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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.

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We would like to thank the reviewers for their professional, detailed and constructive comments, which improved our manuscript considerably. We have carefully revised the manuscript following their comments point by point. Our revisions and explanations have been inserted in blue, and all amendments are also highlighted in the version of revised manuscript. Additionally, the writing of our revised manuscript are also under carefully editing by English native speaker with specialized in hydrology. Anonymous Referee #1 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. Response: Thank you for your comment. We feel sorry for the confusion and inconvenience we have brought to you. In the revised manuscript, we have substantially revised our manuscript according to the reviewers' comments. Major comment (1) The general objective of the paper is interesting (especial the link to teleconnections) but how the authors structured the manuscript is not convincing. Response: Thank you for your comment. We have reorganized the data and method, result and discussion section according to referee's comments, particularly in the result interpretation and discussion content. (2) 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. Response: Thank you for your comments. In our revised manuscript, we have tabulated the datasets used in our study. The lakes and glaciers that considered in our study are listed in the table (Table 1, see attached supplement, hereafter). The rivers and reservoirs indeed not included in our study, we have discussed the associated uncertainties in discussion section. We also made a methodology flow diagram of data processing in our revised manuscript (Figure 2, marked by the figure number in the revised manuscript and attached supplement, hereafter). (3) 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 the result of the study or the interpretation is? 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. Response: Thank you for your comment. We have substantially modified the inappropriate phrasing in results interpretation, and also added citations for each interpretation. Notably, the trend in precipitation was mistake in our former version of manuscript, we have recalculated and reproduced the spatiotemporal changes of precipitation over the study area (Figure 3). (4) 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. Response: Thank you for your comment. We have reorganized the discussion section according to reviewers' comments in our revised manuscript. (5) 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. Response: Thank you for your comment. We feel sorry for the inconvenience we have caused to you. In the revised manuscript, we have reproduced all figures according to the detail comments. We have attached all figures at the end of this response. (6) 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. Response: Thank you for your comment. We have carefully read these papers and properly cited them in our revised the manuscript. Specific comments (1) 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. Response: Thank you for your constructive comment. The spherical harmonic solutions generally suffer from correlated errors that manifest longitudinal striping in the gravity solution (Rodell et al., 2018). Although largely successful in removing errors, the post-processing also damps and smooths real geophysical signals (Landerer and Swenson, 2012). Recent advances in GRACE data processing have shown that solving for gravity anomalies in terms of mass concentration (mascon) functions with carefully selected regularization results in superior localization of signals on an elliptical Earth (Save et al., 2016). Therefore, two publicly available GRACE mascon solutions are employed in our study: Jet Propulsion Laboratory mascons RL05M (Watkins et al., 2015) (JPL-M) and Center for Space Research mascons RL05M (Save et al., 2016) (CSR-M). Notably, JPL-M has the unique characteristic that each 3° mascon element is relatively uncorrelated with neighboring mascon elements, whereas the 1° mascon elements in CSR-M solutions is highly correlated with their neighbors. Moreover, three degrees correspond approximately to the 'native' resolution of GRACE. Therefore, in this work we mainly used JPL-M for trend analysis and mapping. (2) Line 72: The Mount Kilimanjaro comes unexpected in this list – isn't it located in Tansania (Africa), or is there also one in Asia? Response: Thank you for your comment. The Mount Kilimanjaro is indeed located in Africa, we have corrected the mistake in our revised manuscript. (3) Line 75: The sentence "Under the combined: : :" needs references or does it belong to the hypotheses? Response: Thank you for your comment. We have rewritten the study area section and deleted this sentence in our revised manuscript. (4) 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? Response: Thank you for your comment. There are indeed certain months during which the GRACE orbit is in a near-repeat pattern. This phenomenon leads to sub-optimal spatial sampling and thus typically leads to larger errors in the higher spherical harmonic coefficients. The mascon solutions used in this study have already considered the measurement errors and leakage errors in the final data analyses data product. (5) Lines 86-94 should be rewritten as it is repeating partly itself Response: Thank you for your comment. We have rewritten the data section in our revised manuscript. (6) 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 comparison. 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. Response: Thank you for your comment. We have rewritten this section and revised the word "directly comparable" in our revised manuscript. (7) 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. Response: Thank you for your comment. We have listed the lakes and glaciers used in our study in table 1. But we did not include reservoirs and rivers parts in our study. We have discussed the associated uncertainties in discussion section as follows. Multiple uncertainties remain in understanding the changes in TWS and its components over the Asian and Eastern European regions. These may include the unaccounted for reservoir and rivers in surface water storage, which may induce uncertainties in a certain area in estimating the groundwater by deducting the surface water and soil moisture from TWS. The glacier data used here is during 2000-2016, this inconsistent with our study period (2002-2017) may also cause uncertainties in separating the water components from TWS. (8) 