**Hydrology and
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Interactive
Comment

Interactive comment on "Past terrestrial water storage (1980–2008) in the Amazon Basin reconstructed from GRACE and in situ river gauging data" *by* M. Becker et al.

M. Becker et al.

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Dear Referee#2, we are very grateful with your review of our paper. We will try to take advantage of your advice to improve the clarity and support the validity of the methods. For an easier comprehension of our answers, your comments are reported below and our response is given just after.

Referee#2: 1. To allow for reconstructing past water storage variations from recent GRACE observations, one main assumption of the approach used here is the stationarity of the spatial patterns. In Chapter 5, the authors test this stationarity based on

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the gauge data for different time periods. However, the time periods they chose are overlapping. It should be more convincing to prove the stationarity for independent time periods, e.g., 1980-2002 and 2003-2008, or for two separate periods of 14 years each.

Maybe the use of the term "stationarity" in the manuscript is somehow misleading. It is in chapter 4, when we deduce equation 3 from equation 2, that we are making what we call the "stationarity of the spatial patterns" assumption. This assumption assumes actually that over the reconstructed period, the EOFs of the TWS field can be written as the linear combination of the temporal amplitude of the EOFs ( ) with its spatial patterns computed from the short GRACE period ( ) instead of the one computed over the whole reconstructed period ( ). This assumption does not actually assume the stationarity of the spatial patterns through any time scale (as usually done in the literature when using the term "stationarity") but it only assumes that the spatial patterns of the TWS over the GRACE era (2003-2008) ( ) is representative of the spatial patterns of the TWS over the reconstructed era (1980-2008) ( ). So actually the test we made in the paper is the closest possible to the assumption that has been made in the reconstruction process. It is maybe the term "stationarity" that does fit exactly to the sense of the real assumption that has been made.

Referee#2: 2. The authors use in-situ water level time series as a proxy for total water storage comparable to GRACE data. At the beginning of Chapter 2.1., they argue for using water level instead of discharge by saying that water level represents one of the TWS components, presumably surface water storage. However, neither variable represents surface water volumes directly. Even water level may relate in a nonlinear way to inundation extent and thus surface water volumes.

Thanks for this comment which we agree with.

Referee#2: 3. 'Scaling factors' between GRACE time series and water level (Table 1): Is there some physical reason or regular pattern that may explain the variation of

scaling factors found among the different stations?

The variation of the scaling factors seems due to their location in the basin : small in the Solimoes basin and large in the Tapajos basin (near the mouth of the Amazon). This is only an observation and actually we are currently trying to understand the physical reasons which drive these variations. It appears to be a difficult work which is beyond the scope of this paper.

Referee#2: 4. page 8131, lines 1 and 2, Figure 2: Mode 1 of the EOF results rather suggests a North-South pattern than a West-East pattern as claimed by the authors in the text.

This point is noted and the text will be changed in a revised version of the paper.

Referee#2: 5. page 8131, line 15: for computation of the regression between GRACE and water level time series, have water level data also been averaged for the same 10-day periods as GRACE?.

We consider in this study the monthly GRGS GRACE data (and not the 10 day). This will be corrected in a revised version of the paper. So, indeed, Grace data and in situ data have been averaged over the same period of 1 month.

Referee#2: 6. page 8132, lines 8-10. Another reason for a poor correlation between water level and TWS may be the significant contribution of other than surface water components to TWS. Do the authors have an explanation why the correlations tend to be higher for the northern Amazon basin, in particular the Rio Negro, and less for the south(-east)?

This question enter as well in the problematic referee 2 pointed out at paragraph 3: How and why water level correlate with TWS in the amazon basin?. We are currently investigating this issue and it seems that the different level of correlation are also due to environmental variables such as topography, geomorphology, soil types. Nevertheless we do not have many details on this now. This is a hardwork that we think is beyond

the scope of this paper.

Referee#2: 7. page 8132, line 18: refer to Chapter 4 instead of Chapter 3.

This point is noted and the text will be changed in a revised version of the paper.

Referee#2: 8. page 8137, line 26: comparison of reconstructed TWS to model output: Has the ISBA model output been filtered in the same way as GRACE data? Filtering of GRACE data to reduce error-prone signal components and to subset for a particular area usually results in signal amplitude damping. The comparison to an unfiltered model output may not be consistent in terms of e.g. amplitudes in this case.

We agree with the reviewer. A fair comparison with GRACE observations requires that ISBA fields be spatially filtered in a similar way. In the revised manuscript, ISBA TWS gridded fields are expanded in spherical harmonic (SH) functions. SH coefficients are truncated above degree and order 50. Finally, ISBA SH representations are evaluated on a global $1°\text{x}1°$ grid. The figure 6 and the correlation coefficient are recomputed.

Referee#2: 9. page 8138, line 21-23: 'In effect, river levels in downstream flows ..' This sentence is not clear to me neither from the point of view of its English expression nor from its argument in relation to the 'directional correlation' mentioned before. This part should be re-formulated. The authors may also consider that precipitation events in upstream areas may affect downstream water levels with some time delay only due to water transport and not 'directly' as written in the text.

This point is noted and this sentence, line 21-23, will be removed in a revised version of the paper. And we will add a sentence on the impact of the precipitation on the water level, as suggested by the reviewer.

We will do our best to address your comments and concerns above in the revised manuscript. Thank you again for your comments.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8125, 2010.