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Discussions

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

L. Wang-Erlandsson et al.

lan.wang@su.se

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We are grateful for the constructive comments, and would here like to briefly respond to Referee #2's specific comments. Below, referee #2's specific comments are in bold and our responses are in upright font. We refer to the manuscript for explanations of variables and abbreviations.

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")

"Global" is succinct and also common in other similar context, e.g., "Global Soil Map" and "Global Land Cover". For these reasons, we intend to keep "global" in the title.

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.

It's true that not all of the mentioned studies calibrated directly on root zone storage capacity. Instead, they calibrated various both physically-based and conceptual parameters (sometimes related to root zone storage capacity). We will replace the sentence at P6 L23-27:

"For global hydrological models, calibration has mostly been performed separately for a selection of large river basins and transferred to other regions using a regionalisation approach (Güntner, 2008; Hanasaki et al., 2008; Hunger and Döll, 2008; Nijssen et al., 2001; Werth and Güntner, 2010; Widén-Nilsson et al., 2007)."

with the following:

"For global hydrological models, parameters can be calibrated separately for a selection of gauged river basins and transferred to neighbouring ungauged catchments (Döll et al., 2003; Güntner, 2008; Hunger and Döll, 2008; Nijssen et al., 2001; Widén-Nilsson et al., 2007) . This procedure, known as regionalisation, has (to our knowledge) only been performed for other parameter values than the root zone storage capacity, although the principle does not change with the parameters tuned."

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

We will change "stores" to "storages" here.

Page 8, line 10: change "global" by "global-scale"

"Global" and "global-scale" are both grammatically correct here. Since we kept "global" in the title, we intend to also stick to "global" in this phrase.

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

At L15, it reads “This is because irrigation is captured in satellite-based evaporation data, but obviously not in precipitation data”. We never claimed that irrigation is included in the precipitation data. It is part of the incoming flux, but not part of the precipitation. Therefore we are not sure what the referee means by “Why should one understand that irrigation is included in precipitation data?”.

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

The statement on D reset is an error that slipped through. The resetting was first introduced when we allowed accumulation to persist for two years, but we later changed this threshold to three years. In fact, we do not reset any grid cells when the threshold of persistent D accumulation years is set to three years. We will delete the sentences “In addition, D is reset to zero by the end of a three years period in a few grid cells where D accumulation persist for three years or more. Such increases are likely the effect of lateral supply of water, or reflect erroneous combinations of P and E. The resetting of this limited number of pixels does not affect the outcome of this study in any measureable way.”

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

We will correct this.

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.

We will provide information on the number of grid cells per land cover class in the revised manuscript.

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

We downscaled monthly evaporation and precipitation by applying the daily-to-monthly ratios derived from ERA-Interim E and P data. We will added the following:

“In the temporal downscaling, we first established the ratios between daily values to the mean monthly ERA-Interim, and second, used the relationship to estimate daily values from monthly E_{SM} or E_{CSM} values.”

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

We will add this to the revised manuscript.

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.

De Boer-Euser et. al. is now published. We will update the reference.

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.

The coping mechanism of deciduous forest is to shed leaves when there is too much moisture stress. In semi-arid tropical climates we can see this happen when the wet season comes to an early end; likewise we see this in temperate climates when the cold season starts early. However, shedding the leaves during the wet season (semi-arid tropics) or the growing season (summer in temperate climates) is not attractive because it prevents reproduction. So the only option to prevent lack of reproduction is to create adequate storage. This storage needs to be big enough so as to reach reproductive age.

Therefore, it also depends on the age of the species. An annual species only needs to cater for an average year (or somewhat drier than average year), particularly if there is a seed stock that can survive drought, or if the plant can go dormant (grasses). So age also comes into the equation. In the analysis, the "deciduous forest" constitute of both "deciduous needleleaf forest (DNF)" and "deciduous broadleaf forest (DBF)". We lumped the needleleaf and broadleaf to prevent statistical artefact due to a low number of grid cells covered (i.e., only 13 grid cells of deciduous broadleaf forest and 31 grid cells of DNF, see figure below). However, this concealed an effect that probably is real – i.e., that DNF performs the best with a very long return period, whereas DBF performs the best with a shorter return period.

It appears that the age structure between DNF and DBF are radically different. Global mapping of forest age structure using country database and MODIS data (Poulter, 2012) show that DNF have a very high age, whereas large areas of DBF are notoriously young. The majority of the forests were > 80 years in the region (i.e. Siberia) with highest concentration of DNF (i.e., where our analysis covered due to the 90 % single land use occupancy requirement). By contrast, 1-30 years and 31-80 years were far more common in the regions with high DBF concentrations (i.e., in Easter U.S. and South America). Based on U.S. forest inventories, (Hicke et al., 2007) report mean forest age of 60-100 years, with hardwood (incl. DBF) younger than softwood (incl. DNF). In terms of longevity, it also seems that gymnosperms (incl. DNF) beat the angiosperms (incl. DBF). The mean maximum age of 60 species of reported North American angiospermous trees is 248 years, whereas it is 596 years for the 50 species

of gymnospermous trees (Larson, 2001; Loehle, 1988). Longevity could be explained by strong defence mechanisms against fungi and insects, lack of physical environmental damage, but also low occurrence of environmental stress such as drought (Larson, 2001). Thus, we think it could be correct that deciduous needleleaf forests, which is older, also have root zone storage capacities that have catered for longer drought return periods.

