Hydrol. Earth Syst. Sci. Discuss., 9, C1573–C1575, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C1573/2012/
© Author(s) 2012. This work is distributed under
the Creative Commons Attribute 3.0 License.

Hydrology and Earth System Sciences Discussions

Interactive comment on "A comprehensive approach to analyze discrepancies between land surface models and in-situ measurements: a case study over US and Illinois with SECHIBA forced by NLDAS" *by* M. Guimberteau et al.

Anonymous Referee #2

Received and published: 20 May 2012

In the paper, the authors evaluate the sensitivity of soil moisture simulation (e.g., annual cycle and inter-annual variability) to vegetation parameters, bare soil evaporation parameterization in a land surface model. Some uncertainties from meteorological forcing and the estimation of field capacity are also discussion. I think the authors have done a hard modeling work, and I would like to recommend for publication of their paper if they can address my comments below.

Major comments:

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.

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).

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.

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.

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

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 indispensible.

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5039, 2012.