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Hydrology and Earth System Sciences
Discussions

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 #1 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: The knowledge gap or research questions addressed by this manuscript are not clear. [. . .] First, the question is what is the real aim of the manuscript? I think that the authors should state more clearly the research question or hypothesis driving this study, than just focusing it as a new version or integration of WaterGAP. Which is

the real aim of the study: 1) to quantify (or rather update or give other versions of) glacier and non-glacier contributions to sea level, with more emphasis on glacier or non-glacier?, 2) to validate TWSA from WaterGap with GRACE, to update WaterGap with the glacier module integration? It is not clear at all. I recommend a main focus, with appropriate redistributed weight across the manuscript. [. . .] The introduction should be restructured to really focus on the research question addressed, the identification of knowledge gaps and objectives specifically the knowledge gaps. Glaciers, water use, sea, ocean mass change. I know that combining all of these concepts together and reducing the state of the art to an introduction or discussion is not easy, but I think it is worth the effort. There are many efforts attempting to do similar objectives, with completely different methods, that should be mentioned (for glacier melt contributions, impoundment and withdrawals of water, etc). If GRACE is not the main objective, I suggest reducing considerably the emphasis on it in the introduction. Also, missing important references [. . .].

AC: The aim of the study was stated in L. 104–105:

"Our assessment aimed at quantifying this contribution during the period 1948–2016, as well as identifying its main drivers and components."

However, we agree on the fact that the knowledge gaps, aims and research questions of the study need to be expressed in a more structured way and formulated more clearly. Therefore, we will conscientiously re-formulate the introduction, making sure that all of these elements are clearly addressed and that the connection between them is apparent. Moreover, we will reduce the emphasis on the comparison to GRACE, since this is not the main focus of the study. With respect to the research questions, they will be formulated in the form of a list as follows:

"Through this comprehensive assessment, we aim to address the following questions:

1. How did changes of total water storage on the continents of the Earth (except Greenland and Antarctica) contribute to ocean mass changes (and thus sea-level rise)

during the period 1948–2016? (section 3.2.1)

2. Which continental storages underwent the most significant mass changes during this period? (sections 3.2.1 and 3.2.4)

3. How have man-made reservoirs and human water abstractions affected water storage on the continents? (sections 3.2.1 and 3.2.2)

4. What were the main climatic drivers of glacier-free land water storage changes? (section 3.2.3)

5. To what extent can we rely on our modelling approach to quantify global-scale water storage changes on the continents? (sections 3.1 and 4.2)"

Note that the order of the research questions intends to convey that the long-term assessment of TWSA and individual components (research questions 1, 2, 3 and 4) is the main focus of the manuscript, whereas the validation of the applied modelling approach (research question 5) is a secondary focus that is indispensable, as only based on this validation can the reader judge the reliability of our assessment. The fact that the main focus is disaggregated into four research questions shows that this main focus is broad and points towards an exhaustive assessment of TWSA and individual components. Note also that the section number(s) indicated in parentheses for each question corresponds to the new structure of the results and discussion sections (see AC 4).

2.

RC: I also think that the manuscript is too long, giving too much information on all models and related details, and limited in the discussion and comparison with other studies.

AC: We generally agree on the fact that the manuscript is too long. Therefore, we will reduce to some extent the information on models, data sets and methods as described in AC 5. Furthermore, Table 5 will be shortened (see Table 1 in the supplement of

the response) by moving part of the results to the supplementary information, and the introduction and conclusions will be shortened as far as possible. On the other hand, we disagree with the suggestion that our manuscript is limited in the discussion and comparison with other studies for the reasons stated in AC 3.

3.

RC: Also, the authors need to put their results in the context of the urgency to understand TWSA and OMC that precisely requires these results. [. . .] And again, the discussion, a comparison with other works is necessary, and to put the results of the study in context. Some of the references I here mention could help, as many others. Discussion is completely missing. For instance, can these estimates be compared in some way with others? Anomalies related to water storage and/or consumption by irrigation and reservoir impoundment (Chao et al., 2008; Hoekstra and Mekonnen, 2012; Jaramillo and Destouni, 2015; Stefanie Rost et al., 2008), glacier contributions to oceans (Braithwaite and Raper, 2002; Giesen and Oerlemans, 2013; Huss and Hock, 2015; Jacob et al., 2012; Meier, 1984; Radić and Hock, 2014).