In the revised manuscript, we will discard the lumping of needleleaf and broadleaf, and instead report all forest types separately together with grid cell numbers. We will delete the wording "speculate" and the possible explanation of poor snow simulation in STEAM. Instead, we will explain the result by relating it to studies of forest age.

(We changed the soil moisture stress function in STEAM, see response to referee #1's specific comments and Axel Kleidon. This changed the best return period for mixed forest, woody savannah, savannah and evergreen broadleaf forest, but does not affect the deciduous forests and the discussion above.)

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..."

We agree it's not complete, but the full sentence is also cumbersome to read. In principal, the method used in the paper is for use with remotely sensed data. Irrigation only affects the irrigated regions in the world. We will revise to the following:

"This study presents a method to estimate root zone storage capacity in principle from remotely sensed evaporation and observation-based precipitation data, by assuming that plants do not invest more in their roots than necessary to bridge a dry period."

Page 23, line 7: did you compare $S_{\mathbf{R,CHIRPS\text{-}CSM}}$ with $E_{\mathbf{SM}}$ or with $E_{\mathbf{CSM}}$? Is that conclusion somewhere covered in the paper?

Yes, see Fig. S4 in the Supplementary Information. The results are discussed in the

Supplementary Information Sect. 1.3.

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

We will ask the typesetter at Copernicus Publications for the re-submission.

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

The journal is Remote Sensing. We will correct this reference.

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.

The committee members were erroneously listed as co-authors. We will correct this in the revised manuscript.

Page 31, line 22: Check names

We will correct the capitalising of author 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?

It should be CMH once. We will correct this in the revised manuscript.

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

We will correct this.

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

We will correct the last co-author initials to Xu, C-Y.

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?

This comes from the CMRSET evaporation product. We will add figures showing the S_{R} as combination of the separate E and P products in the Supplementary Information, and this will become apparent. Also added an explanation in Sect. 4.3:

"The positive values of $S_{\mathrm{R,CHIRPS-CSM}}$ in the Sahara desert are caused by overestimation of evaporation in the CMRSET evaporation product, (see also the Supplement)."

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

We will add this.

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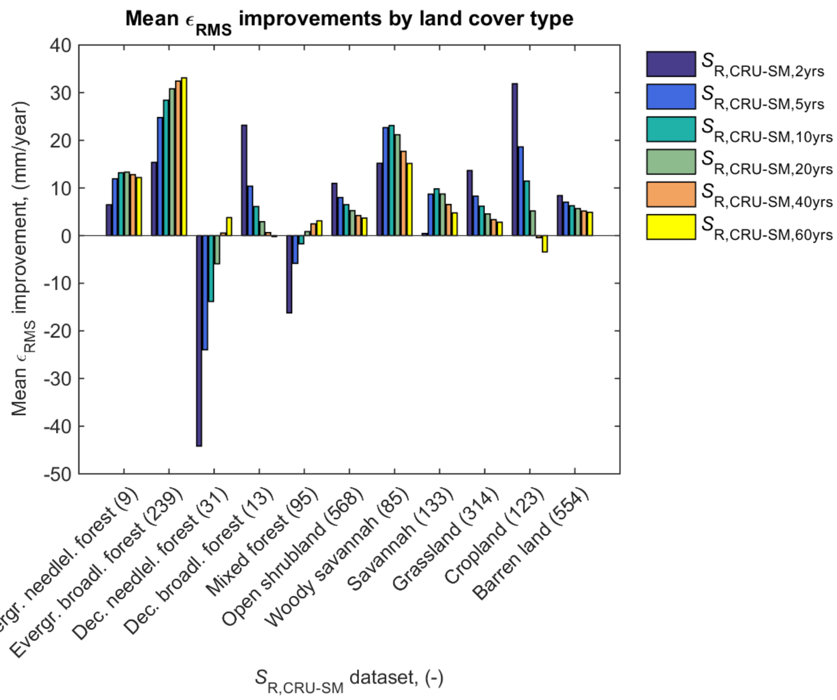

Fig. 1. The mean RMSE improvement in simulated monthly E (2003–2013) by implementing SR,CRU−SM,Lyrs instead of SR,STEAM in STEAM. The number of represented grid cells are in parenthesis.