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Geoscientific Model Development Discussions

Interactive comment on "A hydrological model for root zone water storage simulation on a global scale" *by* Ganquan Mao and Junguo Liu

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This study tries to develop a 'global' hydrological model. The authors are lack of good understanding on hydrological processes, and the methodology they used are not appropriate at all. The authors overclaimed their contribution. The manuscript is poorly written. Some of the figures are not clear. The current manuscript cannot be accepted, and should be returned to the authors to make it a better work.

My comments are as below: 1. The methodology used are not appropriate at all. The authors compared their runoff simulation against some global model simulation and composite runoff data. We all know the global runoff simulation/composite runoff data are designed for global studies, and can have very large uncertainty on each river

basin. They cannot use these data to verify their simulation, especially for a study aiming to develop a 'new model'. Thus, the comparison between the authors' simulation and other runoff data that the authors used means nothing: the authors cannot claim their model is good. The authors should compare their simulation against hydrological gauge observation which is not difficult to collect at all. I doubt the authors' results. They may choose to avoid the comparison against hydrological gauge observation purposely because their model is suffering fatal flaws. For a paper developing a hydrological model, comparison against in situ gauge observation is extremely important. The authors should not skip this step. In addition, the authors compare their runoff simulation after calibrating their model, whereas the other models in ISIMIP are not calibrated. Thus, the comparisons are useless because the models in ISIMIP have large uncertainty which have already been unrevealed in several recent studies by the ISIMIP group (perhaps the authors missed these very important publications).

2. The authors are lack of basic knowledge about remote sensing. The root zone can be more than 10 meters in depth. The sensors used in the NDII studies cannot penetrate the earth ground up to 10 meters, and even one or two meters are suffering large uncertainty because the attenuations of signals with increase in depth. This is why the most state-of-the-art soil moisture products just provide data in the surface 5/10 cm. Thus, the comparison against NDII based data is not appropriate at all. Because this paper is to develop a model, the authors should use in situ observation which is not difficult to collect. I don't understand why the authors choose to skip the comparison against gauge observation.

3. The authors do not have a good understanding on hydrological processes. i). Vegetation plays a vital role in runoff variations especially in densely vegetated regions (e.g., the Amazon, Congon and some regions in the Yangtze, Mekong, Ganges, Mississippi Rivers et.al.) through the transpiration processes. At the leaf and canopy scales, the mechanisms of transpiration are also different. LAI, fPAR, CO2, wind, solar radiation, stomatal conductance all are influencing transpiration. The authors did not consider the

stomatal influence at all (as shown in the Figure 1 and Table 1). Without comprehensively considering the transpiration processes, how the model developed can predict water resources availability, especially many recent studies have unravelled that the earth is greening and CO2 concentration is increasing. Thus, the model developed by the authors has fatal flaws, and this paper cannot be accepted.

ii). The infiltration capacity of soil plays an important role in controlling the volume of surface runoff and subsurface runoff, and also influences root zone water storage. The infiltration capacity of soil is related to soil type, and has clear physical meaning. The authors considered the infiltration as shown in the Figure 1. However, the authors did not report how they determine this important parameter value. If the authors used the values related each soil type, they did not report which soil map distribution data and which hydraulic property datasets of the soil types are used. If the authors calibrated the parameter values, the authors should be aware of that if it is appropriate to calibrate because the results may be wrong after calibrating some parameters with clear physical meaning. The authors are afraid of reporting the calibrated parameter values and the parameter ranges used in the calibration. The authors stated they calibrated their model for good runoff simulation. I am afraid that they calibrated their model for good runoff simulation with the cost of losing the physical meaning of important parameters. Perhaps the authors choose to not show the important information purposely in order to get their paper published. No, absolutely no. The authors have to show which parameters are calibrated, the parameter ranges used in calibration and calibrated parameter values.

4. The model developed is not a global scale model at all. Because the authors did not use soil map and related soil hydraulic parameter values, the use of the model must rely on calibration to determine some of its parameter values on river basin scales. Therefore, it cannot be a global scale model. It is still a river basin scale model, and the authors just applied the model in several large-scale river basins (without any river basins in most of the regions of Canada, Europe, Middle East, Russia, Mongolia). The

used river basins just cover a small proportion of global land surface.

5. The authors claimed they used 2000 iterations to calibrate their model. However, the authors did not explain the reason. Why 2000 iterations were used?

6. The root zone storage variations are related to ground water level dynamics. Did the model simulate the ground water level changes? Please show the simulation results.

7. Please use scientific languages. The sub-titles of Section 2.4 and 2.5 are not appropriate in such as a scientific paper. The statements ‘Fast- and Slow-’ are vague.

8. I agree with the reviewer 1 about the capillary mechanism which is missed by the authors. This indicates the authors are lack of good understanding on hydrological processes from another perspective. When we develop a new model, we try to incorporate new hydrological mechanism to advance our understanding on hydrological processes. However, the authors missed several very important hydrological processes which have already been recognised to be very important. Therefore, the ‘developed’ model cannot provide any new understanding on hydrology to us. I am afraid that the authors just copy other models’ code, delete several important parts, replace a few equations and change computer language used in original code, and then the authors claim they develop a new model. No, this is not the right way to do research. I also wonder why the authors delete the capillary mechanism part from the original code. The authors should realize that they cannot just delete some codes of other’s model, and make it look like a ‘new model’ in order to get the manuscript published. This is not real science. The authors must work hard to consider the capillary and vegetation transpiration mechanisms and using gauge data to validate their simulation. Otherwise, their model cannot be better (based on the physical processes considered) than other hundreds/thousands of models that already exist.

9. The manuscript is poorly written and needs to be largely reworked. There are many typos and grammar mistakes. Many sentences are vague and lack of support. The figures are not clear, e.g., Figure 4 and Figure 5, and one cannot distinguish the lines.

10. Figure 2 is not your result. Please remove Figure 2. Using related references in the manuscript to refer to the data is ok.

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