AC: We have rather extensively discussed our results in the light of other works. For instance, in section 6.1.1, we have compared our linear trends of TWSA, LGWSA, LWSA, LWSA_hum and LWSA_clim to estimates from multiple recently published studies (see Table 6 in the manuscript). Furthermore, section 6.2 also contains numerous references to other studies. In section 6.2.2, global glacier contributions to oceans are discussed (1) by referring to studies (Marzeion et al., 2015; Slangen et al., 2017) in which the results of the glacier model used in our study have already been compared to other models, including the ones from Radić et al. (2014) and Huss and Hock (2015), (2) by comparing our estimate to the one from Zemp et al. (2019), which provides the state-of-the-art observational baseline of global glacier contributions to oceans, and (3) by referring to the results of Hirabayashi et al. (2010), who evaluated the mean seasonality computed by their model in a comparable way to ours. In section 6.2.3, we have compared our estimate of the contribution of global reservoir impoundment to oceans to the most comprehensive assessment done up to now (Wada et al., 2017), which is based on and updates the results of Chao et al. (2008). As to groundwater storage anomalies related to irrigation groundwater abstraction, we have discussed this in relation to other studies in a qualitative way only; we recognize that some quantitative comparison to other estimates is missing here.

Some of the references that you mention can be of interest for qualitative discussion. Rost et al. (2008) quantified the impact of anthropogenic land cover change on the global terrestrial water balance by applying a dynamic global vegetation and water balance model (LPJmL). Their conclusions can be of interest to discuss the implications of lacking this influence in our assessment (section 6.2.4, "Missing components") due to the fact that WaterGAP does not include land cover change. However, many other references are not very relevant or suitable for comparison in our opinion. For instance, the estimates reported by Jaramillo and Destouni (2015) are relative to 100 large basins which represent 35% of the global land area excluding Antarctica, and thus could not be compared to our global-scale estimates. Moreover, given the fact that the manuscript is already very long, we do not intend to include too many further references.

In the revised manuscript, we will compare our groundwater depletion estimates to other works and will include a reference to the study of Rost et al. (2008). As to the need of putting our results more in the context of urgency, this will be resolved by re-formulating the knowledge gaps in the introduction (see AC 1).

4.

RC: I find also some issues with structure, that again, break the main thread and hide the really important results. [...] I have a feeling that Section 6.2 does not belong there, and it is also containing much information that it is not important and dilutes the main message of the manuscript, from my perspective. But again, I may be wrong, depending on the main aim of the article. Can much of it be moved to Supplemen-

tary information. Instead, a good discussion of the results and comparison with other studies could fill that gap.

AC: We consider that the content of section 6.3 (we assume that the referee comment refers to section 6.3 and not 6.2) should be left as part of the manuscript, rather than moved to the supplementary information. As mentioned in AC 1, our assessment of TWSA and individual components is intended to be exhaustive. Even if we did not disaggregate LWSA_clim into individual contributions (as opposed to LWSA_hum, which was disaggregated into LWSA_res and LWSA_abs) due to technical limitations of the model and general complexity, we still intended to investigate the main drivers behind this component. This will be reflected by research question 4 (see AC 1). This being said, we agree that the placement of this section in terms of structure is not ideal, and thus will move it from the discussion to the results (new section 3.2.3, see below). Also, we will make the content of this section more concise. The structure of the results and discussion sections will be modified as follows:

(3) Results

(3.1) Model evaluation

(3.1.1) Comparison of observed and simulated annual and seasonal glacier mass changes

(3.1.2) Comparison of observed and simulated global mean TWSA during the period January 2003 to August 2016

(3.2) Global water transfer from continents to oceans over the period 1948–2016

(3.2.1) Contributions of glaciers (LGWSA), climate-driven land water storage (LWSA_clim) and human-driven land water storage (LWSA_hum)

(3.2.2) Contributions of reservoirs (LWSA_res) and water abstractions (LWSA_abs)

(3.2.3) Relation between climate and land water storage

(3.2.4) Contributions of individual water storage compartments

(4) Discussion

(4.1) TWSA temporal components: seasonality and long-term variability

(4.1.1) Linear trend: comparison to independent estimates

(4.1.2) Seasonality

(4.1.3) Residual: long-term non-linear variability

(4.2) Limitations of study

(4.2.1) Glacier data integration approach

(4.2.2) Global modelling of LGWSA

(4.2.3) Global modelling of LWSA

(4.2.4) Missing components

5.

