Hydrology and Earth System Sciences Discussions

Interactive
Comment

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.

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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 ...".

We agree with this remark and the title will be changed adequately.

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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.

The runoff has been estimated using land and ocean storage terms detrended or not. Results are identical, in terms of interannual variability and runoff trends. At this time scale (17 years), the impact of de-trending these terms can be clearly considered as negligible.

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.

We first remind that for the comparison between LSM and GRACE over the period 2002-2009, both signals have been detrended. Over this period, the GRACE trend is 1.28 km3/month2, whereas it reaches 1.39, 2.59 and 1.33 km3/month2 for ISBA, WGHM and LaD, respectively. GRACE trend is then of the same order than ISBA and LaD trends, and WGHM shows a twice higher trend. Although this difference is out of the scope of the study (it would require a separate study), it shows the importance of considering several models to estimate the global runoff. As written in the manuscript, discrepancies between the models give an order of the uncertainties related to model-

ing errors.

In Figure 3a, the LSM and GRACE signals have been detrended only for the comparison. If the time span of the study were reduced to the period 2002-2009, the trend over this period would have had no impact on the interannual variability (as said previously), and in this case, the graph shows a good correlation between LSM and GRACE. Nevertheless, LSM estimates of land storage are not detrended over this specific period for the computation of the global runoff over the whole time span. At a longer time scale, this positive trend is included in the interannual variability and will tend to reduce the global runoff over this period.

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)?

With the methodology developed in this study, the global runoff trend over the period 1995-2006 equals 1.07 km3/month2 for Rl and 1.19 km3/month2 for Ro. While these values are greatly higher than for the period 1993-2009 for the reasons developed in the article, they are still lower than the value obtained by Syed et al. (2010). The difference remains quite large and comes likely from different datasets used to compute P-E over oceans. Namely, Syed et al. (2010) used OA-Flux and HOAPS datasets for Eo, and figure 6 shows large differences between them in terms of interannual variability.

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"?

Combining equations (1-2) with the assumption Pl-El=-(Po-Eo) leads to

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Sl+So=constant. Then combining this equation with equation (6) leads to equations (8) after having removed the constant. A sentence will be added in the revised version.

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 (Chapter 3.4).

The terms "quite good agreement" may be attenuated. Figure 10 shows that our TE reconstruction and IK09 and WOD09 have similar high frequency behavior, but significantly differ in the low frequencies. In addition to the uncertainties of the three time series, our TE estimation differs from the two others by the fact that deep variations of TE (bellow 700m) are supposed to be accounted for, whereas they are not in IK09 and WOD09. Even though we expect these variations to be negligible (p4639 l15-18), they may have a low frequency behavior that could explain the differences with our reconstruction. Moreover, IK09 and WOD09 estimates may suffer from irregular data sampling of ARGO sensors, potentially leading to errors at the global scale. This part of the article will be rewritten accordingly.

Minor comments:

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

(b) page 4645, lines 23ff: trend units: "km3 month-2" should read "km3 month-1"? *No. Runoff trend is expressed as units of runoff (km3 month-1) per unit of time, i.e. km3 month-2.*

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

(d) Figure caption 10: note that the reconstructed time series have been shifted by 3 months (backward/forward?). *forward. The word will be added.*

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