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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Data assimilation of GRACE terrestrial water storage estimates into a regional hydrological model of the Rhine River basin" *by* N. Tangdamrongsub et al.

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We firstly would like to acknowledge the insightful comments and suggestions provided by Referee 3. Followings are the responses (A) based on the comments (Q):

Q1: Comment on the calibration vs. non calibration experiment: My first guess when I read the experiment setup was that results will not change much if the parameters were not calibrated but assumed to be the average over the basin. Even if you are using gridded (1deg) GRACE products, the spatial representation of GRACE is much courser than that so I would have guessed that the impact of a detailed (high spa-

tial resolution) calibration of the model parameters does not have a major impact on your results if the spatial average of the parameters are used instead. In my opinion choosing an average of the calibrated parameters as the "non calibrated" case may be too optimistic and not representative of a region with limited observations. I would suggest to add/substitute this case with one where the parameters are not known (e.g. for example maybe just derived from a global land classifications such for example: http://webarchive.iiasa.ac.at/Research/LUC/External-World-soil database/HTML/ or other globally available database)

A1: It is important to note that the OpenStreams-wflow model is a conceptual model, the parameters of which are calibrated using streamflow data and not based on physical observations (remote or in-situ). Figures 5, 7, 8, 9 and 10 show that the results do change when the parameters are not calibrated but assumed to be the average over the basin. Assuming no knowledge at all, one could set the mean parameter value in the non-calibrated case to the mean of the range of values permitted in OpenStreams-wflow. However, even in a data sparse region, data is available on the land cover type, topography, and climatology from globally available databases. Figure 3 shows how variable the parameters are across the Rhine basin. Averaging each parameter across the entire Rhine basin is intended merely to reflect this kind of first-order assumption. Though not all OpenStreams-wflow parameters can be gleaned from such global databases, we could compare the averaged values to the one in the FAO database. We found that our areally averaged parameter value over the Rhine falls into the range the value FAO provided. For example, the areally averaged soil moisture field capacity over the Rhine FAO provided is approximately between 150 and 200 mm, while the areally averaged value of approximately 180 mm is usedas a mean in the paper with a standard deviation of 33 cm. Therefore, our use of the basin mean value as a mean for the non-calibrated case is quite close to what the referee suggests given the constraint of working with a conceptual model. The discussion will be clearly stated in the revised manuscript.

Q2: Comment on the verification methods: The whole section about how/why you choose to scale groundwater in situ observations from piezometric to storage units needs some work. It is not clear to me why if you remove the soil moisture temporal mean from GLDAS you can get \Delta_GW_{in-situ}? Where does the \DeltaSM_{GLDAS} come in the context of equation (1)? If you remove a constant (average SM) from the GRACE aren't you effectively obtaining the same time series just shifted by a constant value?

A2: In order to validate our estimated GW, we need to express the variations in piezometric head in terms of a change in storage in order to compare them to the GW (UZ+LZ) estimates from the model simulation. Previous literatures have shown that GW storage can be computed by GRACE minus GLDAS SM. Therefore, we adopt a similar idea by using the relationship between GRACE minus GLDAS SM and the observed head to scale the observed head. The role of \DeltaSM_{GLDAS} in Eq. (1) is \DeltaGW_{GRACE} = GRACE - \DeltaSM_{GLDAS}. We do not remove the constant averaged SM_{GLDAS} from GRACE. Instead we removed the anomaly (variation) from GRACE. We firstly computed the anomaly of SM_{GLDAS} by removing its long-term mean (Page 11847, Line 20-21, "The variation in SM from GLDAS (\DeltaSM_{GLDAS}) was compute ..."), and then this SM variation is removed from GRACE (Page 11847, Line 22-24, "The groundwater variations from GRACE (Delta_GW_{GRACE}) were obtained by removing \DeltaSM_{GLDAS} from the GRACE observations every month"). Because both GRACE and GLDAS provided the temporal value (e.g., every month), \DeltaGW_{GRACE} are not identical to either the scaled GRACE or GLDAS soil moisture. This explanation will be stated more clearly in the revised manuscript.

Q3: Treatment of snow: It is unclear to me what is the need to remove snow from the GRACE observations prior assimilation? Why don't just include it in the assimilation scheme? And include a snow term in the calculation of the modeled TWS?

A3: The snow component is small averaged over our study area (in winter, approxi-

mately 2%, except over the Alp which is approximately 7% respected to SM+UZ+LZ), and so our defined TWS from OpenStreams-wflow does not include the dry snow component (this is stated in Page 11843, Line 25-27). To reconcile GRACE to OpenStreams-wflow TWS, we then removed the dry snow component from GRACE before the assimilation process. In catchments where the dry snow component is more significant, it could be included in the state vector as the referee suggests. This explanation will be clearly stated in the revised manuscript.

Q4: Actual EnKF scheme: It is assumed that a single observation is acquired in the middle of the month, however GRACE TWS is assumed to "represents the surface mass deviation for that month relative to the baseline average over Jan 2004 to Dec 2009." therefore this has to be considered as an average TWS variation for the entire month. This is effectively the reason why existing GRACE-EnKF techniques used a "two-step" approach (Zaitchik et al., 2008, Forman et al., 2012) where a single month was modelled twice: one time to obtain a "monthly average" observation prediction (from an open-loop simulation of the entire month, and not simply from the TWS modeled at a single day; and a second time to apply the increments computed from the EnKF. Are you also using a two-step approach or a straightforward application of the EnKF (as a real time assimilation scheme)? How would results change if instead the observation was assumed to be taken of the end of the month?

