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Interactive comment on "Assessing global water mass transfers from continents to oceans over the period 1948–2016" *by* Denise Cáceres et al.

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We thank referee #2 for the very pertinent comments and questions, as well as the positive feedback. We have listed the referee comments and respective replies below. Each set of referee comment (RC) and corresponding author comment (AC) is identified by a number.

1.

RC: My first concern is the selection of GRACE solutions. Obviously, GRACE solutions are important here as they were used as validation benchmarks. The authors applied the ensemble of four spherical harmonics (SH) solutions. I agree with the authors that the derived water storage trend can be sensitive to different GRACE solutions.

Therefore, I wonder why the authors opted to only use SH solutions but not mascon solutions. Mascon solutions have merits in improved resolution and signal isolation. I am wondering what kind of influence the inclusion of mascon solutions will have on the model validation (I know some of the mascon solutions result in a smaller global trend, meaning the modeled LWSA trend even more overestimated). I would like to see more justifications, or at least a little discussion, about the selection of GRACE solutions.

AC: We agree that, in principle, mascons may be beneficial for many applications, especially in basin-scale analysis. However, this is not our use-case. Deploying mascons was initially discussed internally, but subsequently not considered for several reasons (without order):

- Being part of the ESA CCI Sea Level Budget Project (SLBC_cci) let us aim for a complementary GRACE product to ocean mass change with preferably identical corrections and uncertainty assessment procedures, i.e. a degree-60 spherical-harmonics (SH) based approach that includes recent Glacial Isostatic Adjustment corrections after Caron et al. (2018), which is – to our knowledge – not available with mascons. In the meantime, there may be options without individual corrections included (e.g. CSR), but then the entire approach runs the risk of losing consistency.

- Mascons generally use geophysical a-priori constraints / regularization in space and time, which we find too interfering. The implementation of time-correlation as in JPL-RL06M introduces (small) changes to previous months after an update, which affects trend determination and comparability.

- We undertook an in-depth uncertainty assessment for the SH approach, which we understand much better than mascon uncertainties, where provided. Ours includes a combined time-dependent estimation of contributions from low-degree replacements (geo-center, flattening), GIA, leakage and noise, which lets us derive uncertainty ranges for individually picked temporal base-lines, as promoted in SLBC_cci.

- One of the key elements in our GRACE continental solution is an oceanic leakage buffer: we implemented a dedicated procedure in order to integrate out-leaking signal over the oceanic area and correct for mean ocean change therein. For this, we need to employ a specific latitudinal-dynamic buffer mask (∼300 km) that exactly nestles to the WGHM land-water mask. This is not easily accomplished with the mascons' dedicated masks, also not with JPL's CRI version. Even though mascons are (by design) not as affected by leakage as SH solutions, mascons would still require additional coastal processing due to fractional land-ocean separation. The mean-ocean-mass correction for such cells would require a product dedicated to ocean-mass and not ocean bottom pressure, as commonly provided with some mascon solutions. Adjusting for this effect is possible but, again, adds to inconsistencies in the overall approach.

- If gain factors were to be used, they could not be consistently applied in our global approach, as this also includes combined glacier- and hydrologically affected cells.

We could theoretically derive solutions solely based on mascons for comparison, yes. However, these would not fulfil our requirements in a consistent way. Therefore, we believe it would not be worth the effort for this study. With regard to filtering and statistical noise-cancelling, we should also say that our approach is suitable for the global case, but may be significantly weaker in regional applications.

2.

RC: Second, as the authors specified, the Caspian Sea is not included in WaterGAP, so the modelled LWSA excludes the impact of the level decline in the Caspian Sea (which can be substantial). The GRACE solutions, however, include the Caspian Sea in the land mask. I am unclear, at least given what the authors described, whether the GRACE TWSA used for model validation excludes the Caspian Sea or not. If not, the comparison wouldn't be apple-to-apple (GRACE TWSA with the Caspian Sea impact and modeled TWSA without), which leads to an even greater overestimation in the modelled water storage trend.

AC: Our GRACE continental mass change estimates do indeed include the Caspian

Sea area. In our global approach, it would technically not be possible to treat it consistently as an ocean, primarily for the following reasons: (1) we extend the continental integration kernel onto the ocean area for leakage, but need to subtract the mean ocean change therefrom; which does not connect to the Caspian Sea for obvious reasons. (2) In recent versions of the ocean correction, we restore AOD1b (GAD) background models; which do not exist for the Caspian Sea, because it is not an ocean. And (3) if we wanted to apply the extended leakage-mask technique to the Caspian Sea, the latter would almost entirely be covered by the integration kernel and, hence, still contain the mass change signal. Which, in turn, would have to be corrected for unknown mean 'ocean' (Sea) mass change each month. One might just mask out the Caspian Sea from the integration kernel, but thereby one would also introduce methodological inconsistency. We will discuss internally, whether such an option would be more beneficial than a trend-wise discussion. In either case, we understand that this issue needs more clarification.

