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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

Anonymous Referee #3

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General comments

The manuscript presents an interesting extension of the FLEX model with enhanced capability for root zone storage simulation at the global scale. Root zone storage capacity is an Achilles heel in global hydrological modelling that is crucial for determining water stress, but most often dependent on highly uncertain soil and rooting depth data. Thus, the authors are addressing an important issue of high relevance for the hydrological modelling community. However, among other improvement possibilities, I think that the analyses need to be more systematic and rigorous, and the manuscript need to better communicate the motivations underlying the developers' choices. I think the manuscript merits to be published after a major revision. My main concerns are the following:

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- The manuscript could benefit from clearer descriptions of rationale and motivations for the model development, the analyses performed and other choices made. For example, why was runoff selected for evaluation against ERA-Interim/Land and the non-calibrated ISIMIP simulations? Why not use gauged data for a selection of basins and the ERA-Interim/Land and ISIMIP for global gridded comparisons? Were other variables and potentially better datasets considered and rejected for which reasons? How come capillary rise is disregarded on the basis of "lack of information at the global scale" is there are other models that take it into account? Why was Penman-Monteith FAO 56 PM method used (P6L25)? What were the considerations? Etc. Reviewers and readers will always have different views on preferred evaluation datasets and equations, but a clear description of the underlying rationale and motivation could help bridging differences in perspective if choices can be well-justified.

- The analyses could be better designed to facilitate understanding of how and why WAYS perform in certain ways, and thus, give more insight into how various components of the model affect the root zone storage and runoff simulation? For example, can the authors show how results are affected by e.g., use of root zone storage capacity derived from uncertain root depth and soil data versus the root zone storage capacity from Wang-Erlandsson et al., 2016? Can the authors perform some sensitivity analyses to highlight model structure and parameter sensitivity?

- The WAYS model is developed based on essential features of the FLEX model (P3L14), and as such I would (1) suggest the authors to present an overview of the similarities and differences between the two and (2) to retain "FLEX" in the model naming (e.g., FLEX-WAYS). Retaining FLEX in the name benefits the model developers that do not need to explain the model roots and will have an easier time communicating the new model developments that builds on an existing well-established mode, and would also be a nice acknowledgement of the

earlier FLEX model developments. The practice of name roots exists in the modelling community, and e.g., the models LPJmL and LPJ-GUESS show through their names that they share the same roots.

- The WAYS performance evaluation in terms of root zone storage moisture is highly dependent on the comparison with NDII, which weakens the conclusions, since also further work is still needed to robustly establish the relationship between NDII and soil moisture at the global scale. It is after all only recently suggested by Sriwongsitanon et al. (2016) – a study in a river basin in Thailand – that NDVII can have the potential to be used as a proxy for catchment scale root zone storage capacity. The authors could potentially strengthen their conclusions by evaluating model simulation outputs with additional sources of data/methods, such as FLUX-tower, evaporation, EVI etc. Summarizing evaluation figures can be shown in the main manuscript, and others could be included in Supplementary Information. A more detailed list of the equations and calibration process could also be included in Supplementary Information for transparency.

- Wang-Erlandsson et al., 2016 found that normalizing the root zone storage capacity using the Gumbel distribution by land cover type further improves performance, and recommended the use of Gumbel distribution. Please consider applying the Gumbel normalization to the root zone storage capacity data.

- Please consider discussing how and where the results might be influenced by groundwater access and irrigation, noting that the root zone storage capacity in Wang-Erlandsson et al., 2016 was adjusted for irrigation but not access to groundwater, while WAYS do not account for either groundwater or irrigation.

- Please provide the source code, and not only by request.

Specific comments

- P1L10: state what was used for evaluating root zone storage (i.e., NDII) in the abstract.
- P1L10: “many applications”: please provide concrete examples.
- P1L11: “attention needs to also. . .”: hardly the most important limitation, please consider rather listing the more pressing future model developments needs and emphasize the key contribution of this model in comparison to other existing global hydrological models.
- Please point out that Sriwongsitanon et al. (2016) is a study in a river basin in Thailand and not a global study.
- P6L27 “no information is available at the global scale”: Please consider including a few more lines describing the issues related to capillary rise modelling in global scale models and include related references, such as (Vergnes, Decharme, and Habets 2014) and references within.
- P8L28, “it has been well-justified (de Boer-Euser et al., 2019)”: please consider specifying what is justified and add other relevant sources, e.g. “the method has been shown to increase model performance at both basin and global scale (e.g., de Boer-Euser et al., 2016, 2019, Gao et al. 2014, Wang-Erlandsson et al., 2016, Nijzink et al., 2016)".
- P14L11, “reported in his work”: please change to “reported in their work”.
- P22L6 “DNII”, should be NDII.

On a rather different note, please excuse me for taking the opportunity to promote constructive and supportive comments and reviews. Upon reading the short comment by William Chris, I felt urged to stand up for the view that reviews and comments can be both critical and constructive at the same time. Non-constructive comments attack

the person and could look like this "the authors lack basic knowledge about...", while equally critical, but constructive comment would simply address the issue "the authors neglect the fact that NDII only ..., and thus, does not provide an adequate... ". Surely, there are manuscripts that sometimes so lack in substance and show utter disregard for the reviewers' time, but this manuscript is not one of them. I would like to believe that we all - the authors, reviewers, and comments writer - all have poured time and efforts in this because we fundamentally share a love for the science and the science community, so please also let our writing reflect the fact that we are all in this together.

References

Vergnes, J.-P., B. Decharme, and F. Habets. 2014. "Introduction of Groundwater Capillary Rises Using Subgrid Spatial Variability of Topography into the ISBA Land Surface Model." Journal of Geophysical Research: Atmospheres 119(19): 11,065-11,086.

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