Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-281-RC1, 2019

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Widespread Decline in Terrestrial Water Storage and Its Link to Teleconnections across Asia and Eastern Europe" *by* Xianfeng Liu et al.

Anonymous Referee #1

Received and published: 8 October 2019

The manuscript "Widespread decline in terrestrial water storage and its link to teleconnections across Asia and Eastern Europe" by Liu et al., submitted to HESS, analyses the terrestrial water storage (tws) for regions with declining tws based primarily on GRACE, hydrological modelling data and literature values, links it to a huge number of teleconnections and separate tws both in seasonality and compartments and link it as well to teleconnections. While the manuscript started promising (and the idea of linking TWS dynamics to teleconnections is interesting), it has several drawbacks both structural and content-wise. Simultaneously, I have the impression that the manuscript was not prepared carefully and properly reviewed by the co-authors before the submission. Otherwise I could not understand the number of the major and minor very obvious

problems that made it hard to focus on the content of the manuscript. In sum, I have doubts, if a major revision could lead to an acceptable improvement for the high journal standard and therefore recommend to reject the manuscript but I of course leave it up to the editor if the chance for improvement should be given.

Major comment: The general objective of the paper is interesting (especial the link to teleconnections) but how the authors structured the manuscript is not convincing. The method section does not provide the details that are needed to understand the results. Should the reader know every single teleconnection? What are the methodological details of assessing water storage changes for lakes (e.g. are reservoirs included?), how are glaciers included (a reference to literature does not allow a reader to really get a clue how specifically the data has been included in this study)? Wetland and river storage seem to be missing at all in the study – at least those are not indicated in the definition or in data sets used. The results section contains a too short and selective description of the results, often followed by an interpretation. Should it be up to the reader what is the result of the study or the interpretation? There are questionable interpretation included, for example that the (very small) changes in precipitation is responsible for the (one magnitude higher) change in tws, or that glacier melt leads to soil moisture increase – without citing any reference. In the discussion section, the arguments of the results section are partly repeated. The authors are not embedding the findings of their study to the literature (except a very few examples), so it is hard to get a proper information of the robustness of their findings. Most disappointing I found is that for nearly every figure, major problems arise. Most of the diagrams do not even have a proper axis naming / labelling, so I have hard times to understand the results and the text that is based on it, all that made it hard to review the content. More specific, there are (other than mentioned in the state of the art) already a number of global / large scale studies that deal with those or a subset of those regions or even on global scale but often directly include anthropogenic impacts (by the way, those regions could have names), e.g. Wada et al, 2010, Döll et al, 2014, Scanlon et al, 2018, 2019, Syed et al, 2008, Tangdamrongsub et al., 2018, Zhang et al, 2017 and more, those and some of the references therein should be considered when re-designing the manuscript.

Specific comments: For the distinguishing of water storage compartments, a single mascon-solution and a single hydrological model is being used. Few years earlier that would have been state of the art, but now, and especially as a number of GRACE solutions (spherical harmonics and masons) and a large number of hydrological / land surface models are available, this kind of study should be done in a multi-model/multi-data setting to be able to verify the results, provide uncertainty information which then might lead to a valuable scientific contribution. To reduce the approach of the manuscript it to the minimum, the GRACE tws was reduced by NOAH soil moisture, snow and canopy, by lakes and glaciers; the leftovers are then groundwater and/or human interventions. Why have not the authors used a hydrological model (or better more) that consider human interventions, to allow direct assessment of trends / residuals? There are a number of global-scale studies that are using GRACE data in combination with global water models (Scanlon et al., 2018, Döll et al., 2014), especial to trends which contains also a huge list of references within for some of the regions of this study.

Line-by-line specific comments (not a full list, only the major things I stepped over during reading):

Line 72: The Mount Kilimanjaro comes unexpected in this list – isn't it located in Tansania (Africa), or is there also one in Asia?

Line 75: The sentence "Under the combined…" needs references or does it belong to the hypotheses?

Line 79ff: GRACE data, especially in the months at the end of the orbit time shows an increasing error in the signal – have you considered this in your analyses?

Lines 86-94 should be rewritten as it is repeating partly itself

Line 95: Whereas I agree that two things are comparable in general, please be concise in wording. One can compare an apple with an orange but this is not a good compari-

son. Comparing full TWS from GRACE with TWS from Noah that consists only of soil, snow and canopy leaves out important compartments such as water bodies, groundwater and glaciers. Of course, this is written in the next sentence but the word "directly comparable" is misleading.