A4: We are not implementing a two-step approach. We are implementing a straightforward (i.e. real time assimilation) application of the EnKF by assimilating GRACE information every five days. The main advantage of doing so is that the model is run once rather than twice, which significantly reduces our computational burden and makes the problem a lot more feasible. This is particularly relevant if we consider implementing our scheme over larger areas. If the observation were assumed to be some sort of snapshot taken at the end of the month, we would see two effects: 1) A phase shift of half a month. Right now, interpolating between the middle of the months keeps the monthly mean correct. 2) Assimilating with a single update per month would apply all of the increment at once, potentially leading to a significant step change in TWS as mass is added or removed. In addition to the step artefact in the TWS time series, this sudden injection/withdrawal of water can lead to implausible discontinuities in the streamflow time series.

Q5: Temporal correlations: Observations are assimilated every 5-days. This is done after the temporally interpolating observations. Isn't this interpolation introducing an implicit temporal correlation across the assimilated observations? The EnKF assumes that each observation is independent from each other but the 5-days temporal interpolation includes temporal correlation. Did the authors consider the effects of their 5-days interpolations in the assimilation scheme? For example, how would results change if instead a different temporal window (lets say daily or every 15 days) is chosen for interpolation? Or how would results change if none interpolation was done after all and perhaps observations were assimilated only at the end of a month?

A5: It is true that interpolation is introducing an implicit temporal correlation across the assimilated observations, which is currently not considered. Assimilating with a single update per month would apply all of the increment at once, potentially leading to a significant step change in TWS as mass is added or removed. This sudden injection/withdrawal of water can lead to implausible discontinuities in the streamflow time series. Assimilating observations more frequently results in smaller increments. However, it is also essential to give the ensemble an opportunity to grow between updates and avoid ensemble collapse. The five day interval was chosen through trial-and-error to be a good compromise between allowing the ensemble to grow between updates and avoiding implausible discontinuities. We observed that using 10 day interpolated observation led to larger RMSE of the estimated streamflow. This discussion will be clearly stated in the revised manuscript.

Q6: Spatial correlations of the GRACE observations: I read from http://grace.jpl.nasa.gov/data/gracemonthlymassgridsland/ that "The spatial sampling of all grids is 1 degree in both latitude and longitude (approx. 111 km at the

Equator). However, this does not mean that two neighboring grid cells are 'independent' because spatial smoothing has been applied" this means that spatial correlations between neighboring GRACE-TWS pixels should be applied. It seems that the authors did not consider observations spatial correlations in their EnKF, is it correct? If so what is the rationale for not including it?

A6: In this study, we implement a so-called "1D-EnKF" in which each grid cell is updated individually. As shown by De Lannoy et al. (2009), working with a spatially distributed state vector can lead to an improved estimate. Given the coarse resolution of GRACE, we expect that implementing our framework with a 3D-EnKF (De Lannoy et al., 2009) would lead to an improved performance. Our focus was more on comparing the four model scenarios, rather than the observation error structure. Nonetheless, more recent studies have explored the effect of spatial aggregation of GRACE TWS prior to assimilation (Forman and Reichle, 2013) as well as inclusion of the full GRACE error structure (Eicker et al., 2014). Combining the advances made in these studies with our assimilation framework can be expected to yield a more realistic estimate. We thank for the referee suggestion and will add this remark into our conclusion.

Q7: Figure 2/or add to the text: : : can the authors add a schematic representation of the model? E.g. it would be useful to understand what exactly upper/lower (UZ/LZ) mean in terms of the actual model physics. In the same figure, of text can the authors described how is soil moisture (SM) defined (e.g. depth? rootzone only? surface+rootzone? etc)

A7: OpenStreams-wflow defines UZ and LZ as the upper and lower groundwater zone, respectively. SM represents the surface and rootzone. In the revised manuscript, the schematic representation and the description of the physical meaning of the storage will be clearly explained.

Q8: Please avoid the usage of "later" e.g. in section 2 toward the end of the first paragraph

A8: The usage of “later” has been removed from the manuscript.

Q9: Can the authors add orographic contours on the Figure 1. Also the text oftentimes refers to the “Alps” region, could you please add this label in Figure 1.

A9: Topography layer and the label “Alps” will be added to Figure 1 in the revised manuscript.

Q10: Table 4-5 are very hard to read, maybe can group these by regions identified in Figure 1. Or perhaps help the reader by highlighting which stations improved or not upon the open loop case?

A10: We thank for the referee suggestion. In the revised manuscript, the values will be grouped based on the groundwater network (as in Table A1).

Additional References:

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Forman, B. A. and Reichle, R.: The spatial scale of model errors and assimilated retrievals in a terrestrial water storage assimilation system. Water Resour. Res., 49, 7457–7468. doi:10.1002/2012WR012885, 2013.

Eicker, A., Schumacher, M., Kusche, J., Döll, P., Müller Schmied H.: Calibration data assimilation approach for integrating GRACE data into the WaterGAP Global Hydrology Model (WGHM) using an Ensemble Kalman Filter: First Results, Surv. Geophys., 35(6), 1285-1309, doi:10.1007/s10712-014-9309-8, 2014.

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