

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/hess-2022-190-RC2>, 2022
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Comment on hess-2022-190

Yannis Markonis (Referee)

Referee comment on "Global evaluation of the "dry gets drier, and wet gets wetter" paradigm from a terrestrial water storage change perspective" by Jinghua Xiong et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-190-RC2>, 2022

General Comments

The manuscript examines the dry gets drier and wet gets wetter (DDWW) paradigm from a water storage perspective over the terrestrial fraction of the water cycle. The topic is contemplated within the scope of *Hydrology and Earth System Sciences* via the study of the spatial and temporal characteristics of global water resources and would be of interest to its readership. It is a topic that remains under debate in the last decade and studies that bring new evidence, such as this, can substantially impact the ongoing research. The analysis based on the terrestrial water storage drought severity index is reasonable and aims to quantify wet/dry regime trends under different warming scenarios and assess the agreement or rebuttal of the DDWW paradigm. The study has clear research hypothesis and objectives, that are reflected in the results, and help us increase our understanding of the changes in the global water cycle. However, there are some important aspects that should be addressed before being considered for publication.

Before moving to the specific revision comments, it is important to note the lack of consistency across the hydrologic and climatic communities in the use of the terms "wet/wetter" and "dry/drier", which in some cases can be misleading (Roth et al. 2021). Thus, it is easy to misinterpret whether a variable is truly appropriate for describing wetting or drying over a region. In this study, a variable (terrestrial water storage) which is not directly involved in the formulation of DDWW paradigm (precipitation/evaporation) is used to validate the paradigm itself. This raises concerns about the applicability of terrestrial water storage as a metric that can confirm or falsify the DDWW hypothesis. The study needs to convincingly prove this. A feasible way to achieve it would be to compare the current findings with $P - E$, taken from the models (GHMs, LHMs, and CMIP6). This also holds the opportunity to highlight the mechanisms involved in the observed changes and/or pinpoint the biases in the models. Some caution should be taken here since the

comparison should also consider surface runoff to satisfy the budget closure. In any case, though, this can help to bridge the different methodologies and explore their complementarity.

Specific Comments

Lines 1-2: The term "re-examination" should be reconsidered and perhaps be replaced with something like "alternative/complementary examination".

Lines 25-27: It would be more appropriate to reference the original study of Held and Soden (2006).

Lines 34-42: You might want to look at the work of Roderick et al. (2014).

Lines 43-74: This paragraph could be split into two (line 56 perhaps?) to improve readability.

Line 84 (Table 1): Since the three GRACE reconstructions come from two papers it could help the readers if they were also somehow distinguished in the table.

Lines 84-85: Averaging the datasets always comes with certain challenges. For example, in this case we would expect that the three reconstructions of GRACE are strongly correlated and thus their impact to the estimation of the mean would be stronger. A cross-correlation matrix between the datasets would help to assess the magnitude of the impact and comment on it in the manuscript.

Lines 87-88: Can you please provide some information about the percentage of the missing values, as well as their distribution among the number of consecutive missing values?

Lines 107-108: "kinds" might not be necessary here.

Lines 167-168: Please also cite the original paper of Hempel et al. (2013) which has been used by Xiong et al. (2022).

Lines 177-178: Before applying linear interpolation and assessing the statistical significance of the slope the auto-correlation structure of the time series should be investigated. High values of auto-correlation coefficient could result to biased estimates of t , so if this is the case, alternative methods of slope significance should be applied (Hamed and Rao, 1998; Yue et al. 2002). In addition, the reference to the work of Greve et al. (2014) should be revisited as a different statistical test is applied at that study than the t-test.

Line 180: Should the region be considered as "uncertain" or should it be considered a region with no or non-significant long-term change?

Lines 183-184: It is preferable to keep a single tense for the whole manuscript (past/present). This comment also applies for other lines.

Line 185: Citation of the report is missing.

