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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the use of GRACE intersatellite tracking data for improved estimation of soil moisture and groundwater in Australia" *by* Natthachet Tangdamrongsub et al.

Natthachet Tangdamrongsub et al.

natthachet.tangdamrongsub@newcastle.edu.au

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We would like to acknowledge the insightful comments and suggestions provided by reviewer 2. We will consider the reviewer's suggestions in our revised manuscript. Followings are the responses (R) based on the comments:

Line 152: Equation 9 is the most important equation in this study, but some of the information is provided in the later section 3. Also the model covariance matrix is provided in section 4.2. Authors might consider making the method section clearer and reduce some unnecessary equations.

R1: We thank for the reviewer's suggestions. We organize the paper's content in this way to separate the methodology and implementation of the proposed approach. Section 2 describes the methodology of the GRACE-GC in general, which can be simply applied to any GRACE data or land surface model. Later on, sections 3 and 4 describes the specific implementation with the ITSG data and CABLE model, respectively. We believe this is a logical presentation of our methods. This is explained in the manuscript lines 94 – 98: "Firstly, the derivation of GC approach is presented in Sect. 2 while the description of GRACE data processing, including the use of GRACE normal equation is given in Sect. 3. Secondly, the CABLE modelling is outlined in Sect. 4. This includes the derivation of model uncertainty based on the quality of precipitation data and the model parameter inputs."

Line 170: Basically, the paper claimed "the use of intersatellite tracking data", but the data was the normal matrix N and vector c obtained from the ITSG-Grace 2016 as well as the gravity field coefficient from GOCO05s solution. No Level 1B data was actually used directly in this study, so I wonder whether the title is appropriate.

R2: We agree with reviewer. Although the normal equation is constructed based on the L1B data (see lines 172 – 174 in the submitted manuscript, "All L1B data including KBR inter-satellite tracking data, attitude, accelerometer, GPS based kinematic orbit data and AOD1B corrections are reduced in terms of the normal equations"), we understand that the title might be misleading. Therefore, based on reviewer's suggestion, we will consider changing the paper title to "On the use of GRACE normal equation for improved estimation of soil moisture and groundwater in Australia" in the revised manuscript.

Line 195: The GRGS gridded TWS products were used in Equation 9 to work out the TWS values outside Australia. The L3 GRGS products derived from the Earth's geopotential coefficients up to degree and order 80, while ITSG data used in the study were up to 90. Why not using the ITSG TWS data? Can the ITSG normal equation represent the uncertainty in L3 GRGS products?

R3: Eq. (9) requires the knowledge of \DeltaTWS outside the study region. However, the ITSG does not provide such a solution (L3). The official ITSG2016 solution is the unconstraint gravity field. Deriving \DeltaTWS from the unconstraint SHC requires filtering, which might lead to the alteration of the GRACE signal. This is the main reason we use the GRGS solution for such a purpose.

Line 210: The gridded GRGS data was resampled to 0.5 degree spatially, but the normal equation only contains the information to degree 90. How did you deal with the different spatial scale in the error variance-covariance matrix?

R4: This study uses the least-square combination approach in spatial domain. It is possible to compute the \DeltaTWS from the normal equation at any spatial scale (here is 0.5 degree). However, it is noted that these 0.5 degree data are spatial correlated, and the correlated error is already accounted in the GC approach described in Sect. 2.

Line 231: depth between 0.022 m not cm

R5: Reviewer is correct. We will correct 0.022 cm to 0.022 m in the revised manuscript.

Line 256: The sensitivity study of the model parameters is an important process but not necessary to show in the paper. Author may consider removing table 2.

R6: We thank for reviewer's suggestion. However, we find Table 2 contains some insight information about the model, which are useful for the readers. Therefore, we will keep Table 2 in the revised manuscript.

Line 298: Did you do the CDF matching for few years and validate the results for the rest of time period? Or did you match all the time series and validate the same time period? If so, your estimates and observations are not independent. The CDF matching may discard important signals of the observations. Since only correlation was calculated, CDF matching is not necessary.

R7: We built the CDF using 2003-2004 data and applied it to the rest. As the NS coefficient (not correlation coefficient) is used in this section, the remaining bias might

result in poor NS values, the bias correction is then necessary. For clarity, we will add the additional statement in to the revised manuscript as follows: "The CDF is built using the 2003-2004 data, and it is used for the entire time period"

Line 301: The variability of soil moisture inside a basin is quite high. The average of basin and monthly soil moisture can smooth out lot of signals. Since your output is 0.5_x 0.5_ gridded products, why not validate at this scale instead of basin scale? Can you show some validation with in-situ measurements?

R8: We thank for reviewer's suggestion. We also conducted the analysis at 0.5x0.5 scale, and it provides very similar conclusions to the basin mean. Both results do not show the significant improvement, which is supported by the recent publication (Tian, et al., 2017). Moreover, we did not have an access to the in situ data by the time of this study. Therefore, the validation with in-situ measurements is not conducted in this paper.

Line 307: The groundwater estimates were only validated for two states using the state average. It should be possible to validate all the states over Australia or at basin scale to be consistent with other results. Two states are not sufficient to support the improvement in groundwater storage estimates over Australia.

R9: We agree with reviewer that the validation over all the states is important. However, the ground observation network of Australia is very sparse, and particularly we did not have an access to the data of other states by the time of this study. Therefore, we only validate the result in all possible states of Australia, Queensland and Victoria.

