

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1  
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## Comment on hess-2021-645

Anonymous Referee #1

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Referee comment on "A reexamination of the dry gets drier and wet gets wetter paradigm over global land: insight from terrestrial water storage changes" by Jinghua Xiong et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-645-RC1>, 2022

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General comments:

This study reexamine the "dry gets drier and wet gets wetter" (DDWW) paradigm from the perspective of terrestrial water storage anomaly (TWSA) using a large ensemble of GRACE reconstructions, global hydrological models, and land surface modes. Based on the proportional percentages of different patterns, the results showed the consistent/opposite pattern with the DDWW and then the authors claimed that the paradigm faces challenge in both history (1985-2014) and future (2071-2100).

The topic is interesting and this study potentially provide a new perspective. However, I do not see the methods are convincing and the results are robust. First of all, I want to say that the dryness/wetness change itself contains different models, so it is not surprising to find the change models that do not follow the "DDWW" paradigm on a global scale.

(1) My largest concerns are: Can GRACE observed TWS be used to estimate land surface dryness/wetness trends? How well (sensitive) can TWSA represent long-term trends in dryness/wetness across land surfaces? Is it better than traditional drought indices (e.g., the SPEI, PDSI or other methods)? There is no authoritative study demonstrating the suitability and applicability of the GRACE observed TWS in capturing surface dryness/wetness trends, especially on a global scale. Please note that, generally, the GRACE observed TWSA is applied to monitor changes in groundwater, land-ice evolution, and drought/flood events which occur on a short-term scale (see References). Hydrological processes are complex, but indices are often based on a relatively simple calculation. I take an example to show my understanding here. In glacier-covered mountains, as the climate warms, ice/glaciers are degrading with an increase in runoff/soil moisture (moisten the land surface). Meanwhile, as the mass decreases (water flows away), what GRACE observes is a decrease trend in gravity (drying). TWSA estimated trend and the real surface dry/wet trend can be absolutely opposite. Thus, changes in TWS do not equal to changes in surface dryness/wetness. Right?

Let's continue this topic and look at the Figure 3a. Over the past few decades, glacier melting and increasing runoff/wet trend in the southwest of Tibetan Plateau have been reported (e.g., the Fig. 4b of Yang et al., 2019), but the TWSA detect a drying trend in historic period. This clearly shows that the use of TWSA to estimate surface dry/wet trend is not robust.

In addition, terrestrial water storage anomaly contains the information of changes in groundwater. With increasing human activity, large-scale pumping reduces groundwater (i.e., TWS observed a decrease trend) whereas the groundwater pumping and agricultural irrigation can moisten the land surface. I feel that the subtle effects of pumping and agricultural irrigation on dryness/wetness changes also cannot be captured by the TWSA. Therefore, I cannot confirm how valuable the perspective proposed by the author is for the capture of surface dry/wet changes.

(2) My another question is why the authors confirm that an ensemble way is more reliable than a single way? This draft does not show the individual results of different methods, nor does it compare the differences in these results, so I can't be sure that the way of ensemble is reliable. In the Figure S2, there are gaps between gravity satellite observations and climate model simulations. Besides, why the authors use the GRACE observation to correct the CMIP6 historical simulation? Do you think the simulation of CMIP6 is unreliable (relative to GRACE)? Why? How to define and calculate the TWS in hydrological models, CMIP6, and land surface models? Are they talking about the same thing (and same with the GRACE's TWS)? How different are the estimated TWS between these methods? I don't think simply integrating the various outputs is a right path, because of the inherent scale differences between climate models, hydrological models, and satellite observations, and I think the TWS in these methods is not the same object.

(3) I found a fault in the fundamental calculation. The presented area percentages are calculated by the number of grids, which are not the real area of the Earth sphere. Such calculation can greatly reduce the proportion in the tropics, but we think the "wet-wetter" paradigm is generally well followed there.

(4) The titles of section 3.1 and section 3.2 are the same, i.e., "Global trends of dryness and wetness". How rough! Despite an admirable effort by the authors to process data and conduct calculations, the manuscript lacks discussion and more is showing calculation results. Uncertainties regarding to the new methods and results should be fully discussed.

For the above reasons, I do not support its publication in the HESS. There are also minor issues in this manuscript (see below).

## Specific comments:

- Line 13-14: Why the sum of the patterns is 27.1% plus 22.4% (not 100%)? What about other patterns?
- Line 20: What's the meaning of "fresh availability"?
- Line 25: What do you mean "enhance"? What do you mean "vice versa"?
- Line 26-27: "in hydrologic cycle under climate change in both regional and global scales". Is this expression a bit exaggerated?
- Line 29-30: "rational"-->"rationale". Do so many references really question the rationale of DDWW?
- Line 40-41: "The uncertainties within previous studies are mainly sourced from different choices of measures and datasets". However, this study do not reduce such uncertainties, and there are also great uncertainties, as there are various data sources and interpolation methods.
- Line 45: It is true that "neglect the hydrological process on the land surface", but the TWSA used for estimating dryness/wetness is also an index and neglect the hydrological process.
- Line 47: "merely highlight differently single aspect of the water cycle, lacking the complete representation of the terrestrial water storage (TWS) ". Why do you think a complete representation of TWS would be better than a index regarding single aspect of water cycle? I think there are already comprehensive drought/wet indices.
- Line 50: "TWS consisting of water storage in surface water, soil moisture, groundwater, snow and ice, and canopies can physically provide integrated information..." But groundwater pumping reduces groundwater (TWS is decrease) and makes the surface wet.
- Line 93: What's the meaning of offline physically based?
- Is it necessary to carry out regional studies according to the IPCC? The zoning studies make no sense in fact. They are just another display for the same results.
- The conclusion section is not well written. What new things the manuscript provide? It is recommended to summarize from two aspects: method and finding. How well does the new method/perspective works and what is the scientific value of the results in this study?
- Figure 1: Which method was used to calculate the slopes in the left panel? Which method was used to analyze the significance of trends? Which level?
- Figure 2: What does the fan shape in the map means?
- Figure 4: I cannot figure the fan shapes and their meaning clearly.