Lines 98 ff: the description of how lake level and glacier change have been used in this study is much to short described. For lake levels – which lakes are included? Only the large ones? Are reservoirs included? Are wetlands included? Which time series are assessed? For example, Wang et al., (2018) ends in 2016, the time series of this manuscript exceeds this.

Line 101: If SW does not include wetlands or rivers (at least this information is missing in the manuscript), then the residual of GRACE TWS minus SW and SM cannot be groundwater only.

Lines 105 ff: The description of the TCs is not very informative. Please provide more details, e.g. for which region they are defined, how they are characterized (e.g. briefly in the supplement).

Lines 113 f: to which TWS does the section refers to? I guess to GRACE TWS, right? The section needs to be reformulated and streamlined for better readability and enriched by references, it reads confused in the current shape. What does the (total-trend-seasonality) mean? Is it a mathematical equation? Please provide details why by using the cross-correlation of the TWS residuals and TC the interference with (…) are reduced. This is similarly repeated in lines 144 f.

Line 144: For which GRACE solution the numbers are standing for? The mean of both? Fig 2c shows not "expected" changes in precipitation. And again, such a small precipitation trend in that region as shown in Fig 2b should not affect the tws signal drastically. Similar interpretation problems are following for the next case studies.

Line 158: The comparsion of Nort-West-India with one single reference is misplaced in the results section. Due to the reason the authors explain, it is not possible to assess the reason for the difference. I suggest to properly frame the trends into the various estimates that are available from the literature and then, in the discussion section of the paper to discuss it.

Line 161: What is the assessment of Caspian Sea Level is based on? Is that focus of the paper?

Line 163 ff: A mix of (selected) interpretation and presenting results, not easy to follow.

Line 169 ff: It is hard to accept that general conclusion that change in tws correlates with natural variability just because of (the magnitude lower) precip trend. This needs to be analysed in much more detail, especially the role of human interventions needs to be considered here (with data/modelling).

Line 170 f: A data product that base on the same satellite input but with a different processing is expected to lead to similar results (at least for the broad picture) especially for the highly human impacted regions. This does not allow justification of the results in my eyes. It could provide an uncertainty information, not more. A different measurement system (e.g. GPS displacement analysis) could be a real justification.

Lines 182 f (Most regions...): I do not agree to the described pattern.

Lines 194 ff: it reads like a new finding that at those locations, groundwater depletion occurs. There is a wide range of previous literature that directly assess regions with groundwater depletion based on GRACE (and hydrological models), e.g. Döll et al., 2014, Wada et al., 2010 and references therein.

Line 199 f: is there any reference that the glacier melt leads to higher soil moisture or is it an interpretation of the results? I am not an expert in glacier hydrology but would assume that the effect of a melting glacier to soil moisture increase is only locally effective and as soon as the glacier water is within a river, soil moisture is affected probably only weak, especially at a larger spatial scales.

Line 202: irrigated agriculture contributes to more than a half of of tws loss? How has this been assessed? Is assumed that irrigation only stems from groundwater resources? The following lines are already a discussion, it is hard to assess what is the specific contribution of this study.

Line 208: the authors refer to a meteorological drought the first time in the manuscript. Is it referring to declining precipitation from Fig 2b? Trends in precipitation does not necessarily imply a drought, this should be clarified.

Line 210: again, everything is comparable. But not everything is similar/equal. Please be concise with wording.

Line 214: which drought definition? TWS is not “recharged”, groundwater can be recharged. What does the word “will” mean? Climate projection? Water use projection? This is not clear.

Line 241: unit?

Section 3.2: I have hard times interpreting and justifying the results. First, maximum correlations are relatively low (Fig. S5) and I guess, only the TC with the dominant correlation is displayed in Fig 2. However, how to interpret plausible, if a correlation coefficient is, let’s assume 0.20 and the next TC has 0.19? The interpretation (such as time lag discussion) solely considers the maximum correlation even though it is in a large part of the study area very low. A correlation coefficient of 0.2 implies that this specific TC explains 20% of the TWS signal, is this correct? This needs more attention and maybe cutting out dominant TCs below a meaningful threshold.

Section 4.1 repeats mainly the interpretation of the results section. The last paragraph does not provide any scientific insights in terms of a discussion.

Section 4.2 is a description of the TC and in last two sentences it is stated that those TCs are impacting TWS. The reader does not have a much better idea how TWS is affected. And yes, there are methodological questions to solve.

