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Interactive comment on "Estimation of hydrological drought recovery based on GRACE water storage deficit" *by* Alka Singh et al.

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Summary: the authors examine two different ways to estimate drought recovery: a storage deficit approach, in which GRACE TWSA is used to define the end of a drought, and a "required precipitation" approach that tracks (or forecasts) cumulative rainfall deficit. They conclude that there is good agreement between the two methods in most regions that satisfy tests of moderate or strong rainfall-storage coupling. Bringing these two methods together is both interesting and potentially valuable in the context of forecasts–presumably, for regions in which this analysis approach works well, a skillful precipitation forecast could be used to predict the cessation of TWSA drought up to several months in advance. Of course, this hinges on having such a skillful precipitation forecast, but the framework presented here provides a guide to how the prediction

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Discussion paper

would be implemented. I believe that the discussion paper can be accepted as a final HESS paper after moderate revision. My specific comments are listed below. I am particularly interested in the authors' response to comment #7, as I fear that I am missing some key element of their methodology. If I'm not missing something then I would recommend that the authors reframe or remove the forecast materials that led me to make that comment.

Response: We thank the reviewer for the positive comments.

Specific comments: 1. line 18: what is "simplistic precipitation forecast skill"? I think some rephrasing is required.

Author's response: We rephrased it to "simplistic precipitation forecast skill based on climatology and linear trend."

2. Introduction: as stated in my summary, my understanding is that this study is motivated by (or, at least, could be motivated by) the problem of monitoring and forecasting the end of a drought on the basis of precipitation requirements. But it took me a while to come to that understanding, in part because the introduction does not, in my opinion, offer a clear statement of the intellectual contribution of this paper. There is good material reviewing GRACE and reviewing drought cessation estimates, but the final paragraph of the introduction simply states what the authors are going to do and not why they are doing it in the context of a gap in the literature or a target application. It would be helpful to have a few sentences that make the importance of this paper more clear.

Author's Response: Thanks, we added a line as advised. "The intellectual contribution of this paper is in the estimation drought recovery and conceptually bringing a framework for drought recovery forecast based on precipitation deficit. "

3. GRACE data: how sensitive are these results to the choice of GRACE product? If only mascon are to be used then please justify the choice of mascon over spherical

harmonics solutions for this application. Also, more than one mascon solution is now available, and it would be useful to see that the results presented here are robust to the choice of mascon product.

Author's response: The GRACE analysis in this paper is based on climatological anomalies of the three monthly smoothed and detrended TWS signals, therefore fine differences between different GRACE solutions after all these postprocessing gets minimized. Mascon based GRACE products have a relatively similar spatial resolution (3x3deg) as that of GPCP (2.5x 2.5deg). Section 2.2 talks about it, "The spatial resolution of the original GRACE solution (3-degree mascon) and GPCP (2.5-degree) are comparable. However, as mascon size varies with latitude, therefore to improve the interpretation both datasets are brought to the 0.5-degree grid. "

4. GPCP: similar question here. How sensitive is the analysis to choice of precipitation dataset? There are a number of choices available for the period of study.

Author's response: Yes we agree there are many precipitation products like CRU, GPCC, etc. However, GPCP is a widely used global precipitation data. GPCP combines the strength offered by in situ as well as satellite data. In many regions of the world in situ data are sparse, so using a product that only utilizes in situ data may not be the best choice. GPCP applies gauge under catch correction to in situ precipitation measurement, which has been found important to improve snowfall measurement (Behrangi et al. 2018). Besides, in section 3.3 historical analysis of the data is done using 1979-2017 precipitation data. For this period GPCP is the best available data. Behrangi, A., A. Gardner, J. T. Reager, J. B. Fisher, D. Yang, G. J. Huffman, and R. F. Adler (2018), Using GRACE to Estimate Snowfall Accumulation and Assess Gauge Undercatch Corrections in High Latitudes, Journal of Climate, 31(21), 8689-8704, doi: 10.1175/jcli-d-18-0163.1.

5. line 110 et seq.: It is true that a long-term linear trend is often due to non-climatic processes. But some GRACE trends ARE due to climate–for example, a major drought

at the beginning or end of the record. The authors should comment on this possibility at some point in the manuscript, and discuss its implications for results in some regions.

Author's response: Thanks for bringing in, we added a line: "We acknowledge the caveat of a possibility of sudo trend due to unusual signal at the beginning or end of the record in some regions."

6. line 158 et seq.: "Figure 2" in this passage is actually Figure 3. Author's response: The maps in Figure 2 demonstrate the strength of the TWA-precipitation relationship globally. So, Figure 2 is correct.

7. Section 3.3.2 and other materials on forecasts: I have to admit that I don't understand the emphasis on these hindcasts in the paper. As the authors acknowledge, it's a simple method that doesn't provide very meaningful forecast. So what is it used for? It seems that the analysis presented in the results section only requires statistics of historical rainfall (mean and standard deviations) that can be compared to observation. The forecasts simply seem to play the role of a not-quite-perfect estimate of climatology. I do understand the authors' point about why forecasts might be useful in the context of predicting the end of drought via forecast of required precipitation. But there is no demonstration of this value in the current paper, as far as I can tell; there's only the claim that it might be valuable.

Author's response: The signal reconstruction and forecast discussed in section 3.3.2 is essential as we used it to create a normal signal first and then used standard deviation to simulate two additional precipitation scenarios of wet and extremely wet conditions. The normal signal is composed of predominantly climatology and long-term trend as the demonstrated model has the least competence in the estimation of inter-annual signals (0-3months). These precipitation scenarios are further needed to demonstrate the possible recovery duration from drought.

8. line 254: Doesn't blue n this figure indicate good agreement?? Author's response: That's right, thanks. Blue is changed to red.

9. line 269 et seq.: It appears that Figure 10 is incorrectly referred to as Figure 8 throughout this passage. Author's response: That's right, thanks, we corrected it.

10. Section 4.2.2: I assume that Figure 10 here really refers to Figure 11 Author's response: That's right, thanks for pointing out. Corrected.

11. I recommend an edit for style and grammar. The paper is clear, but there is some awkward phrasing. Author's response: Edited the manuscript. Many thanks for the supportive comments.

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