

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 #1

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The manuscript tries to indicate how to tune the parameters in SECHIBA to get more behavioral performance of the model in simulating soil moisture, with improved parameterization scheme and in-situ measured forcing data (e.g. precipitation). Furthermore, in order to improve the simulation, the authors used a phenomenal field capacity (i.e. from in-situ soil moisture profile) to 'adjust' the averaged observation of soil moisture in Illinois. Overall, the manuscript did well in introducing how to tune the SECHIBA step by step to get 'behavioral' results. However, some major revisions should be made

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before publishing:

Major comments: 1. The sensitivity tests of the model to the change of vegetation parameters are done by tuning LAI, crop/grass height and roots density respectively. And, the author claims the pronounced effect of increasing roots density on soil moisture content calculation. As a sink term in soil water balance equation, the root water uptake is assumed to have such big effects (e.g. less water storage). However, one concern raised on such kind of tuning, while considering the plant biology. The root distribution, root density of certain specific plant should have a certain statistical characters with respect to different soil textures. In the manuscript, there is no such kind of description related to what is the relation between the root density and crop/grass height, which will determine the LAI. This may mislead readers to adjust the parameters as they want to get 'good' results, which may create an alien plant that does not exist on Earth.

2.The stations the authors used are from GSMD and include 18 stations which were grass-covered as stated on page 5050 line 20-25. However, on page 5051 line 10-15, the author used Figure 4 to show most of stations are covered by 'C3 crops' instead of 'C3 grass', and pointed out for those stations located in grass land only have less than 10% grass cover. The prevailing type of vegetation is then defined as 'C3 crops'. However, for SECH2 (e.g. use a very high maximal value of LAI for grassland) and SECH3 (e.g. use a reduced height of vegetation which is more realistic to represent a grass cover), it seems to me that the author pay attentions only to grass cover instead of 'C3 crops'. Could the author explain in details about this choice? or Could the author add more discussion about SECH2 & 3 in regards to 'C3 crops'? It would be better to list what are the values pattern of LAI, PFT types, crop/grass height and roots density for different simulations (SECH1, 2, 3 ...etc.).

3.For the uncertainty in rainfall forcing data, the author actually just use the in-situ precipitation data instead of that from NLDAS forcing dataset. It is not appropriate to say the uncertainty in rainfall forcing data was analyzed in this way. The uncertainty

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should account for the reliability of the data (e.g. temporally or spatially), but not just a replace of one dataset by another. This part should be discussed further by using statistics analysis of rainfall data, if the author want to use the same subtitle.

4. For section 3.2.3, the method to get measured field capacity. To my understanding, the author use a kind of 'relative' field capacity (here, the 'relative' is used compared to the original 'absolute' field capacity), or an 'effective'/'behavioral' field capacity (e.g. in regards to improve the model performance indirectly by adjusting the observations). Such kind of adjustment may be acceptable for regional application, where researchers can find plenty of in-situ data. However, it is not applicable for global application, which is the purpose of SECHIBA that has been claimed by the authors as a global Land Surface Model (Page 5060, Line 18). Could the author discuss further this part with respect to the feasibility of field capacity correction in global scale? Is there another way to improve or correct the observation of soil moisture with respect to match the simulation generally?

Minor comments: 1. Page5041 Line 9-12: It is not a smooth statements here. It is disrupting the context actually;

2. Page5041 Line 21 as well as other places in the manuscript: 'eighth degree' → '1/8th';

3. Page5042 Line 2: 'but also' → 'and';

4. Page5042 Line6 as well as other places in the manuscript: 'the atmospheric forcing data set NLDAS', NLDAS is the North-American Land data Assimilation System. It is not a forcing dataset. However, you can say 'NLDAS forcing dataset' instead.

5. Page5043 Line15: what is the resolution for IGBP vegetation map (1/5th degree or 1/8th degree?);

6. Page5046 Line 11: here the FAO soil texture map 's resolution is 1/5th degree. How the author deal with different resolution of the vegetation map, soil texture map and the

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NLDAS forcing dataset in SECHIBA? Please discuss;

7. In the model formulations, there is no description on how to determine the relative dryness of the upper soil layer and lower soil layer. What is the definition for them? What is the physics behind them? How to deal with them in SECHIBA?

8. Page 5050-5051: Please list each simulation running with its numerical settings on used parameterization schemes, used field capacity, PFT types, LAI, crop/grass height and roots density and so on. In table 2, please add PFT types, LAI, crop/grass height and roots density and so on for each station;

9. For SECH4, please indicate clearly which type of vegetation is set everywhere the same;

10. Page 5055 Line 7: ... is not enough pronounced.... → ... is not pronounced enough;

11. Page 5057 Line 13: NLDAS precipitation is lower... → 'lower' should be 'higher';

12. Page 5058 Line 6: we consider that the 0-10cm layer is not representative of the field capacity,. Is it true, even after heavy raining? please discuss;

13. Figure 2, please add P E on the top as equation 5 indicates;

14. Figure 3, is it completely the same as De Rosnay and Picher 1998's paper? if so, please ask for permission from them or the journal published it;

15. legend description or legend name should be provided in the figure for figure 4.

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

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