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Hydrology and Earth System Sciences Discussions

Interactive comment on "The bias in GRACE estimates of continental water storage variations" *by* R. Klees et al.

Anonymous Referee #4

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General comments: This manuscript evaluates GRACE monthly mass change observations over Central Africa and compares the data with model outputs from the LEW model for 4 different basins. The paper is nicely written and the problem is introduced in a very efficient way which allows the reader to easily comprehend the problem, the approach and the results. The results are interesting and surprising at the same time. This requires an in-depth evaluation of the approach and the conclusions drawn from the results. Firstly, the authors assume that GRACE products always underestimate the water storage variations after a Gaussian filter of a certain correlation length has been applied. The authors use the method outlined by Swenson and Wahr, 2002 and 2006. It has been accepted by the GRACE community that a filtering process is required to

eliminate stripes in the monthly gravity field solutions. Different filters have been proposed, but the authors limit themselves to the Gaussian smoothing filter after Swenson and Wahr. Secondly, the authors use the hydrologic model LEW to estimate the water storage variations over the same time period from 2003 to 2006. There is significant expertise among the authors to guarantee a proper performance of the LEW model. However, the limitations herein lies in the fact that its is the only model which has been compared and which has been used to estimate the a priori information to build the filter. It is clear that this does not allow an independent assessment of GRACE or model uncertainties. Thirdly, the authors claim to be able to test the uncertainties caused by the model by doing a Monte Carlo simulation of the model with 200 zero mean white noise added models. This is not enough to quantify uncertainties of the LEW model as the noise is just filtered by the model and does not represent independent model output which would be required to be able to identify the differences between models. The last paragraph provides such a comparison with the CPC-LDAS model. This comparison shows significant differences which indicate that different models must be used to assess model uncertainties. Certainly, the area of study is not targeted by many modelers and not much data is available which would allow for a proper calibration of GRACE data or a hydrologic model. Fourthly, The authors assume that the GRACE data quality is homogeneous throughout the 3 years which is certainly not the case due to different orbit constellations which result in different coverage and different aliasing mechanisms. Some GRACE data products are available to degree 120, some only to 70. The quality and resolution of the particular monthly solutions must be reflected in the bias estimated herein. It must be assumed that similar heterogeneities must exist in the data which is assimilated into the LEW model from a very heterogeneous distribution of the observation network.

The paper is technically sound and interesting by suggesting a bias correction method which result in a better fit between the GRACE and the model output, however, there are significant limitations in this approach as only one filter is used and only one model is employed. It is not clear why the target area was choosen as it must be one of the

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less observed areas in the world. The conclusions are encouraging, but not proven to be correct. The authors claim that a bias correction is always neccessary also in the future to use GRACE data for assimilation into models. This should not be the case once the aliasing and orbit characteristics and the accelerations are properly accounted for. It cannot be a solution to smooth GRACE data to make it useful. There is reason to do this in a first approach, but cannot be the longterm goal of the GRACE community to proceed like this. The study is interesting and should be published, but some issues should be corrected or better explained before publication. We are not requesting a new study using a different model, but the conclusions drawn are not validated by the approach taken in this study. Please discuss your results in comparison with Crowley et al, GRL 2006, Vol 33.

Specific comments: page 3560, l15: "It is subject of optimal filter design to find a filter that minimizes the sum of GRACE errors and filter errors". It is important to note that the two error types cannot be separated, on top of that, model errors come in and complicate the problem. page 3562,l9: Is the model LEW considered perfect when estimating the a priori information? Please comment on this at this section. page 3564, l20: Please comment on the degree of the monthly GRACE data. page 3566, l 23: What would happen if the units were not given the same model stuctures, how much does the output change? page 3568, l18: Why do you feel that a rough estimate is adequate? page 3568, l25: Please mention that these interactions have not been included herein. page 3570, l8: The degree 1 and 2 have been removed. The reason is because they are not well constrained by GRACE, but other systems, e.g. SLR could deliver good values, why did you not restore these degrees from other validated sensors/models? And who defines the adopted procedure (reference). page 3570, l14: Interpolation, is it not possible to obtain the excat time step from the model? page 3570/3572, l28+: the differences depend on the correlation length. The authors claim it is an effect of residual GRACE noise and LEW uncertainties. This is an easy way out but does not help to pin down the sources of this effect. Please elaborate on this effect a little more. page 3572, l21: "It could be attributed to either the model or GRACE".

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This statement makes the entire study obsolete, since the goal was to estimate the bias caused by GRACE filtering and not to estimate the bias caused by GRACE filtering AND LEW uncertainties. page 3572, l26: Orbit changes lead to aliasing changes and different quality in GRACE monthly data. This must be included and corrected for. Bad month will certainly cause a different bias in GRACE, but maybe not in the model. Maybe there is a way of investigating this which allows to separate the GRACE and model errors using different quality GRACE data. page 3572, l24+: The entire monte carlo simulation does not really contribute to the quantification of the LEW model uncertainties. The only outcome is that we can see what the LEW model does with white noise in the rainfall data. It does not reflect the model uncertainties as we can see in the comparison with CPC-LDAS. The latter is a much more valuable comparison to quantify model uncertainties. The section on Monte Carlo simulation should be modified to reflect that. Figures 9 demonstrates that the different models show variations in the differences which are not just annual as in Figure 8, where the biases of Monte Carlo simulations are shown.

Technical comments:

page 3582: Reference Han et al must have another author between C.Y.K. page 3571, l5: TABLE 3

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3557, 2006.