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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A global water cycle reanalysis (2003–2012) reconciling satellite gravimetry and altimetry observations with a hydrological model ensemble" *by* A. I. J. M. van Dijk et al.

A. I. J. M. van Dijk et al.

albert.vandijk@csiro.au

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We thank the referee for these valuable comments. The reviewer makes several specific comments that we will address in revising the m/s. Below we respond to the 5 main concerns raised (letters between brackets are sometimes added for cross-referencing where there are multiple sub-comments).

COMMENT 1) A lot of assumptions about data errors (systematic, random, as well as error structure in space and time) are made. As mentioned, I think this is good, since

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they would remain unused if the authors would not have considered them, but how do these assumptions on errors impact your results? In fact the conclusions drawn from this paper are difficult to judge, as they could easily change significantly if other assumptions on errors would have been made. (a) To name a few: all models are forced by the same forcing (combination of Princeton forcing and TRMM). This makes the outputs more correlated and therefore could result in underestimation of errors. (b) Second, GRACE models are also dependent on the same data. Are the errors of GRACE data also underestimated because of this? (c) Hence, the sensitivity of the results to the chosen error sizes as well as the chosen error structure (non-correlated in space and time, which is doubtful to my mind) should at least be properly discussed. E.g. is the conclusion that 0.39 mm yr-1 of ocean mass increase is missing from the water balance not an effect of uncertainty in the errors and therefore in the assimilation gains? Or even an effect of the length of the time series (only 10 years)?

RESPONSE 1) We thank the reviewer for stressing the important point that using observations as constraints demands some assumptions about their structure. (a) Technically only the W3RA model used the mentioned forcing, however it is true that the 4 GLDAS model outputs were all based on the same (GLDAS) forcing and so may well have had partially correlated errors. Although this did not affect the error estimates (only one model was used each time in triple collocation error estimation), the assimilation itself necessarily has the implicit assumption of uncorrelated errors in the ensemble, which is likely to have been violated to an unknown extent. (b) Yes, the GRACE products are partly derived from the same primary observations and hence there may have been correlated errors between the GRGS and Tellus products, which we dealt with by inflating the calculated errors (see Section 2.4, page 14 top paragraph). However we note that a manuscript has just been published (Ensemble prediction and intercomparison analysis of GRACE time-variable gravity field models, C. Sakumura, S. Bettadpur and S. Bruinsma, GRL, DOI: 10.1002/2013GL058632) that demonstrates that the different GRACE retrievals we used have errors that are substantially independent, which provides additional confidence in the triple collocation approach used. (c)

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In the absence of better information it is typically not possible to judge what influence the error structure assumptions introduced. However what we could establish is that (i) the gain matrix is actually not affected much by error inflation chosen, because the model and GRACE errors are generally of quite different magnitude (see Fig 2), and (ii) the 'missing' 0.39 mm y-1 is not due to our error assumptions but inherited directly from the GRACE products (see last paragraph Section 4.2, page 23). Also note that we did not discover but simply confirmed the well-documented sea level closure problem and found some evidence that the explanation proposed by Chen et al. (2013) may not fully solve it. However this was of course not the focus of our study.

COMMENT 2) In more detail, triple collocation requires that errors do not vary over time and errors are not correlated in time (p. 15487, l. 14-17). For GRACE errors, this could be true, but for the hydrological models this could be very wrong, especially in areas where storage change is strongly dependent on rainy seasons. In these seasons, the hydrological models will produce much larger errors in the rainy season than outside. Again, if not considered the effect of this assumption is an important point for discussion.

RESPONSE 2) Agreed, and we can discuss this point explicitly. Note however that only the (temporally stable) gain matrix is affected by this; in disaggregating the analysis update the errors are derived from the ensemble and therefore are temporally dynamic.

COMMENT 3) There's no mentioning of spatial correlation in errors. Is this considered by the triple collocation technique? If not, again implications on results need to be discussed.

RESPONSE 3) We are not entirely sure what errors the reviewer refers to. Triple collocation acts on single grid cell, but as Fig 2 shows there is much spatial correlation in the derived error estimates. This correlation is combined with the spatial correlation in the (coarse) GRACE signal and imparted in the analysis update step. That in turn will have been propagated in the disaggregation step, and combined with the spatial

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correlation in the priors. Hence most spatial correlation is preserved.

COMMENT 4) Section 2.5, p. 15489, l. 19-22. A linear relationship between river levels and discharge is assumed. It is not clear to me why this was necessary. In somewhat broader rivers you may expect that the relationship (i.e. a rating curve) reads as Q = a(h - h0)b. And therefore, logQ = log a + b log(h - h0). So a linear relationship between logQ and water levels may be assumed and h0 tuned to make the relationship linear. Why was this reasoning not used?

RESPONSE 4) In fact we did not assume a linear relationship. We expected a non-linear relationship and that is why we calculated Spearman's rank correlation coefficient rather than Pearson's r (Section 2.5, p. 15489, line 21)

COMMENT 5) In section 3, many observations in the results are made that remain unexplained. Please consider hypothesizing what the observations may imply.

RESPONSE 5) Where we could identify a probable explanation we suggested on this in Section 4, but overall we were hesitant to over-interpret the results. However the reviewer has indicated some specific examples in the annotated m/s that we can and will address in revising the m/s. Overall these analysis results are quite novel which necessarily limits our ability to compare to previous studies.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 15475, 2013.