Lines 190-208: My understanding after reading the bias-correction method of Xiong et al. (2022) is that that the CMIP6 ensemble was bias-corrected using GRACE TWSA. If this holds true, then the evaluation of the TWSA derived from the ensemble mean of CMIP6

raises some questions about its validity and therefore NRMSE is lower compared to DATASET.

Lines 214-216: Is there any likely explanation about the increase in the range of DATASET after 2010? Perhaps it can be linked to the decline of TWSA of a specific dataset.

Line 219: It would be helpful to remind the readers that for the historical period DATASET is used, and not only the CMIP6 data that are used for the scenarios.

Line 221: Since you are referring to the slope "/a" is redundant. Still if you would like to keep it, you could consider replacing it with "/yr".

Line 228: I think "increasing" is the correct word here.

Lines 228-229: There is no study related to S. Europe in the references. Most importantly, in Figure 1b it appears that SE. Europe is wet and the rest of the south not significantly drier. This contradicts older studies reporting a drying trend over the Mediterranean (e.g., Hoerling et al. 2012) and could shed new light to the ongoing discussion about the current and future conditions of S. Europe, so I would recommend elaborating more.

Lines 233-235: Do you mean that your results for these regions disagree with the previous studies? If yes, you could clarify a bit and discuss potential reasons for the disagreement. Also, you might want to replace "alternatively" with "on the contrary".

Line 236: In SSP126 scenario, S. Europe also has a strong wetting trend.

Lines 236-245: This paragraph discusses only the SSP126 scenario, while the other two more probable scenarios remain uncommented. It would be nice to discuss the differences between each projection scenario and highlight the regions that all scenarios agree. Another striking difference appears in spatial clustering between the historical period and the model results in terms. It is evident that in the historical period there is stronger spatial homogeneity, while the models replicate this behavior only for SSP126 scenario. Any idea why this is happening?

Line 245: You could consider rephrasing to "a pattern also considered".

Lines 246-259: It is not very clear what the investigation of the changes over the SREX regions offers to the study.

Line 260: The legend is not very clear (some spaces between the numbers would help). Also, the scale order should be from higher to lower. Since the stippling marks are not very clear, could you please remove them and reproduce this map with only the statistically significant slopes in the supplementary material?

Line 266: Same concerns here for the legend as the previous comment on Figure 1. The bar plot needs also revising as D should be above D ($p < 0.05$). Additionally, I am not certain that the pie plots help the readers and are not discussed in the manuscript. You might want to consider removing them and adding them as a separate figure in the supplementary material.

Lines 273-276: These lines would fit better to the Methods section.

Line 274: Please elaborate about transitional regions.

Line 290: You might want to remove "Under climate change" since you mention the SSP126 scenario.

Lines 295-304: Again, I have similar concerns about SREX regions as the ones for lines 246-259. If you decide to keep them and justify the added value they offer in the analysis, please consider presenting these results in an individual paragraph.

Line 319: "In climate model projections", would be more appropriate than "Under climate change".

Line 336: Please see the comment about Figure 2 (Line 266) regarding the pie charts.

Line 343: I would recommend using "Non-significant" instead of "Uncertain" here.

Line 348: It would be helpful to the readers to link the limitations with some suggestions for future research, especially for the first two paragraphs.

Lines 379-385: Similarly to lines 190-208, bias-correction comes with certain limitations which need to be mentioned here.

Line 382: A minor typo here "bias correction".

Lines 403-407: It would be preferable to present the differences to the 0.05 threshold both in text and Figure S13.

Lines 421-424: A quite strong statement appears here. Are there any other studies that support it or is it derived only by the results of this study?

Line 457: Another minor typo "is still challenged".

Line 463: It would be very beneficial to the community to share the data used in the manuscript figures, as well as the DATASET and bias-corrected CMIP6 members. This will have a positive impact on the study itself, as it will improve its reproducibility.

References

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