

Title: Quantification of Drainable Water Storage Volumes in Catchments and in River Networks on Global Scales using the GRACE and / or River Runoff

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I carefully read the manuscript and comments from M. Bierkens and A. Gunter. They both raise important questions on the actual applicability of the proposed method. I found a few points in the manuscript that need more deepening. For that reason, I suggest that the paper could be considered for publication after review. Please find my comments below.

Major comments:

1. As pointed out by M. Bierkens, the abstract should be significantly shortened. I also recommend that the author avoid the use of concepts or terms in the abstract that are not properly explained. For example, it is not clear in the abstract alone, what the runoff-storage relationship (P.1, L. 17) is. It is also not clear what phase shift (P.1, L. 20) is being referred to. I also suggest that the Introduction should be rewritten, focusing on a clear statement of the issue the author is trying to address, a comprehensive literature review on what has been done before, and a simple description of how the problem will be tackled. Details on the technique should be reserved for the following sections.
2. My feeling is that there is a general lack of recent and appropriate literature in the field. For example, in the abstract, the author states: *“A possible reason for the observed phase shift might be found in the river network storage, which so far has not been addressed separately in the R-S relationships.”* Also, in the introduction: *“Very little attention is given so far to the storage volume of renewable water resources participating in the dynamic water cycle driven by precipitation P, actual evapotranspiration ETa and river runoff R.”* Many modeling studies have been performed towards a better understanding of surface water storage (SWS) and dynamics. The impact of SWS on the terrestrial water storage variability is evaluated globally in Getirana et al. (2017a). In that study, the authors use Noah-MP, accounting for a detailed computation of the water and energy balances, including groundwater recharge, and an advanced river routing scheme, accounting for river and floodplain dynamics using the local inertia formulation. In page 5, the author states: *“Even though global hydrological models comprise a number of storages like soil, surface water, groundwater etc. some of them show considerable phase shifts between the calculated and measured runoff and an underestimation of the signal amplitudes (Güntner et al., 2007, Chen et al., 2007, Schmidt et al., 2008, Werth et al., 2009, Werth et al., 2010).”* There are very well known reasons for these issues to happen, and the references used to support that statement are somehow outdated (8-11 years old). Recent developments on hydrological modeling, in particular, river routing schemes have successfully dealt with phase shifts and amplitude ratios in both Amazon and globally (Getirana et al., 2014, 2017b; Luo et al., 2017; Paiva et al., 2013; Yamazaki et al., 2011, 2012, 2014; Siqueira et al., 2018). I strongly suggest that the author better contextualize the study pointing out what the contribution is, considering what has already been done.

3. It is common sense to use the term runoff for the surface or total runoff generated by a land surface model, usually given by mm/d or mm/s, which is the rate of water flowing to the river network, while streamflow is used for the river discharge, usually in m³/s. The former is either simulated by LSMs or estimated from the spatial distribution of the latter, which can be observed at gauge stations.

Sometimes, in the text, I get confused with what the author is referring to. For example, in the abstract, the author refers to “observed runoff”, while it should be “observed streamflow”. I suggest that the author make a proper use of these terms and clarify when runoff and streamflow are used.

Minor comments:

1. In the paper, the application of the technique is limited to the Amazon, and I think that using the term “global scales” in the title is a bit of an overstatement. I suggest the removal of that term from the title.
2. P. 2, L. 1: Define Cascaded Storage approach
3. P. 2, L. 6: Define w.r.t.
4. P. 2, L. 31: “semi / arid” – Do you mean, semi-arid, or semi-arid and arid?
5. P.3, L19-20: “surface water, the river network and temporarily inundated areas” – what differentiates surface water from river network and temporarily inundated areas? It seems to me that the latter two are part of the former.
6. P. 6, L. 2: Define GIEMS
7. P. 6, L. 2-4: “Observations of inundated areas in river networks provided by the GIEMS project (Prigent et al, 2007, Paiva et al., 2013) indicate a considerable contribution of river network storage for the Amazon Catchment” – Getirana et al. (2012) provide the actual water storages in rivers and floodplains in the Amazon basin.
 1. P. 6, L. 6: Paiva et al. (2013) is not a GIEMS reference.

I hope these comments will be useful for the preparation of an improved version of the paper.

Additional references

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