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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Inter-annual variability of the global terrestrial water cycle" *by* Dongqin Yin et al.

Rene Orth (Referee)

rene.orth@bgc-jena.mpg.de

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Review of Dongqin and Roderick "Inter-annual variability of the global terrestrial cycle"

This study investigates the propagation of precipitation variability into the water cycle, i.e. into variations of runoff, evapotranspiration, and of storage changes. The authors show that this is mostly controlled by temperature (in wet regions), long-term aridity (in transitional regions), and by soil water storage capacity (in dry regions). Further, the results illustrate that the corresponding partitioning is different from the partitioning of mean precipitation into the means of these water cycle variables.

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Recommendation: I think the paper requires major revisions.

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Discussion paper

The analysis is very interesting and provides new and fundamental insights into large-scale land surface hydrology. Related variability analyses are still not commonly done due to a lack of reliable data and underlying theory. This study can foster theory development in this area, and it underlines the importance of continuous improvement of the just-emerging global hydrological re-analysis datasets. Therefore I would be happy to see it published in HESS, but after some general revisions:

(1) Next to the consideration of the soil water storage capacity and the mean temperature to explain variations in the partitioning of precipitation variability, I am missing the inclusion of vegetation type as an explanatory variable. It might have strong implications on evapotranspiration variability, and therefore also on runoff and storage variabilities.

(2) I agree with the authors that comprehensive hydrological reanalysis datasets are lacking, and the CDR dataset is an important contribution in that respect. Further, I appreciate the effort they make to validate the applicability of the dataset in the context of this study. However, also the CDR dataset is (necessarily) based on a model and hence it is not clear that the reported relationships are operating in nature, and not only in this model. To address this issue, I would like to see the key analyses from this study repeated with the state-of-the-art ERA5 reanalysis, which should be superior to ERA-Interim also in terms of land surface representation.

(3) I appreciate the idea of investigating the influence of the soil water storage capacity and the mean temperature on the variability partitioning. However, I think parts of the conclusions drawn by the authors from Figures 8-10 are not supported by the data. For example, I cannot see in Figure 10 that the temperature influence is particularly strong in very wet regions. Rather, to me it seems to be strong in moderately wet and dry regions (Fig 10b,d,f,h,j,l,n,p). Further, also the aridity limit of 6 which the authors suggest in their interpretation of the results in Figure 9, is arbitrary and not supported by the actual results. Storage capacity is obviously having an influence already for aridity values above 2-3 (Fig. 9b,c,f,j,k). Overall, in these Figures there are many inter-

esting patterns but the authors focus only on few sub-plots and limit their interpretation to these. Therefore, I suggest to either show less information/sub-plots there, or to develop explanations also for patterns emerging within other sub-plots.

(4) The paper contains (too) many figures, which is diluting the main message(s), I feel. For example, Figures 1 and 2 could be merged, Figure 5 could be moved to the supplementary material, Figure 13 could be merged into Figure 8. The authors might have further ideas to reduce the amount of figures. Moreover, I do not really understand the difference between Figures 7 and 8, and why both are needed.

I do not wish to remain anonymous - René Orth.

Specific comments:

line 8: Equation 2 not introduced yet

line 13: It should be 'variabilities'.

line 15: Some word is missing towards the end of the line

lines 35-39: Orth & Destouni (2018) might be relevant in this context and could be cited.

line 37: Not sure I get the point here.

lines 106-118: Please clarify that what you are determining here is actually not the soil water storage capacity, but rather the active range within which the soil moisture varies.

lines 157-163: I would recommend to replace the LandFluxEVAL and the Jung et al. datasets with more recent gridded ET datasets such as the Jung et al. 2019 dataset and the GLEAM dataset (Martens et al. 2017).

line 180: Gudmundsson et al. (2016) might be relevant in this context and could be cited.

line 181: What is meant by seasonality here? I thought you are considering annual data? In general, I think the considered temporal and spatial scales and resolution need to be more clearly stated and motivated at the beginning of the manuscript. Also, the role of these decisions on the results could be discussed.

line 252/253: I could not find this discussion in section 5!? Would be important to explain these discrepancies, though.

line 327 & 333: 'leaving very limited variance' - not really true given your statement in lines 385-387

lines 403-405: I cannot see this from Figure 8.

Section 5: Overall a bit lengthy with too much summarizing, I think. Could be shorter, and more concise.

Figure 3: Why are there data points outside the physically plausible range?

Figure 4: Many values seem to be cut at 10 as this is the end of the color bar. You could use log scale here for the color bar.

References:

Gudmundsson, L., P. Greve, and S. I. Seneviratne, 2016: The sensitivity of water availability to changes in the aridity index and other factorsâĂŤA probabilistic analysis in the Budyko space, Geophys. Res. Lett. 43 (13), 6985-6994.

Jung, M., S. Koirala, U. Weber, K. Ichii, F. Gans, G. Camps-Valls, D. Papale, C. Schwalm, G. Tramontana, and M. Reichstein, 2019: The FLUXCOM ensemble of global land-atmosphere energy fluxes. Scientific Data, 6 (74).

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