RC: The methods section is very hard to read, or in other words, very hard to focus while reading it. I assume it starts in L. 120 and finishes in L. 330? This should be explicit. [. . .] The information on models and data sets is important and should be generally included in a methods section, but in this case, due to the massive amount of information due to the complexity of the study, I suggest just leaving the most important methods and sending the details to supplementary information. Also, because now the red thread is completely lost by the end of the methods, and since the objectives are not very clear in the introduction.

AC: We agree that the information on models and data sets should be part of the methods section. Therefore, we will merge sections 2 and 3, “Models and data” and “Methods”, respectively, into one section entitled “Models, data and methods”. The structure of this section will then be:

(2) Models, data and methods

(2.1) Models

(2.1.1) Global hydrological model (WaterGAP)

(2.1.2) Global glacier model (GGM)

(2.2) Data

(2.2.1) Modelled land water storage change (LWSA)

(2.2.2) Modelled land glacier water storage change (LGWSA)

(2.2.3) Glacier mass change from in situ observations

(2.2.4) GRACE-derived total continental water storage change (TWSA)

(2.3) Methods

(2.3.1) Integration of GGM glacier data into WaterGAP

(2.3.2) Model experiments

The content that will be assigned to section 2.2.3 will roughly correspond to the content of the old section 3.1 (“Evaluation of GGM glacier mass change with in situ observations”), but more concise and focused on the observed glacier mass change data set. Section 2.3.2 corresponds to the old section 3.4 (“Overview of WaterGAP variants”).

Concerning the amount of information on models, data sets and methods, we are of the opinion that most of it should remain in the main text, because it is necessary to understand the results. We are also encouraged to do so based on the feedback of referee #2, who states that, although the manuscript is very lengthy, the methods are well elaborated, and the technical details help readers understand and potentially replicate our work. Nevertheless, for the sake of reducing the length of the manuscript, we will make the content of section 2.1.1 (“Global hydrological model (WaterGAP)”) more concise, we will move the old section 3.2 (“Pre-processing of gridded GGM output data") to the supplementary information and we will modify Table 1 to make it more compact (see Table 2 in the supplement of the response).

6.

RC: There are so many acronyms, too many, I would just use the most important ones. For example what is the purpose of OMC, it is not a variable.

AC: We agree that the vast number of acronyms makes it difficult for the reader to stay focused. Therefore, we will not use the following acronyms, as they are not considered essential: SLR (sea-level rise), OMC (ocean mass change), SWB (surface water bodies), SMB (surface mass balance) and GWD (groundwater depletion). Furthermore, in paragraphs which contain too many acronyms (e.g. L. 464–471), we will replace part of them with the corresponding long names in order to reduce the load of acronyms. Moreover, in Table 5, we will include both the long name and the corresponding acronym of each variable (see Table 1 in the supplement of the response).

7.

RC: The results. . .where do they start by the way, in Model evaluation? Also, are they focused on the comparison with GRACE? That is why I ask if that is the main aim of the article.

AC: In order to avoid confusions as to where the results begin, we will modify the structure as shown in AC 4.

8.

RC: For figure 4 and 5, can I recommend an additional simple barplot figure showing the TWSA with and without the accounting of glacier melt, with uncertainty ranges?

AC: We are not sure what you meant here. Would the values (bars) represented in the barplot still represent the TWSA time series (i.e. one bar per month/year), or would they represent the linear trends of contribution of TWSA to ocean mass change over

the period considered by the respective figure? In any case, for both Figure 4 and 5, we do not see how adding an additional barplot would help the understanding of the reader.

However, in the case of Figure 5, including a barplot alongside each graph with the corresponding linear trends over 1948–2016 could be beneficial. First of all, the barplot related to the upper graph (TWSA) would replace Table 4 in the manuscript and, in this way, contribute to save some space (see Figure 1 in the supplement of the response). Secondly, the barplot related to the bottom graph (TWSA components) would allow us to include these additional results (see Figure 1 in the supplement of the response).

