

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 #1

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This study aims to estimate the TWS change and its individual components in the Upper Nile Basin using GRACE, LSMs and in situ observations. Actually, similar studies have been done in this region by Awange et al. [2013], Awange et al. [2014], and Nanteza et al. [2016]. So, the main point is whether this manuscript can bring enough new knowledge based on new/updated data or methods. Different from previous studies, three different GRACE products (gridded level-3 GRCTellus, JPL mascon and constrained GRGS products) were compared and validated with in situ TWS observations in this study. However, the detailed scaling process used in this study is still unclear for me (see detailed comments below). I also suspect that whether limited 6 monitoring well observations can represent actual large-scale GWS variations in the study region.

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Especially, all three well observations in the LVB are located near the Lake Victoria. The representativeness of these wells is questionable. In addition, there are some obvious typos in the manuscript.

GWS estimation from GRACE: Based on my understanding on the manuscript, ΔGWS = the rescaled GRACE ΔTWS ($sf=1.7$ for GRCTellus, $sf=?$ for JPL mascon) minus scale-down ΔSWS ($sf=0.11$ for GRCTellus and $sf=0.39$ for JPL mascon) minus simulated ΔSMS . Why so-called a scale down of ΔSWS was used rather than the original ΔSWS (EWH, based on equation 2, Line 317)? In fact, the ΔGWS estimation from GRACE (GRCTellus, JPL mascon and GRGS) was not given in detail. I would suggest the authors explain it in a paragraph in 3.2.2.

(1) Line 240, CRS should be CSR.

(2) Line 248-249: GRCTellus datasets are provided as 1X1 grids, but ~ 111 km is not the so-called spatial resolution of GRACE. At least, in some place of the manuscript, the authors should emphasize that the real resolution of GRACE is about 300 km, rather than that provided by these level-3 products.

(3) Line 254, the citation Geruo et al., 2013 should be A et al. 2013. This is also a mistake in some other papers. Actually, A is his family name and Geruo is his forename.

(4) Line 287, the citation (CSR, 2016) was not shown in the References. If there is no publication about it, maybe the authors can provide the website link where the information was available.

(5) Line 405, JLP should be JPL. For GRGS, whether scaling factor was applied?

(6) Line 310, Fig.s should be Figs.

(7) Line 394, if I understand it correctly, gridded scale factors from Landerer et al. were not used in this study finally. The authors applied a single scaling (1.7) actually. Based on Figure S1 and the authors' experiment (Fig. S10b), the factors are highly underestimated by Landerer et al. in the LVB.

(8) In 3.2 Methodologies, how to estimate GWS using GRACE in detail? I would suggest the authors explain it in a paragraph in 3.2.2.

(9) Line 434, “in both LVB and LKB (see supplementary Figs. S2–S7).” The captions of Figs. S5-S7 are “over the Victoria Nile Basin”. Does the Victoria Nile Basin mean the LKB? The caption of Figure S9 also contains “in VNB”.

(10) Line 436, simulated Δ SWS should be simulated Δ SMS?

(11) Line 446, “all 5 GRACE Δ TWS and in situ Δ TWS time-series records”. There are only 4 curves in each panel of Figure 7.

(12) Line 449, “the period of 2004 to 2006”, but in table 3, “2003-2006”. This kind of inconsistency occurs several times in the manuscript.

(13) Line 464, “see supplementary Table S1”. No correlation estimates in table S1 in fact.

(14) Line 465-466, “GRACE Δ TWS is unable to explain natural variability in in situ Δ TWS in LKB though this may be explained by the fact that SWS in Lake Kyoga is influenced by dam releases from LVB”. GRACE can detect all mass changes including both natural and anthropogenic variability, but can not disaggregate individual components. If in situ Δ TWS includes all mass change signals, it should be consistent with the GRACE estimate, no matter mass change is natural or anthropogenic. I suspect that the lower correlation in the LKB might be caused by the smaller area of LKB and larger leakage errors from the surrounding regions (including LVB).

(15) The caption of Table S1, no “variability (i.e., variance, cm^2)” in the table. In the caption, what is the meaning of 120 cm^2 and 24 cm^2 ? The variances of in situ Δ TWS?

(16) Line 473-477, GRACE-derived Δ TWS was rescaled to recover the actual mass change. But, why the scaling down process was needed to remove Δ SWS for estimating Δ GWS? If rescaled Δ TWS time series was used to estimate Δ GWS, maybe the

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authors should use in situ ΔSWS (equation 2) rather than scaling down ΔSWS . I also cannot understand the caption of Figure 8. Why a scaling down process of SWS is needed for disaggregating GWS from GRACE-derived rescaled TWS (Line 399-405)

(17) Line 399-405, were these factors calculated from the product of Landerer and Swenson 2012 (Figure S1)? Note that this product should be used for recovering TWS rather than only for SWS. In line 402, $s=0.71$ for experiment 1. But in caption of Figure S10, $s=0.77$ for experiment 1.

(18) Section 3.1.3, GLDAS does not assimilate surface water, which is an important TWS component in the study region. Whether the absence of surface water process will highly affect the accuracy of simulated soil moisture from GLDAS? Maybe the authors can try to use WGHM model which considers the surface water. In Figure S12, the authors compared many LSMs except WGHM, which simulates all TWS components. If the authors removed ΔSMS from WGHM, maybe there will be a better agreement between in situ well observations and GRACE-based ΔGWS , although the representativeness of these wells is also questionable.

(19) Line 1117, cantered should be centered.

(20) Figure 8, what is the criterion of selecting Sy ?

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