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. Response: Thank you for your comment. We indeed not consider rivers and reservoirs parts in our study. We have added the uncertainties in discussion section in our revised manuscript. (9) 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). Response: Thank you for your comment. We have supplemented the briefly introduction of the TCs in data section in our revised supplement. (10) 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 (totaltrend-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. Response: Thank you for your comment. Yes, this section refers to GRACE TWS, we have revised the statement. Also, we have reformulated and streamlined this section according to your useful comment in our revised manuscript. (11) 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. Response: Thank you for your comment. Both JPL-M and CSR-M show similar spatiotemporal pattern of changes in TWS (Figure 3 and Figure S3). Since the JPL-M solution has the merit of lack of correlation between neighboring mascon elements in the retrieval, in this work we use JPL-M for trend analysis and mapping. Notably, the trend in precipitation was mistake in our former version of manuscript, we have recalculated and reproduced the spatiotemporal changes of precipitation over the study area. (12) 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. Response: Thank you for your comment. We agree with your suggestions, and we have revised the sentences according to the comment in our revised manuscript. (13) Line 161: What is the assessment of Caspian Sea Level is based on? Is that focus of the paper? Response: Thank you for your comment. In this paper, we estimated the surface water loss by assessing the decline in water body level of Caspian Sea. The sharply declined in Caspian Sean level could better understand the loss of surface water storage. (14) Line 163 ff: A mix of (selected) interpretation and presenting results, not easy to follow. Response: Thank you for your comment. We have redesigned this paragraph in our revised manuscript. (15) 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) precipitation trend. This needs to be analysed in much more detail, especially the role of human interventions needs to be considered here (with data/modelling). Response: Thank you for your comment. The trend in precipitation was mistake in our former version of manuscript, we have recalculated and reproduced the spatiotemporal changes of precipitation over the study area. Challenges remain in separating the long-term relative roles of natural climatic variation and anthropogenic forcing on TWS changes. Well-designed experiments and coupled human-natural system models are still needed to clarify the quantitative contributions of each influencing factor on TWS in our future study. (16) 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. Response: Thank you for your comment. We have rephrased this sentence, and rewritten the results section in our revised manuscript. (17) Lines 182 f (Most regions: : :): I do not agree to the described pattern. Response: Thank you for your comment. We have revised this statement in our revised manuscript. (18) 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. Response: Thank you for your comment. We have carefully read these papers and properly cited in our revised manuscript. (19) 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. Response: Thank you for your comment. In addition to the glacier melt water, the increase in precipitation could also contribute to the increase in soil moisture (Figure 3). We have revised this sentence in our revised manuscript. (20) Line 202: irrigated agriculture contributes to more than a half 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. Response: Thank you for your comment. Actually, groundwater contributes to more than a half of TWS loss in region2 instead of irrigated agriculture. We have rewritten this part in our revised manuscript. (21) 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. Response: Thank you for your comment. We indeed inferred drought from declining precipitation, and we have rectified the statement in our revised manuscript. (22) Line 210: again, everything is comparable. But not everything is similar/equal. Please be concise with wording. Response: Thank you for your comment. We have replaced the word “comparable” of “similar” in our revised manuscript. (23) 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. Response: Thank you for your comment. We have replaced the word “recharged” of “changed”, and we also rephrased this sentence in our revised manuscript. (24) Line 241: unit? Response: Thank you for your comment. We have rectified the unit in our revised manuscript. (25) 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. Response: Thank you for your thoughtful comment. We indeed adopt the maximum correlation coefficient as the dominant TC. We also agree with your comment, and the situation mentioned above could occur in data processing. However, the pixel is independent each other. For each pixel, we could extract the maximum correlation coefficient between TWS and TCs, but we could not obtain the area proportion of each dominant TC during extraction process. Therefore, we adopted maximum correlations to interpretation, and we also discussed this uncertainty in discussion section of our revised manuscript. (26) 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. Response: Thank you for your comment. We have reorganized the discussion section according to the both reviewers' comments in our revised manuscript. (27) 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. Response: Thank you for your comment. We have added the possible impacts of TCs on TWS according to reviewers' comments in our revised manuscript. (28) 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. Response: Thank you for your comment. We have revised the statement in our revised manuscript. The coupled human-natural model is a promising and challenging issue that need pay more attention in our future work. (29) 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. Response: Thank you for your comment. We have reproduced all figures, and rearranged the sequence of figures in our revised manuscript. We have attached all figures at the end of this response. (30) Fig 1 and lines âĹij75: sources are missing for definition of humidity and for area equipped for irrigation Response: Thank you for your comment. We have supplemented the sources for definition of humidity and for area equipped for irrigation in figure caption. (31) 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. Response: Thank you for your comment. We feel sorry

