maximum LAI, I think they are on the right way and consistent with vegetation growth theory; however, tuning minimum and maximum LAI makes the work less plausible. In the future, the authors may need to focus more on tuning the LAI growth function, given the (field or remote) observed minimum and maximum LAI. Therefore, although the SECH2 LAI in Figure 7 is more consistent with reality, it comes from less reliable tuning work. I would like to remind the authors being more cautious with tuning physical parameters.

Thank you for this comment. The maximum LAI is a biological characteristic of a crop in optimum growth conditions. Our LAImax in the model is a potential LAImax for a given PFT. In our model configuration, we agree that there is no direct relationship between the LAI and the vegetation growth. We made this simple choice because we want to introduce and test the parameters which are simple and generic for a given PFT. In the future, we should improve/tune the LAI development using a function of the sum of the air temperature instead of the function of soil temperature. A more consistent way is to activate the module LPJ that simulates the dynamic vegetation (as said in page 5060 lines 12 to 14) in order to represent the link between the aerial part of the plant and the root in the soil.

2. For the comparison in Illinois, are all results based on top 2m soil moisture? If so, I suggest the authors checking the variation of top 1m soil moisture, which is more challenging for many LSMs. For instance, Yuan and Liang (2011, JHM, 10.1175/2010JHM1302.1) shows that LSMs capture the annual cycle of top 2m soil moisture quite well (Fig. 2f), while they perform differently for the top 1m soil (Fig. 2e). In the Illinois case, the top 2m soil moisture variations can be constrained through reasonably unconfined aquifer modeling in terms of baseflow. For the top 1m soil moisture, they are very sensitive to the LAI data (whether AVHRR or MODIS), accurate modeling of shallow water table depths, and the parameterization of hydraulic conductivity (Yuan and Liang, 2011).

Many thanks for this comment. Our soil model is quite simple. The total soil depth in the model is two meters. However, when the rain has just fallen, we differentiate this “upper” layer to simulate a strong evaporation for this water. That is why the depth of this layer is not prescribed and fixed, but depends on the history of precipitation rate. All our results are based on top 2m soil moisture. However, we could have perform a comparison at 1 meter with the observations but this would imply to reprocess soil moisture content outputs of the model.

Specific comments:

3. P5040, L4 in the abstract. What does “mesoscale” of soil moisture mean in the paper? I guess “local scales” is more suitable.

Yes, you are right . « Mesoscale » is removed and we put « local » in the text.

4. A schematic flowchart for the parameterization of ET and root extraction will be useful, and please also highlight the part that is new to the previous version of the model.

Section 2.2.3 is now better organized and the section that describes ET computations is rewritten for more clarity (cf: recommendation 2 of the reviewer 3). So we think that a schematic flowchart is no longer necessary. Moreover, the article includes many figures now (18 figures and 3 tables). So we ask to the reviewer if we can avoid to add another figure.

5. A table describing the differences in parameterization for SECH1 to SECH6 is necessary to complement many figures in the paper.

Table 2 is added in the new version, listing each simulation with its numerical settings.

6. I could not find model spin-up information in the Experimental design section. Since the authors are conducting simulation for a short period (1997-1999), some treatment of spin-up is indispensable.

You are right. Some informations about the spinup are added (page 5051 after line 28 or page 9 line 182-183):
For both US and Illinois simulations, a four-year spin-up has been performed over the same year 1997 to reach a state of equilibrium under the applied forcing.

7. Figure 16, the underestimation of runoff during the summer is very likely related to the parameterization of baseflow. 8. P5059, L15-24, I like such discussion. As I point out in comment #2, the soil moisture modeling in Illinois is sensible to the variation of water table depth, and the parameterization of hydraulic conductivity.

We agree with that but our model, again, is too simple. We mention it in section 3.2.5. at page 5059 : the parametrization of runoff/drainage and the water table representation are not present in the model.