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

Anonymous Referee #2

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The manuscript "Global root zone storage capacity from satellite-based evaporation" from Wang-Erlandsson et al, submitted to Hydrology and Earth System Sciences, describes an interesting approach of filling a gap in global-scale water modelling by developing a method as well as a dataset for global-scale root zone storage capacity. The study is based on the assumption that plants are optimizing their roots to drought conditions (with a given return period). Based on the cumulative difference of the sum of (satellite and ground observation based) precipitation and modelled irrigation and two to three MODIS based evaporation products, the authors used the largest "drought" (soil moisture deficit) condition to assess the root zone storage capacity. Furthermore, they used the Gumbel-approach to assess vegetation types with longer return periods than the data provides as well for a sensitivity analysis. The resulting product is compared to different types of previous work as well as used in a model experiment.

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Discussion paper

First of all, the paper is well written, well structured and I really like the review of methods to estimate the root zone capacity. I think the overall approach can contribute in better quantifying worlds water resources with global model approaches. Anyhow, there are some (mostly minor) drawbacks that should be improved before publishing.

General comments

The beginning of Sect. 2.1 should be revised. It refers to a figure which is (for me) not really self-explaining. Also, I had problems while reading Page 9, line 10 “P and the evaporation originating form irrigation”. I asked myself why is this evaporation not included in the F_out term. I suggest to write something like “the amount of effective irrigation water (that is evapotranspired by the crops)”.

At Section 4.3 the authors assess the improvement of the new root zone storage capacity information within the STEAM model by comparing it to the same product with which the root zone storage capacity was developed. I wonder if this is an independent benchmark product or if you should use E_CSM instead (or only E_C) as benchmark. Sure, there is a lack of real observation based benchmark products for evaporation, but this is a weak point. You should select a different benchmark or rephrase the term benchmark.

Most of the figures are very tiny, and sometimes due to the choice of color very hard to read (e.g. Fig. A1). Please take care of figure size in the final production phase of the manuscript if it is accepted.

Specific comments

I suggest changing the first word of the title to Global-scale. That is more reflecting the issue, that the product is done for global land surface (but one can also live with just “global”)

Page 6, line 23: I would recommend a new section starting with “For global” or revising the first sentence. The last sentence of the section are general statements of model

calibration, but to my knowledge, none of the models described here is calibrated directly on root zone storage capacity as it is written in the first sentence. Please revise.

Page 8, line 2: you should change “stores” to “storages” as this is more common term in that sense

Page 8, line 10: change “global” by “global-scale”

Page 9, line 15 f: strange sentence. Why should one understand that irrigation is included in precipitation data? Or do you refer to satellite precipitation data? This sentence needs to be revised

Page 10, line 13: please be precise and present the number of pixels that are affected.

Page 11, line 21: a dot at end of section is missing.

Page 13,line 3: I wonder how many of the 1.5 deg grid cells are available for each land cover class, if land cover needs to be at least 90% of a single land cover. Maybe I have misinterpreted the information, but is it correct that you used MODIS land cover with 0.05° resolution to assess land cover for the 1.5° cell? So, the 1.5°cell consists of 90 0.05° tiles, and at least 81 of them needs to be in one land cover class. The global pattern of land cover is very heterogeneous and think it is important for interpreting the results if you write (e.g. in a table) the number of grid cells per land cover class that went into that analysis.

Page 15, line 8 ff: please describe at least in some sentences the temporal downscaling of monthly evaporation and precipitation data.

Page 16, line 24: again, it would be nice to have an idea, how many grid cells are used per land cover class.

Page 20, line 14 f: Hard to judge that because de Boer-Euser is not available for the reviewers. Maybe you should write in a few sentences what is written there.

Page 21, line 14 f: The term “speculate” is not nice in a paper. Is there any reference

for the message “deciduous forests need a large root zone storage capacity to cater for dry periods during their most active summer months” available or is it something you found out during your research? Please try to verify this. Same for the next sentence.

Page 23, line 2: after “from” can be misinterpreted and is not complete. I suggest to write: “. . .from remotely sensed evaporation, remotely sensed and station based precipitation and model based irrigation. . .”

Page 23, line 7: did you compare SR,chirps-CSM with E_SM or with E_CSM? Is that conclusion somewhere covered in the paper?

References, general: why are the page numbers written after the reference? Is this the new standard of HESS?

Page 28, line 12: belongs “Open Access” really to the journal title?

Page 31, line 3: Please check the citation. It is a master thesis, and I am not sure if there are so many co-authors.

Page 31, line 22: Check names

Page 32, line 4: does Jennings really have CMHCMH as initials? I tried to get access but that failed. Could you maybe update the resource?

Page 33, line 24: soil should be in lower case

Page 35, line 8: check initials from last co-author

Page 40, Fig 3: For Africans desert region, the CRU-SM product has obviously values of 0, whereas the CHIRPS-CSM product is > 0, and if I see it correctly at Fig 3c, it has reasonable large values. Could you explain somewhere, where the difference comes from - is it due to precip product or due to additional evaporation product?

Page 44, Fig 7: please write after aridity index that the calculation can be found in Sect B1.

Page 45, Fig. 8: Due to the legend, both subfigures does not have the same width. Please try this for better comparability.

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