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Interactive comment on "Quantification of Drainable Water Storage Volumes in Catchments *and* in River Networks on Global Scales using the GRACE and/or River Runoff" *by* Johannes Riegger

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This is an interesting and novel study on estimating the drainable water storage in large river basins based on observed discharge data and/or water storage anomalies from GRACE. The work develops an approach based on the linear storage concept to separate the total drainable storage volume of a river basin into two storage compartments, which are denominated the catchment storage and the river network storage. The manuscript comprehensively presents the methodological and mathematical concept of the approach and nicely illustrates the storage characteristics for the single and the two storage assumption in terms of signal dynamics, amplitudes and phases for

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Discussion paper

virtual experiments, including an assessment of uncertainties of the parameter estimation procedure. The concept is then applied to the real world example of the Amazon river basin, leading to interesting lumped results of basin storage and runoff dynamics. Nevertheless, I have some general doubts on the method and the way it is realized in this study:

1) The storage concept presented here, by looking at linear storages and their storage coefficients or 'time constants', takes a purely temporal perspective on catchment storage. It separates two storage compartments of different volume and drainage behaviour in time. While basically a viable approach, this is presumably not as straightforward as the manuscript implies when it comes to linking these quick and slow storages to a spatial (i.e. source area) perspective of storage and flow. For example, a quick runoff response may partly occur from the so-called catchment storage by, e.g., subsurface storm flow, whereas a slow response may also occur along the river network due to surface water-groundwater interactions or floodplain storage. Thus, I wonder whether the separation into a catchment and a river network storage as implied by the title can really be achieved by the method applied here, instead of a separation of a quick and a slow storage compartment.

2) As a prerequisite of the applicability of the approach, uncoupled storages (i.e. storage compartments that do not directly drain to the catchment outlet) need to be negligible or time invariant (page 23, lines 4-5). It is assumed that this condition is fulfilled in the study area Amazon basin (page 18). However, I doubt whether this assumption holds true. Given the strong seasonality of rainfall and evapotranspiration in large parts of the basin, there are substantial temporal variations of water storage in the unsaturated zone (e.g. Tomasella et al., 2008), including moisture states drier than field capacity of the soil, i.e., non-gravity-driven conditions. Such conditions correspond to storage variations in non-coupled storage compartments as defined for this study and were assumed to be negligible. This calls the approach into question.

3) The separation approach presented requires an estimate of recharge for the river

basin of interest. Three options are suggested (page 18). Following these suggestions, the input that should rather be called precipitation surplus, as commented by another referee, is not necessarily what it is claimed to be, i.e., it is not groundwater recharge or a similar flux term that contributes directly to a connected storage, but may at last partly go into an intermediate storage or it may experience travel times to the saturated zone given large groundwater depths in some parts of the Amazon basin. Thus, I wonder what the effect of this discrepancy between precipitation surplus and the required contribution to the connected storage is on the validity of the results and the values obtained here (e.g. of time constants).

4) The approach assumes a linear storage concept for representing the river network dynamics. As commented by another reviewer, this may not be adequate for several river basins. In particular, it does not apply to the Amazon basin given the particular dynamics of floodplains and inundation areas, and different gradients of large-scale water levels at the seasonal scales between the rising and falling limb of the annual flood wave.

5) It should be clarified to which extent the drainable storage values obtained for a particular river basin depend on the actual time period used in the analysis and on their particular observed storage amplitudes (which probably are smaller than what is physical reasonable and possible at the long-term), or whether they represent some fundamental catchment property.

6) While phase shifts between simulation results of hydrological models and GRACE storage variations exist as noted by the author, it is generally accepted that they can be attributed to model deficiencies in representing river flow routing or inundation dynamics, and the discrepancies may eventually be used to improve the model. In my view, parts of the manuscript that indicate that this study provides a new explanation for these phase shifts (e.g. page 5 last paragraph, page 23 line 11) may need to be re-written as I do not see this potential new contribution. In particular, given the lumped temporal nature of the approach presented here, the study does not contribute to a bet-

ter understanding of reasons for phase shifts from a process-based perspective (page 5, line 21).

Minor comments:

7) As pointed out by another referee, the abstract needs to be shortened considerably, and the entire manuscript needs polishing of English language.

8) Hysteresis plots (Figure 1 and others): the direction of the hysteresis should be indicated.

9) Throughout the paper I suggest to use the term ‘storage anomaly’ instead of ‘storage deviation’, in line with what is usually used in literature.

10) page 11, line 21: ‘signal amplitudes’: first occurrence of this term which is not fully clear at this point. Explain here. Also the notion with a sigma sign for these signal amplitude ratios is somewhat confusing at the first instance.

11) page 12, line 17: ‘2D dependence’: not clear.

12) page 19, line 2: ‘monthly residuals’: not clear at this point. (Only later it is explained that the de-seasonalized time series are meant here.)

13) page 20, figure caption 11: what does d in dMT mean? (if it is deviation, use mass anomaly)

14) page 23 and 24: equations may be skipped in the conclusions.

References:

Tomasella, J., Hodnett, M. G., Cuartas, L. A., Nobre, A. D., Waterloo, M. J., and Oliveira, S. M.: The water balance of an Amazonian micro-catchment: the effect of interannual variability of rainfall on hydrological behaviour, Hydr. Proc., 22, 2133-2147, 10.1002/hyp.6813, 2008.

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