Line 297 f: what is meant with TWS dynamics attributions? I fully agree that coupled human-natural approaches have to be done to better understand to which part TWS dynamics are due to natural or due to anthropogenic variations. This could be then connected with a link to TCs.

The arrangement of Figures is not consistent. Fig 2f is referred to before 2c-e, Figure S6 is referred to before referring to S3 etc. Please follow the journal guidelines which improves the readability. It seems that Fig S6 is the same like Fig 2f – is there any reason for this repetition? Fig. 2e is not referred to in the manuscript.

Fig 1 and lines ~75: sources are missing for definition of humidity and for area equipped for irrigation

Fig 2a and b and line 149 ff: I try to make sense out of the numbers and colours. TWS trend seems to be a magnitude larger then precipitation trend. How does a precipitation change of < 1 mm/yr can be the cause for 10 to 20 mm tws change? Precipitation can be a cause, yes, but if the numbers are correct, then I cannot agree that this is the reason and similarly I not agree that there where the pattern looks differently, human impact is the (only) reason. This needs by far more discussion and thorough analysis. From Table S1 some differences are visible for the two Mascon solutions. I suggest to display the two Mascon solutions in Fig 2. The regions in Table S1 could get names.

Fig 2c: check spelling of header text

Fig 2f: a legend is missing, and I can only see 4 lines and a mess of shaded area which does not allow any meaningful assessment. Please re-arrange (e.g. splitting it up to 5 single plots with same Y-axis) and it would be meaningful to use month/years for x-axis.

Fig 3: Labelling of Y-Axis with “Water loss” and then negative values – does it imply a water gain? Please name it more meaningful.

Fig 4: what can be seen at both axis? It seems that the months are not consecutive (If I

interpret it correctly as spring season), then drawing a solid line through it is misleading.

Fig S1: unit for Y-Axis is missing. I suggest to use month/years instead of month numbers. Why does the time series ends ~ at month 165 whereas the other figures are ending at month ~177/181?

Fig S3: what is shown at X- and Y-Axis? I have not checked if the references are listed in the reference list and vice versa, and also have not checked the reference list itself.

References Döll, P., Müller Schmied, H., Schuh, C., Portmann, F. T., & Eicker, A. (2014). Global-scale assessment of groundwater depletion and related groundwater abstractions: Combining hydrological modeling with information from well observations and GRACE satellites. Water Resources Research, 50(7), 5698–5720. https://doi.org/10.1002/2014WR015595

Scanlon, B. R., Zhang, Z., Save, H., Sun, A. Y., Müller Schmied, H., van Beek, L. P. H., et al. (2018). Global models underestimate large decadal declining and rising water storage trends relative to GRACE satellite data. Proceedings of the National Academy of Sciences, 201704665. https://doi.org/10.1073/pnas.1704665115ˆ

Scanlon, B. R., Zhang, Z., Rateb, A., Sun, A., Wiese, D., Save, H., Beaudoing, H., Lo, M. H., Müller Schmied, H., Döll, P., van Beek, R. Swenson, S., Lawrence, D., Croteau, M., Reedy, R. C. (2019). Tracking seasonal fluctuations in land water storage using global models and GRACE satellites. Geophysical Research Letters 46 (10), 5254-5264, 10.1029/2018GL081836

Syed T.H., Famiglietti J.S., Rodell M., Chen J., Wilson C.R. (2008). Analysis of terrestrial water storage changes from GRACE and GLDAS. Water Resour Res 44:W02433

Tangdamrongsub, N., Han, S.-C., Tian, S., Schmied, H. M., Sutanudjaja, E. H., Ran, J., & Feng, W. (2018). Evaluation of groundwater storage variations estimated from GRACE data assimilation and state-of-the-art land surface models in Australia and the North China Plain. Remote Sensing, 10(3). https://doi.org/10.3390/rs10030483

Wang, J., Song, C., Reager, J.T., Yao, F., Famiglietti, J.S., Sheng, Y., MacDonald, G.M., Brun, F., Müller Schmied, H., Marston, R.A., Wada, Y. (2018). Recent global decline in endorheic basin water storages. Nature Geoscience 11, 926-932, doi: 10.1038/s41561-018-0265-7.

Zhang L, et al. (2017) Validation of terrestrial water storage variations as simulated by different global numerical models with GRACE satellite observations. Hydrol Earth Syst Sci 21:821–837.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-281, 2019.