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Discussions

Interactive comment on "Forecasting terrestrial water storage changes in the Amazon Basin using Atlantic and Pacific sea surface temperatures" *by* C. de Linage et al.

Anonymous Referee #3

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The paper by de Linage et al. relates GRACE-derived terrestrial water storage (TWS) anomalies (TWSA) over the Amazon Basin to climate indices via a differential forecasting approach. The study aims to forecast the seasonal water storage anomalies over the study region. I find the topic of the study quite interesting. Such simulation and forecast of TWS changes have the potential to be used for drought monitoring.

When I read the paper, however, I found several issues that were not well addressed in the manuscript. The main missing/vague issues, for instance, include;

- a discussion on the selection of proper indicators (in this case: the selected climate

indices)

- physical interpretation of the introduced lags

- the influence of the local dynamics, caused by e.g., precipitation and evapotranspiration, which are ignored in the current forecast

- a discussion on the selection of the forecasting method

- an evaluation of the robustness of the performed forecast

- uncertainty estimation of the simulation and forecast

The mentioned issues make a fair justification of the manuscript in its current shape quite difficult. Therefore, I suggest reconsidering the performed analysis and the structure of the paper.

Detailed comments

Abstract: (P12454)

L5-L6: propose a modeling framework for inter-seasonal flood and drought forecasting. –> This is not true. The proposed approach only relates TWS to climate indices.

L8: calibrated against –> related to

L20-L23: What is the physical assumption behind the selection of the mentioned lags? There has been no justification offered in the manuscript for selecting 8 months lags between indices and TWS.

L24-end: These relationships may enable the development of an early warning 25 system for flood and drought risk –> The provided approach has the potential to be used for drought monitoring.

Such strong statements on using the proposed approach for drought monitoring should be claimed after applying more rigorous investigations on for instance:

- the uncertainty of the model (uncertainty of the inputs, simulation and the forecast should be evaluated),

- its possible biases (a phase bias is evident in plots of Fig 4),

- the input indices might not be representative enough to forecast seasonal variations of TWS. This is addressed in the later comments.

- the relationships between TWS changes and drought/flood occurrence.

Introduction:

The introduction missing the following points:

1- Several haydrological models exist which can be used to simulate and forecast TWS. What is the reason behind selecting the current forecasting approach, which makes it more interesting than an available model?

2- Since the method works based on the correlations between inputs and TWS, one can use statistical approaches such the common technique of canonical correlation analysis for the forecast (see examples in http://www.cpc.ncep.noaa.gov/). What is the benefit of using the current modeling rather than a relatively simple statistical approach?

3- Add some statements to explicitly show: how long the method intends to forecast TWS e.g., is it up to some months? one year, or more?

4- Regarding to the claimed forecasting time-scale, which is seasonal in this case, I believe that the use of indices as indicators is insufficient. This is explained in my later comments.

(P12456):

L1: Therefore, a dual external forcing seems to control the Amazon's hydrology and climate on interannual time scales. –> Several local factors also contribute in interan-

nual variability of water storage in the region. The contribution of local Precipitation and Evapotranspiration might be even stronger (on seasonal scale) than the contribution of the studied large-scale climate phenomenon.

The term hydrology is quite general and includes several parameters rather than the storage compartment. This study only addresses water storage, please simply call it TWS.

L2: ecological studies –> Ecology is a term that usually refers to interactions among organisms and their environment. Please replace it by a proper term.

L4: remove ecosystem

L24-L26: One might consider physical relations in introducing the lags. It is obvious that the lag of 8 months is somewhat unrealistic.

L26: data section –> section 2, please call the result section and so on with the section number appears in the paper.

Data:

2.1 GRACE terrestrial water storage anomalies (P12457)

Description of the data is quite sufficient. An evaluation of the implemented data set against the products of one of the official data centres is missing.

2.2 Sea Surface temperature (P12458)

The study simply uses climate indices that are, in the case of the current study, computed from SST data. Therefore, no processing of SST data is adopted in the study. I believe that a direct using of SST data (over the proper oceanic basins that are related to the study area) might improve the performance of the presented forecasting approach. Examples regarding streamflow forecast can be found in e.g., Westra et al. (2008). In case that the authors intend to keep the current presentation, the parts related to sea surface temperature data should be excluded from the paper.

Westra, S., Sharma, A., Brown, C., Lall, U. (2008). Multivariate streamflow forecasting using independent component analysis. Water Resources Research, 44 (2), W02437, doi:10.1029/2007WR006104

(P12459)

L11-L14: In the abstract, it has been stated that the longer lags would affect the performance of the presented forecast. In the light of that statement, one might use an indicator which introduces more information to the prediction step.

Methods

The method section is not well described. No references are provided regarding to the concept and the suitability of using the preferred method over the study region. As I mentioned before: since the method's forecast is based on lag correlation between inputs and outputs, what might happen when one uses a common canonical correlation analysis approach to forecast TWS?

Results

Since the forecasting approach has not been properly parameterized, detailed comments on the Results Section are avoided here. Here, the major issues are summarized:

- Applying a spectral analysis to the indices, for instance the ENSO index, the dominant frequencies include some 5 and 3 yearly frequencies along with considerably less dominant annual frequency and some low amplitude cycles with seasonal frequencies (period of some months). Therefore, the contribution of the index (as it is used in this approach) in the seasonal forecast might be marginal. This can be seen e.g., by estimating the contribution of ENSO in TWS changes over the study area which the amplitude might be around 5cm. This is much smaller than the reported 30 cm TWS. Subsequently, using the presented setup of the forecast, the annual variability of ENSO and that of TNAI are poorly related to the annual variability of TWS over the

study region. Thus, probably, the model learns the annual component from 10 years of GRACE observations rather than the input parameter. If this is the case, one should test whether the simulation length is long enough to reach such knowledge and what happens when an extreme event happens. Is the model able to correctly update the amplitude of the annual signal? As I see in Figure 4, all the model curves represent a visible phase bias, which is quite pronounced e.g., in the year 2010. Evident miss match of the annual amplitudes also can be seen in several years. The situation is worse for the seasonal component, which I think; it misses a proper indicator to introduce the local dynamics of water storage changes to the forecasting model.

The performed analysis also misses:

- A test for analyzing the robustness of the simulation step as well as for the forecast skill.

- Uncertainty estimation of the simulation and forecasting values.

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