3.

RC: Third, the modeled impact of reservoir water impoundment may be underestimated, particularly in the recent couple of decades. This is because WaterGAP incorporated the initial version of GRanD (version 1.1), rather than the updated version 1.3 that added another 458 large reservoirs mostly constructed after the year 2000. As a result, the modeled reservoir impact was underestimated after 2000. This underestimation was seen in Figure 6, where LWSA_res flattens and declines during the recent decades. In addition, as the authors mentioned, GRanD only includes the largest reservoirs. Medium-sized and smaller reservoirs, such as partially documented in ICOLD, may add up to a substantial impoundment volume that was not taken into account in the model. These limitations (leading to underestimated LWSA_res) ended up overestimating the declining trend in net human impacts (LWSA_human). The authors may want to discuss more about these limitations about modeling reservoir impacts, and how they affect the conclusions.

AC: We thank you for this very pertinent comment. We recognize that the modelled impact of reservoir water impoundment is underestimated by the model, particularly during the last decades of the studied period, due to the fact that the additional reservoirs documented in GRanD v1.3, as compared to GRanD v1.1, are not accounted for by the model. There are ongoing efforts to incorporate these reservoirs in WaterGAP. However, this enhancement is still in progress and will only be available upon the release of the next model version. Therefore, as suggested by you, we will discuss about this limitation and how it affects our results and conclusions. The following text will replace L. 640–643 in the discussion:

"Wada et al. (2017) might overestimate the additional water due to seepage, as well as the fraction of the design capacity that is in reality filled (85% according to their assumption). However, the estimate of our study is likely an underestimation of the impoundment of water in man-made reservoirs because WaterGAP only simulates the largest reservoirs and does not account for seepage. In addition, WaterGAP incorporates the reservoirs from the GRanD v1.1 database, but not the additional ones from the new GRanD v1.3 release (http://globaldamwatch.org). GRanD v1.3 includes 458 additional reservoirs as compared to GRanD v1.1. Out of 458 reservoirs, 447 were put in operation between 1948 and 2016. Out of these 447 reservoirs, 173 have a total capacity of at least 0.5 kmˆ3 and thus would be simulated as reservoirs by WaterGAP. The cumulated total capacity of these 173 reservoirs amounts to 599 kmˆ3. The remaining 274 smaller reservoirs have a cumulated total capacity of 62 kmˆ3. Out of the 173 large reservoirs, 164 were put in operation between 2000 and 2016. Taking into account that we computed an actual total water impoundment of roughly 63% of the global reservoir capacity, we can infer that incorporating the additional large reservoirs would lead to an additional impoundment of 378 kmˆ3 (1.05 mm SLE) over 1948–2016, thus increasing total impoundment of water from 8 to 9 mm SLE, i.e. from 22 mm LWH to 25 mm LWH (compare Fig. 6). Most of the additional impoundment not taken into account in this study (369 kmˆ3, 1.02 mm SLE) occurred in the period 2000–2016. Therefore, WaterGAP is expected to overestimate the contribution of water storage

on the continents during the GRACE period by approximately 0.06 mm SLE/yr, which explains part of the overestimation as compared to GRACE (Table 3)."

4.

RC: Line 558: "closer to the estimate of Reager et al. (2016)". I found this sentence far-fetching as the estimates of Reager et al. (-0.71 mm per year) and of the authors (-0.41 mm per year) have a difference of a factor of 2.

AC: This sentence will be modified as follows:

"the trend from Reager et al. (2016) suggests that Wg_gl underestimates continental water mass gain due to climate variability; by assuming GRACE-based TWSA, we obtain a more negative LWSA_clim trend, however still differing from the estimate of Reager et al. (2016) by roughly a factor of 2"

5.

RC: Lines 660–661: This sentence can be misleading. 0.109 mm per year from 2002 to 2014 as reported in Wada et al. (2017), includes the contribution from both the surface water in the Caspian Sea and its influenced groundwater. 0.114 mm per year from 2002 to 2016 as reported in Wang et al. (2018), is the contribution of the entire Caspian Sea Basin, not the Caspian Sea alone. Based on Wang et al. (2018), the contribution of the Caspian Sea alone, i.e., the trend in its surface water volume, is 0.071 +/- 0.006 mm per year (or 25.52+/-2.12 Gt per year). For improved clarity, I recommend that the authors modify this sentence to be something like: "The contribution of this endorheic lake to SLR was estimated to 0.109 +/- 0.004 mm SLE per year (including variations in both surface water and the influenced groundwater) during the period 2002–2014 (Wada et al., 2017) and 0.071 +/- 0.006 mm SLE per year (including only surface water variation) during the period April 2002 to March 2016 (Wang et al., 2018)."

AC: We thank you for having pointed out this misinterpretation of ours. We will modify the sentence as proposed. In addition, we will also cite a trend reported by Milly et al.

(2010); 0.06 mm yr-1 SLE over 1992–2002.

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