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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Global root zone storage capacity from satellite-based evaporation" *by* L. Wang-Erlandsson et al.

Anonymous Referee #1

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OVERVIEW

The manuscript investigates a method for estimating root-zone storage capacity, S_R, by using remote sensing observations. Specifically, satellite-derived evapotranspiration data and gauge/satellite precipitation data are used for estimating S_R on a global scale. The obtained maps of S_R are compared with three previous S_R products and the differences between datasets are analysed. Finally, the new S_R dataset is used in the global hydrological model STEAM for analysing the impact on evapotranspiration

estimation of the new S_R parameterization.

GENERAL COMMENTS

The manuscript is well written and quite clear. The topic is really of interest as the estimation of root-zone storage capacity, S_R, on a global scale would be highly beneficial for modelling and understanding the land-atmosphere interaction with several implications on climate, agriculture, hydrology, etc. The study build on previous studies from the same authors that were made on regional and/or local scale and here analyses the possibility to estimate S_R on a global scale by using satellite data. I believe the paper deserves to be published for the high relevance of the investigated topic. However, in my opinion, several aspects should be improved/changed before the publication. I reported below a list of the general comments to be addressed with also the specification of their relevance.

1) MAJOR: The description of the method should be improved. Is the method the same as in previous studies (e.g., Gao et al. 2014 GRL)? If yes, it should be clearly acknowledged. Is it different from the paper (under review, not available to reviewers) by Boer-Euser et al.? It should be clear to the reader if the novelty of the papers is on the method or in the satellite dataset used as input. Please clarify.

2) MODERATE: In the "Methods" section it reads several assumptions: (i) "irrigation is captured in satellite-based evaporation data", I am not sure it is true. At least, not for all satellite-based datasets, please clarify. Moreover, at page 14 it reads that the evaporation originating from irrigation water simulated by LPJmL is considered. Why if irrigation is already included in the satellite-based evaporation data? (ii) "the long term average is added ... in order to compensate for overestimation of evaporation and underestimation of precipitation". Why? (iii) "in order to take into account of surface

runoff, D never becomes negative". Again, why?

I believe that the authors should better justify the assumption made in their method and why these assumptions are valid (or not). This will allow the reader to understand the strengths and the limitation of the proposed approach.

3) MAJOR: The selection of the input datasets is for me a major issue. Again, it should be clarified why satellite-based data are considered for evaporation and not for precipitation. Why satellite-based datasets for precipitation are not considered (e.g., TMPA, CMORPH, PERSIANN)?

Moreover, why the average of the three evaporation datasets should be "attractive"? Are the results changing by using only one of the datasets? What is the relative impact of the evaporation and precipitation datasets on the final results? It should be clarified, too.

Why ERA-Interim data are used for temporal downscaling? Apart that it is not mentioned how the temporal downscaling is carried out, currently daily evaporation and precipitation datasets are (freely) available (actually, several datasets). Why the authors do selected monthly datasets and then performed downscaling with ERA-Interim? Why not using directly ERA-Interim data? Or other daily products (e.g., GLEAM for evaporation and TMPA for precipitation)? All these points should be clarified.

4) MODERATE: In most of the paper, only the $S_{R_{CRU-SM}}$ dataset is analysed. Why two datasets are considered (CHIRPS and CRU)? The real value of considering also the CHIRPS dataset is not clear to me. Please clarify.

5) MAJOR: I found the selection of the application for validating the obtained S_R maps not correct. In the paper, it is assessed the improvement in estimating evaporation with the new S_R parameterization in STEAM. It is fine for me. The problem is that the same evaporation dataset (E_{SM}) used for computing S_R is also used for assessing the improvements due to the new S_R parameterization. It is a circular argument that

is not good. I suggest performing a different validation test. Why not considering the differences in the runoff prediction with the old and new S_R parameterization? It looks to me much more relevant, and a good independent evaluation.

6) MINOR: A very recent paper with the same topic has been published in Journal of Hydrology by Campos et al. (2016, http://dx.doi.org/10.1016/j.jhydrol.2016.01.023). I suggest mentioning and analysing this study.

In the specific comments, I added some corrections and suggestions that should be implemented.

On this basis, I believe the paper deserves to be published only after a major revision.

SPECIFIC COMMENTS (P: page, L: line or lines)

P10, L13: "The resetting of this limited number of pixels..". Please specify what is the percentage number of pixels for which resetting was needed.

P10, L19-23: Simply say that S_R is the maximum of the obtained D values.

P11, L8-11: This paragraph is not clear to me, please revise.

P12, L10: The C parameter values is set to a value equal to 0.1. Why? What is the impact on the results? Why "C" in equation (7) is different from "c" in equation (6)?

P12, L18: $S_{R,CRU-SM}$ is not defined, only later in the text.

P12, L20: The formulation of equation (8) is wrong for me. The root mean square error should be between $E_{S_{R,STEAM}}$ and E_{SM}, not only for $E_{S_{R,STEAM}}$ or E_{SM}. Please reformulate.

P13, L16-17 The symbols $sigma_n$ and $sigma_{S_R}$ are missing in the text. Please correct.

P16, L13: ERA-I evaporation is used as forcing of STEAM. What is the output of STEAM? Is it the actual evaporation? It should be clarified and clearly distinguished from potential evapotranspiration throughout the text.

P16, L27-28: The methods used for downscaling/upscaling the different datasets should be described.

P17, L12: "S_R estimated are generally larger", larger than? Please clarify.

P19, L22-P20, L8: Too many details are given here for the description of the differences of the simulated evaporation data. It is difficult to follow, please reduce the text focusing on the most relevant differences.

P22, L20: Recent studies have obtained huge differences between global scale precipitation datasets (e.g., *Trenberth et al. (2014)*, *Herold et al. (2016)*). It seems not true that evaporation data (on a global scale) have larger spread than precipitation data. Please reformulate.

P25, L13: It seems that "and $S_{R_{CRU-SM}}$" is missing here.

Tables/Figures: Please check captions for symbols. Captions should be self-describing.

REFERENCES

Herold, N., et al. "How much does it rain over land?" Geophysical Research Letters in press (2016). DOI: 10.1002/2015GL066615.

Trenberth, K.E., et al. "Global warming and changes in drought." Nature Climate Change 4.1 (2014): 17-22.

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