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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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Review of GMD-2019-52 by Mao and Liu.

First of all, I would like to mention that I find this an important paper. In my view, the authors have convincingly shown that their model is a very valuable addition to the set of existing global hydrological models. Its innovation is that it uses the root zone storage capacity (RZSC) determined independently by remote-sensing-based global products for precipitation and evaporation and does not calibrate obtain it by calibration. As far as I know all other global models either calibrate the RZSC or determine it on the basis of incomplete soil maps and inaccurate maps for rooting depth. The authors using climate-derived RZSC has the intuitive advantage that ecosystems apparently adjust

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their RZSC to climate variability by creating a buffer against dry periods. In hydrological models the RZSC is the key variable determining the partitioning of precipitation into transpiration, recharge and surface runoff, making it the most important hydrological parameter for land-atmosphere interaction and runoff generation. The fact that the authors demonstrated that a remote-sensing-based estimate of the RZSC can be efficiently used in a global hydrological model is nothing less than a breakthrough.

On top of this, the authors demonstrated in their validation that the NDII, a simple remote sensing based proxy for root zone moisture stress, is a powerful tool to validate (and possibly calibrate) global hydrological models. Highly sophisticated satellites claim to monitor soil moisture, with limited success (e.g. SMOS, ERS, and AMSR-E). But NDII, a readily available remote sensing product observing the moisture content of vegetation, apparently can do this better, because it connects to the root zone moisture tension and not the moisture content of the surface. (Sriwongsitanong et al., 2016).

Of course this paper is a modelling paper, and should be treated as such. In that respect I think that the authors should make the code of the model freely available and not merely on request. The model builds on earlier work by Gao et al. (2014a and 2014b), and by Wang-Erlandsson et al. (2014 and 2016), and I think it is fair that the software is freely made available so that other people can advance this approach further. In fact, I think that a more sophisticated evaporation module as in Wang-Erlandsson et al. (2014) could improve the model even further.

Having said this, the paper requires some (major) revision. I shall highlight the major points.

1. The comparison in Figures 4 and 5 is not entirely fair. The models of the ISIMIP2a data set are not calibrated, whereas WAYS is. This is mentioned in the paper, but the comparison in these figures suggests otherwise. The caption should mention this.

2. It is important that the authors indicate which parameters are input independently (from what I can see: S_(rz,max), K_s, f_s, R_(s,max), S_(I,max)) and which are cal-

ibrated (I guess: Beta, K_ff, . . .). The fact that a number of these have been input as independently obtained parameters is crucial information, but we should also know which have been obtained by calibration. It is well known that there is equifinality between Beta and the RZSC, so this is not trivial. I would also want to see a Table with the calibrated values. There should be an openly shared data set with all parameters used, whether obtained by calibration or independently.

3. In my view, the Beta parameter is crucial. It affects the partitioning of precipitation into transpiration and runoff. The time scales K_s, K_ff and K_f merely affect hydrograph shape, but not the water balance. In this regard, it is interesting to know that Gao et al. (2019) developed a HAND-based method to determine Beta from independent topographical information. This method assumes that the dominant mechanism is Saturation Excess Overland Flow and therefore is not applicable on hillslopes. So it should be used with good judgement, but it offers another venue of estimating Beta independently without calibration.

4. I found a mistake in Equation (2). The correct equation should read: P_tf = MAX{0, P_r – (S_imax – S_i)/Delta t }. The Delta t is required to make the equation dimensionally correct and to prevent that if the model is used at another time step, no error is made. The MAX{0,x} operator is essential since P_tf is an overflow. Forgetting the MAX{0,x} operator can lead to negative P_tf values for small amounts of rainfall. This may trigger relatively small errors, but particularly in wet environments (the Amazon?) this can create errors. I fear that the authors have to rerun the models to correct this mistake.

5. In the validation against NDII, one should realise that some ecosystems (particularly Australian) tap into groundwater, so that in those ecosystems the NDII may not be the correct proxy for moisture stress in the root zone during dry periods. This may be another reason why the Murray Darling performs less well in the comparison with NDII.

6. This brings me to another point, that the FLEX model used apparently does not in-

clude capillary rise. In wetlands, this is a dominant mechanism, and also some dryland vegetation is known to tap water from deeper layers. A landscape-based model as developed by Gao et al. (2014a) could cater for this and could also distinguish between an independently derived Beta function for the wetland-terrace-plateau continuum and a calibrated Beta for hillslopes.

7. I don't understand the last sentence in the abstract. Indeed CHIRPS-CSM is limited to lower latitudes, but CRU-SM covers the entire globe. I think the sentence "Therefore, the performance etc." can be deleted.

8. There are many typos. I think the paper requires copyediting, which probably Copernicus can take care of.

So in summary, I think this is an important paper, but additional work needs to be done before the paper can be published.

The references used in this comment also occur in the discussion paper, except the following:

Gao, H., Birkel, C., Hrachowitz, M., Tetzlaff, D., Soulsby, C., and Savenije, H. H. G., 2019. A simple topography-driven and calibration-free runoff generation module, Hydrol. Earth Syst. Sci., 23, 787-809, https://doi.org/10.5194/hess-23-787-2019.

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