Line 320: Is that only one value of specific yield per state was used to convert the groundwater level to storage? Will it be more appropriate to use different specific yields for different locations and calculate the average?

R10: We agree with reviewer that it is ideal to conduct the conversion at all grid cell independently. However, such information is unfortunately not available. Therefore, we

exploit the best knowledge by obtaining the values from the published literatures for the conversion.

Line 468: The difference between model and GC approach for soil moisture is marginal here for basin monthly average. Can you show some time series examples of GC results and AMSR-E retrievals?

R11: Below are the examples of the time series between our estimates and AMSR-E (C-band): [Figure 1] The statistical value can be found in Table 3 of the submitted manuscript. As stated in the manuscript, no significant change is seen in GC solution, likely due to limitation of GRACE's temporal and spatial resolution. This same conclusion was reported by Tian et al. (2017). Therefore, we decide not to discuss it further in our manuscript. Instead, we provide a reference and recommendation how SM estimates can be improved in the conclusion section of the submitted manuscript.

Line 482: In Table 4, the trend of GC approach is about one third of in-situ measurements for Queensland. What causes such big difference?

R12: As a matter of fact that the GC approach optimally combines the GRACE observation with the model results, the GC result is inevitably influenced by the model estimate. As seen in Fig. 7 and 9, the GC result moves toward the in situ measurement but it is still influenced by the \DeltaGWS estimation from the model.

Line 491: It will be interesting to see the groundwater storage change in Murray-Darling Basin after the GC approach compared with in-situ measurements, during the big drought and big wet period.

R13: The reviewer's suggestion is very interesting. However, we did not have the in situ groundwater data of the Murray-Darling Basin by the time of this study. We will consider the validation in the Murray-Darling Basin in our future study.

Line 502: This section investigates the mass variation in the past 13 years based on the GC approach. Figure 8 is a quite good demonstration of the mass variation at dif-

ferent layers of water storage. The top and root-zone soil moisture show quite different trends. The root-zone soil moisture has similar trends with TWS and groundwater for most of the basins. It will be better to have some validation of root-zone soil moisture estimates and more sufficient groundwater storage estimates to support the analysis in this section.

R14: We appreciate reviewer suggestion and we strongly agree. However, similar to what we stated above, we did not have an access to the in situ root-zone soil moisture of Australia, and the validation of such a component was not possible.

Line 579: 0.39 is App1 and 0.42 is App2? So the trend calculating from GRACE subtracting modeled soil moisture is the same with modeled groundwater trend (in Table 4). The NS value for App1 is 0.46, which is less than the CABLE model without GRACE? For Victoria, the NS value of App2 is 0.3 less than CABLE model without GRACE too. With the assimilation of GRACE in App2, the correlation is degraded. It seems model itself without GRACE is better compared to App1 or App2. Still, for only two states validation results, it's hard to demonstrate that GC approach works better due to the error information. It could be your model uncertainty is better estimated using the ensembles as explained in Section 4.2. When you do the App1 and App2, did you also used 7 precipitation dataset as the same as the GC approach? Please clarify.

R15: Reviewer is correct that App1 and App2 provide poorer results compared to CABLE (in some case) or GC approach. However, this section demonstrates the scenario when different \DeltaGWS computation approach are used (not GC). The model part remains the same (as in GC approach). App1 uses mascon solution with error free scenario while App2 uses mascon with its variance matrix, and the different outputs are mainly attributed to the different application of the uncertainty type. The poor results of App1 and App2 are mainly due to too simplified error information implemented. For clarity, we will add the additional statement in the revised manuscript as follows: "The model uncertainty remains the same as in GC approach (Sect. 4.2). The different

outputs between App1 and App2 are mainly attributed to the different application of the uncertainty type."

Line 587: The future work in this section is interesting but no results were provided. Author may consider removing this section completely or providing the results in this paper together with the GC approach.

R16: We agree with reviewer. Section 6.2 will be removed from the manuscript.

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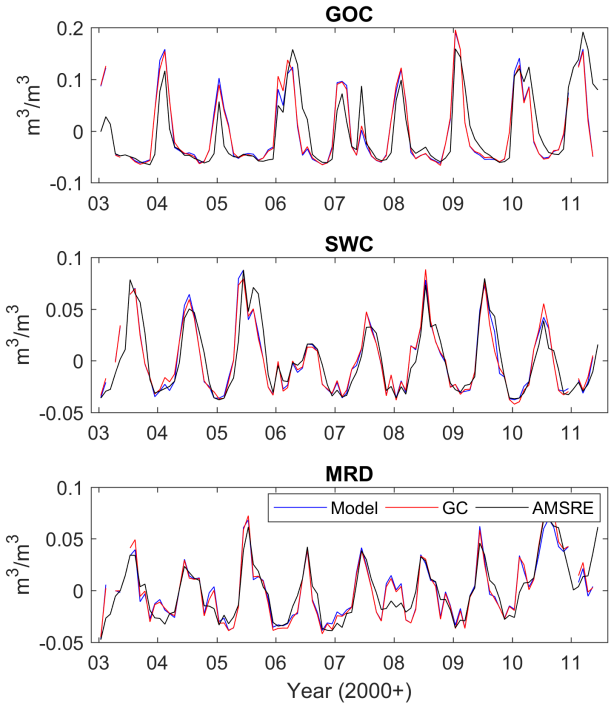

Fig. 1.