9.

RC: Another suggestion, a brief explanation somewhere how the term "anomaly" on land and change of mass in the ocean are related.

AC: The following sentence will be added in the introduction:

"An overall positive (negative) water storage anomaly in a given compartment is equivalent to a water mass gain (loss) in this compartment, thus constituting a "negative" ("positive") contribution to ocean mass change."

10.

RC: The conclusions should also pinpoint the main objectives of the introduction, and focus in what is really important.

AC: the conclusions will be re-formulated to be more in accordance with the aims and research questions stated in the revised introduction (AC 1). In addition, the first paragraph, with summarises the modelling approach and its validation, will be shortened.

11.

RC: The y-axis of Fig. 4 and 5 mean different things but have the same level or no level at all. The level is missing, only units, and complicates the understanding of the results.

AC: At the end of the caption of Fig. 4, it is explained that "Anomalies are relative to the mean over the period January 2006 to December 2015 and given in millimetres of land water height (mm LWH)." The same sentence is found in the caption of Fig. 5, on that the anomaly is relative to the annual value of 1948. We are not completely sure what you meant by "the same level or no level at all", however we would say that the level (i.e. what a value of zero means) is well described by these sentences and the reference levels are different. We think it is better to not have the same reference level in both figures as the reference level in Fig. 4 is due to GRACE data availability only, while Fig. 5 is to show changes since the beginning of the study period.

12.

RC: L. 466 The word "contribution" is not appropriate here, I would delete it. In general, the use of the word "contribution" is very subjective, can you be more direct in its real meaning. What is a "contribution to ocean mass change" really, increase or decrease in ocean mass? Why is it an addition to the continents?

AC: In order to clarify what we mean by negative or positive contribution to ocean mass change, we will add an explanatory sentence in the introduction (see AC 9). In the discussion manuscript, we have distinguished between "contribution to ocean mass change" and "contribution to TWSA". The second expression was used when referring to mass changes of individual components of TWSA; in e.g. section 5.3, the water storage compartments that gain mass (e.g. reservoir) represent positive contributions to TWSA, whereas the ones that lose mass (e.g. glacier) represent negative contributions to TWSA. A positive (negative) contribution to TWSA is nothing more than a water mass gain (loss) in the continents, which translates into a negative (positive) contribution to ocean mass change. To avoid confusions, in the revised manuscript we will only use the word "contribution" in relation to ocean mass change. Regarding TWSA, we will simply refer to water mass gains or losses. Furthermore, we will modify L. 465–468

as follows:

"Glacier mass loss is the dominant component of the TWSA mass budget (Figure 5b), with LGWSA accounting for 81% of the cumulated water mass loss from continents to oceans over 1948–2016. Overall, the contribution of LWSA to ocean mass change, which is dominated by its human-driven component (Figure 5b), is also positive, representing 19% of the cumulated water mass loss from continents."

13.

RC: On the other hand, Table 5 is too complicated due to the amount of numbers and acronyms and the lack of explanations, maybe a Figure could be more illustrative? Many of the components have not been introduced before. Same for Figure 7.

AC: In order to reduce the complexity of Table 5, we will modify it (see Table 1 in the supplement of the response). The variables and corresponding trends that do not appear in the modified Table 5 (as compared to the one in the manuscript) will be moved to an independent table in the supplementary information. On the other hand, we disagree with the suggestion that many of the components in Table 5 and Figure 7 had not been introduced before. The components corresponding to the individual water storage compartments were introduced in section 2.1.2 ("Computation of LWSA").

14.

RC: With losses, do you mean just negative anomalies?

AC: That is correct. Negative water storage anomalies in a given compartment indicate water mass losses in this compartment (see AC 9).

15.

RC: L. 22–25 What do these results have to do with the main aim of the article?

AC: The main aim of the article is to give an exhaustive long-term (1948–2016) assessment of TWSA and individual components. The results that you pointed out give the contribution of the LWSA_res and LWSA_abs components to ocean mass change over the considered period, while relating the large mass decrease in the groundwater storage compartment to LWSA_abs. In addition, the LWSA_clim component is related to some of its main drivers. In the revised manuscript, these results will correspond to research questions 2, 3 and 4 (see AC 1).

