Author response to:
RC2: 'Comment on hess-2021-104', Anonymous Referee #2, 17 May 2021

The article is well written and all the concepts are explained in required details. However, the inference drawn from the study need more clarifications and probably a major revision. After writing this review, I happened to check the review comments from reviewer 1 and I had exactly the same comments as their comment no 1, 2, 3 and 10. I am not repeating them here. My additional concerns are:

Thank you for your time spent reviewing our manuscript and for your valuable comments. We hope our notes written to the other reviewer that address their comments 1, 2, 3, and 10 also address your concerns about this paper.

1) Authors have stated, in the title and in the introduction (line 65 to 69) that they are able to identify and estimate the processes responsible for TWS variability. Explicitly they have stated their aims as: a)"inform and reduce uncertainties of terrestrial hydrologic processes regulating the seasonal and inter-annual variability of TWS in the western Amazon, the Gavião watershed" b)Develop a model to represent the first order controls on seasonal-to-decadal soil moisture dynamics, and c) employ a Bayesian model-data fusion approach to constrain model parameters (namely initial states and time-invariant process variables), such that differences between GRACE and simulated TWS anomalies are statistically minimized

Therefore the first impression is that the study will help us understand the cause and drivers of variability in the water storage. However, the results and summary lack discussion on the parameters and physical processes that drive the dry season or wet season, and why?

This is a great comment. We believe we can address this issue with the inclusion of additional results from two other basins. By examining the differences in the parameter inference and resulting TWS simulations between the different basins, we believe we can better answer the questions laid out in the introduction. We will choose two other watersheds with contrasting precipitation levels, to demonstrate how information content varies across precipitation gradients.

2) The calculation of basin average TWS from GRACE data is not clearly explained. Did the authors use mascon data or spherical harmonic data? How do they combine these three solutions? How do they tackle the limitations of the coarse spatial resolution of GRACE data and the different spatial resolution of the three data products.

Thank you again for this comment. Yes, we use the spherical harmonic data. We aggregate the GRACE information over the entire basin and estimate a basin-wide timeline of the TWS anomalies. We apply the same method to aggregate the other products to estimate a basin-wide timeline of P and Q, and therefore estimate a basin-wide timeline for the ET variable as well.

3) Can authors comment or compute their model for an arid or semi-arid region or a river catchment with trends (such as the Great Basin in the USA)? What do they expect to obtain from such a model at global scale?

Yes, we will apply a similar computation for simulations in two other basins in the Amazon. We will choose two other watersheds with contrasting precipitation levels, to demonstrate how information content varies across precipitation gradients.

4) Please mention the catchment name in the title instead of 'Amazon' or compute the model for whole Amazon.

As mentioned, we will apply the methodology for 2 other basins in the amazon. We will change the title to reflect this, i.e. "Information content of soil hydrology in Amazonian watersheds as informed by GRACE".

5) Line 361: Authors state "GRACE-informed model parameters can be used for predicting seasonal and inter-annual soil water hydrology in the absence of concurrent GRACE measurements. We showed that using a 5-year data record of TWS allows the parameter inference to still be applicable to the remaining 5-year data record, which is simulated without the use of information from GRACE". If the evapotranspiration product was obtained using GRACE, then the simulated part is not completely free from GRACE.

We addressed a similar comment by reviewer 1, and we will re-address this issue here. To clarify this more, there are 3 different derivations used for the TWS variable. These 3 estimates provide a sense of uncertainty for the TWS. The uncertainty from these different 'products' is used in the likelihood function of the MCMC algorithm when fitting the model simulated TWS to the GRACE derived TWS. Then, there is also 3 products used in the precipitation and the runoff driving variables that were used, to get a sense of the uncertainty in each variable. To estimate the ET driving variable in this work, we use the mean of the TWS, P, and Q products and create a water balance that will allow us to estimate a mean for the ET driving variable. Then by application of the ET scaling parameter, we try to estimate whether our initial calculation of ET required any scaling to match the data. Therefore, even though the GRACE TWS is somehow used in the derivation of the ET data, the uncertainty that is applied throughout the work allows us to still estimate ET that is not dependent on the GRACE data. To address this specific comment of the reviewer, though, we will change the terminology in this sentence, and will not state that "*GRACE-informed model parameters can be used for predicting seasonal and inter-annual soil water hydrology in the absence of concurrent GRACE measurements*". Instead, we will simply refer to this part of the manuscript as a calibration/validation exercise.