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Hydrology and Earth System Sciences Discussions

Interactive comment on "Global runoff over 1993–2009 estimated from coupled land-ocean-atmosphere water budgets and its relation with climate variability" *by* S. Munier et al.

Anonymous Referee #2

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Munier et al. assess the inter-annual variability of global continental runoff based on both a land-atmosphere and an ocean-atmosphere water balance approach. For both budgets, the atmospheric part (net precipitation) is estimated using the ERA-Interim reanalysis product. The storage change term is based on land surface model outputs and altimetry-based ocean surface variations for the land and ocean water budget, respectively.

Interestingly enough, the results in terms of global runoff variability of the two water budget approaches correspond well. The authors relate the runoff variability to large-

scale climate patterns such ENSO. The authors do a good job in assessing uncertainties of their water budget approach by considering complementary data sets, e.g. different precipitation products, GRACE-based water storage variations, and three different continental hydrological models. As a side-product, by comparing the land and ocean water budgets, the study proposes an alternative way of estimating the ocean thermal expansion term to resolve ocean mass variations from altimetry-based ocean surface variations.

The study makes an interesting and novel contribution to global hydrology worth of publication in HESS. It gives valuable insight into global-scale water balance terms and their inter-annual variations for the last about 15 years, including some idea of their uncertainties. In general, I suggest several minor to moderate revisions of this manuscript to improve the clarity of methods and results following the comments below.

Specific comments:

1) The focus of this study is the inter-annual variability of global continental runoff. The authors do not give absolute values (mean value removed, page 4642, line 23). Thus, the title ("Global runoff over 1993-2009 ...") is somewhat misleading. An alternatives could be "Global runoff anomalies ...".

2) A major result of this study is that no significant trend in global runoff is observed. However, in Chapter 3.1 on data processing, the authors state that they use de-trended land and ocean storage terms in their water balance approach. What is the impact of this de-trending on the finally estimated runoff trend? Doesn't it force trend-free runoff dynamics? While derivating storage with time in the water balance equation leads to a constant in runoff for a certain time step, this may sum up to variations / trends over long time scales? The authors should clarify this point.

3) Similarly, what is the implication of the positive storage trend observed in the land surface model (LSM) time series for the 21st century (Figure 3)? While the inter-annual variability is in reasonable correspondence to GRACE water storage variations, this

trend behaviour is not. This LSM trend may also have an effect on the estimated runoff (trends), since LSMs and not GRACE are used for the water balance. By the way, it is surprising that all three LSMs show the same trend dynamics. The authors may extend the discussion on this issue and clarify whether they used the LSM data with trend or de-trended.

4) Page 4646, lines 2ff: The authors argue that their result of global runoff without trend may partly be explained by a different time period than in Syed et al. (2010). What is the result of this study for this particular time period 1995-2006 and how does it compare to Syed et al. (2010)?

5) Chapter 5, ocean thermal expansion (TE). The reasoning leading to equation 8 is not fully clear. How is the equation derived? What is meant with "a time invariant interannual variability"?

6) A major concern is the authors' statement that their reconstructed TE time series is "in quite good agreement" (page 4649, line 12) with other time series (IK09 and WOD09) (Figure 10). Except for a seasonal signal I do not see any reasonable agreement between these times series, in particular not for inter-annual variability. In addition, given a 3 month lag (page 4649, line 9) that is not further discussed in the manuscript has been corrected for, the conclusion that the time series are in good agreement cannot be drawn in my view. This part of the discussion should be rewritten. Actually, there is not necessarily a need to demonstrate similarity with IK09 or WOD09 as the authors suppose limitations of these data anyway (Chpater 3.4).

Minor comments:

(a) page 4645, line 2: "the discontinuity correction": make clear what this means: correcting for an offset of 2.78 mm?

(b) page 4645, lines 23ff: trend units: "km3 month-2" should read "km3 month-1"?

(c) legend Figures 2b and c. Add mid latitude "ocean", and high latitude "ocean".

(d) Figure caption 10: note that the reconstructed time series have been shifted by 3 months (backward/foreward?).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4633, 2012.