16.

RC: L. 31 – Missing reference (Chao, 2008)

AC: We will add this reference in connection to this line.

17.

RC: L. 34–35 I don't see the purpose of this sentence.

AC: The purpose of this sentence is to introduce the mass component of sea-level change, i.e. ocean mass change. In the revised manuscript, the sentence will be slightly modified to:

"Primarily, sea-level change can be decomposed into a steric component (i.e. thermal expansion and salinity change) and a mass component (i.e. ocean mass change)."

18.

RC: L138–140 So why are you using them then?

AC: The WFDEI climate forcing is not optimal for trend analysis due to varying station density in space and time related to the bias correction of monthly precipitation sums based on observation-based products (GPCC or CRU data). Despite this non-negligible caveat, WFDEI is still considered the state-of-the-art when it comes to large-scale hydrological impact studies. For instance, it has been used to force multiple impact models such as global hydrological models in the framework of the Inter-Sectoral Impact Model Intercomparison Project (https://www.isimip.org).

Adjusting reanalysis as e.g. ERA-Interim (WFDEI is based on this reanalysis data set) by observation-based products such as monthly GPCC or CRU precipitation allows for the consideration of additional data to scale the monthly sums while keeping the temporal daily variability of the reanalysis (Weedon et al., 2011; Weedon et al., 2014). Together with the snow undercatch corrections that are included in some of the state-of-the-art climate forcings (such as WFDEI), this renders hydrological impact studies more plausible. Müller Schmied et al. (2016) show in their Table 4 the effect of precipitation monthly bias correction, e.g. PGFv2.1 incorporating monthly CRU TS3.21, which results in a close match to initial CRU TS3.21 data, but also the effect of snow undercatch correction on the other climate forcings shown there. Kauffeldt et al. (2013) show in their Figure 8 that physically implausible runoff ratios would be obtained based solely on precipitation observations (CRU) in high-latitude or high-altitude areas without the implementation of a snow undercatch correction method. Hence, the inclusion of monthly observation-based precipitation data sets together with snow undercatch corrections improves the usability of such reanalyses.

In the case of GPCC, the data center provides a wide range of products. The typical product that is incorporated in reanalysis for hydrological impact studies (as e.g. WFDEI) is the so-called "Full Data Monthly" product, described as follows: "for the period 1891 to 2016 based on quality-controlled data from all stations in GPCC's data base available at the month of regard with a maximum number of more than 53,000 stations in 1986/1987. This product is optimized for best spatial coverage and use for water budget studies" (https://www.dwd.de/EN/ourservices/gpcc/gpcc.html). It is the most comprehensive data set (in both, space and time) they currently offer for advancing reanalysis at the given spatial resolution of 0.5°. A specifically dedicated product for trend analysis from GPCC is "HOMPRA Europe (V1)", described as follows: "a homogenized and quality-controlled data product suitable for trend analysis. The product is provided for the years 1951–2005 and is based on 5536 stations" (https://www.dwd.de/EN/ourservices/gpcc/gpcc.html). Even though this product is better suited for trend analysis, it is restricted to Europe and to the period 1951–2005, and thus not useful for most hydrological impact assessments.

To sum up, we justify our choice of climate forcing data sets by a) the lack of global-scale long-term precipitation measurements to generate time series that are specifically suited for trend analysis and b) the raised value of reanalysis when additional precipitation observation data together with snow undercatch corrections are included. We agree that this is not well expressed in the manuscript and thus have modified the corresponding sentences as follows:

"The GPCC and CRU products used to scale monthly precipitation sums within WFDEI use the available number of gauging stations for each month. The variability in the number of precipitation observations over time makes the resulting precipitation data sets less suitable for trend analysis. However, as we are not aware of an available long-term global precipitation data set with high station density that could be used instead, note that the benefits of including those adjustments into reanalysis products due to e.g. the incorporation of snow undercatch corrections result in more plausible hydrological studies (Kauffeldt et al., 2013; Müller Schmied et al., 2016)".

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Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-664/hess-2019-664-AC1-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-664, 2020.