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Interactive comment on "Recent changes in terrestrial water storage in the Upper Nile Basin: an evaluation of commonly used gridded GRACE products" *by* Mohammad Shamsudduha et al.

Anonymous Referee #2

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This study evaluates, for the Upper Nile Basin over the 2003-2012 period, several estimates of terrestrial water storage (TWS) as processed from the Gravity Recovery and Climate Experiment (GRACE) retrievals with in situ and model-derived estimates of its individual terms: surface water storage (SWS), soil moisture storage (SMS), and groundwater storage (GWS).

The authors reach interesting conclusions, namely 1) the pre-processing of GRACE greatly affects estimated annual TWS amplitude and, most notably, reconcilability with bottom-up approaches and 2) uncertainty in GRACE TWS and model-derived prevents a reasonable inference of GWS variation in these aquifers.

While I appreciate the scientific value of this work, I find this manuscript confusing at

times in its logic, and lacking rigor regarding how methods and some quantities are defined. Therefore, I recommend resubmission only after the authors have made a substantial rewriting effort to improve the clarity of the presented results.

General comments

- "In situ ΔTWS" is used throughout the manuscript, but this term is quite misleading: as defined in Eq. (1) and then L379-381, this quantity is the sum of ΔSWS, ΔGWS, and ΔSMS estimates. While the two former terms are indeed estimates based on situ measurements, ΔSMS is averaged from simulations with three *gridded* hydrological models at 0.25° resolution (Sect 3.1.3 and L580-581). This is of particular importance since the whole study is about attempting to reconcile estimates of storage compartments across approaches and scales. I suggest using something like "bottom-up ΔTWS" instead.
- The method section is rather long, in particular the description of GRACE datasets retrievals and the applied methodology in sections 3.1.2, 3.2.1 and 3.2.2. While I understand the authors want to present the remaining datasets (ΔSWS, ΔSMS, ...) before detailed how ΔTWS is being processed, sect. 3.2.1 and sect 3.2.2, are even frankly confusing at times, e.g., when the ΔTWS scaling methodology is explained (L357-363, see specific comments) and then discussed again (L387-397) so that in the end I am not sure what was used for the study.
- TWS sometimes appears instead of ΔTWS (e.g. L79-86). While this be should a mere technical comment, in some cases TWS would actually be more accurate in the general sense (i.e. the concept of storage), e.g. when discussing reduction in volumetric storage in the whole basin (e.g., L537-539 where "ΔTWS" is used).

Specific comments

L21-22: It would be more accurate to say that the authors *"test the phase and amplitude of three GRACE ΔTWS estimates derived from 5 commonly-used gridded products [. . .]"*.

L123: What is the actual time span of the "unintended experiment": 2004-2006 (like stated here)? 2005-2006 (e.g., L553)? 2003-2006 (most of the manuscript)? The authors should delimit this period consistently across the main text, the tables, the figures, and the supplementary materials.

L169-173: The authors should comment on the large discrepancy between these two lake area estimates. In addition, why do the authors report the *HydroSHEDS* area value as being from this study in Table 1?

L357-363: The authors first state that they spatially aggregate the unscaled ΔTWS signal over the study region in order to have a time series, but then say that the scaling factors are applied to each grid of the GRACE mesh, therefore it is done before spatial aggregation? Please clarify.

L395-397: Along with the regionally-averaged gain factor, why did the authors not also test the third method described L392-394?

L415-418: A lag of 2-3 months between lowest rainfall and lowest ΔTWS is also well noticeable, while ΔSMS respond more quickly to rewetting after the driest month ($\approx$1 month) and ΔSWS is slower ($\approx$4 months lag after minimum rainfall).

L432-434: Figs. S5 to S7 are relative to the entire Victoria Nile Basin and not Lake Kyoga Basin, I do not see how the authors can derive the observation that *"GRACE-*

derived ΔTWS signals are strongly correlated in both LVB and LKB (see supplementary Figs. S2–S7)". The same applies L441-444. Maybe the figures were unintentionally swapped with relative to LKB?

L446-447: This sentence is misleading since only 3 ΔTWS estimates are used shown, albeit derived from 5 different GRACE products.

L449-456: The authors might already mention that only ΔGWS shows an increase in 2005-2006, as later discussed in the Discussion section.

L457-458: A support supplementary figure with time series for LKB would help. Is it what Fig. S9 should have been (instead of describing the Victoria Nile Basin)? If so, the authors should add a reference to Fig. S9 here, and replace *"[. . .] (see supplementary Figs. S8–S9)."* by *"[. . .] (see supplementary Figs. S8–S9)."* in L456, and caption of Fig. S9 should read "LKB", instead of "VNB".

L465-466: I am not sure what the authors mean, how could the TWS signal miss one of its component, unless it refers to a water transfer within the system? All the more that even if mention of LVB-driven water balance of LKB is given on L175-177, this point is not picked up later in the Discussion section. Is it related to the substantial variability of ΔTWS deriving from ΔSMS in in LKB as compared to LVB? Could the authors expand their idea?

L476-477: Why scaling down ΔSWS rather than using the rescaled ΔTWS presented right above (L474-476) to disaggregate ΔGWS?

L526-527: This sentence essentially repeats L517-518, with typos (see *Technical comments*).

L529: The measurement error is not necessarily only a bias (systematic) is there are random components; Swenson and Wahr (2006) seem to keep this broader definition.

L541-548: Would not it be more correct to say that the choice of ΔSMS from LSMs contributes to uncertainty in estimating bottom-up ΔTWS (termed in situ in the manuscript, see *General Comments*), and consequently comparing it to GRACE ΔTWS, rather than uncertainty “GRACE analysis”? In addition, the order of sentences in this paragraph leaves me with the impression that this study did not bring any improvement to estimating bottom-up ΔTWS, while most of the manuscript uses this estimate as a benchmark to test GRACE ΔTWS products. In order to avoid finally leaving the reader with *“how reliable is this ΔTWS reconciliation then?”*, the authors should maybe remind in the discussion that ΔSWS is by far the largest contributor in LVB at least, somewhat limiting the propagation of ΔSMS uncertainty.

L616-617: This should probably be stated already in the Discussion.

Technical corrections

L101: SSA is not used anywhere else in the manuscript of supplement.

L527: Likely typos, maybe *“[. . .] priori information from LSMs contributes to adding uncertainty to ΔTWS signals”*.

Figs. 5 and 6: What are the dashed vertical lines in the top panels and the horizontal dashed line in the bottom panels?

References

- Swenson, S., and Wahr, J.: Post-processing removal of correlated errors in GRACE data, Geophys. Res. Lett., 33, L08402, doi:10.1029/2005GL025285, 2006.

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