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for the mistake in trend analysis of precipitation in our former version of manuscript, we have recalculated and reproduced the spatiotemporal changes of precipitation over the study area (Figure 3). (32) Fig 2c: check spelling of header text Response: Thank you for your careful comment. We have revised the spelling of header text in our revised manuscript. (33) 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. Response: Thank you for your comment. Since this figure mainly presented the TWS trend for five hotspots, which is similar to the figure 5 (see below). Therefore, we have deleted this figure in our revised manuscript. (34) Fig 3: Labelling of Y-Axis with "Water loss" and then negative values – does it imply a water gain? Please name it more meaningful. Response: Thank you for your comment. We have rectified this mistake, and replaced "water loss" of "water storage anomaly" in our revised manuscript. (35) 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. Response: Thank you for your useful comment. We have aggregated monthly data to yearly data in our revised manuscript (Figure 6). (36) Fig S1: unit for Y-Axis is missing. I suggest to use month/years instead of month numbers. Why does the time series ends âĹij at month 165 whereas the other figures are ending at month âĹij177/181? Response: Thank you for your comment. The total study period is during April 2002∼June 2017, but we use full years for comparison between 2003 and 2016, therefore the time series is during 1∼168. We have reproduced the figure by using month/year (Figure S1). (37) Fig S3: what is shown at X- and Y-Axis? Response: Thank you for your comment. We have reproduced this figure in our revised manuscript (Figure S4). (38) 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. Response: Thank you for your comment. We have carefully read the following papers, and properly cited them in our revised manuscript. References

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Figure 1: Boundary of the Asian and Eastern European regions. Panel (a) is the spatial

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distribution of arid and semiarid areas based on averaged aridity index during 2002-2017. The aridity index is calculated based on the ERA-Interim dataset downloaded from European Centre for Medium-Range Weather Forecasts. Panel (b) is the percentage area of irrigated land across the study area. The percentage area of irrigated land dataset is derived from Food and Agriculture Organization of the United Nations.

Figure 2: Methodology flow diagram of data processing in this study.

Figure 3: Spatiotemporal changes in TWS as obtained from GRACE (a) and precipitation as obtained from CRU (b) across the Asian and Eastern European regions during 2002-2017. The trend is obtained from the removed seasonal cycle time series.

Figure 4: Spatial distribution of cross correlation analysis between TWS and teleconnection indices. (a) Spatial pattern of maximum correlation coefficients between TWS and teleconnection indices. (b) Spatial pattern of teleconnections that can best represent TWS variations. (c) Spatial pattern of teleconnection lag time. (d) Proportion of the area dominated by each teleconnection and its corresponding time lags. The maximum lag in the correlation analysis was limited to 0$\sim$24 months (significance threshold: $|r| > \sim 0.15$ given a significant level = 0.05 and numbers of time series = 183).

Figure 5: Contributions of different hydrological storages to TWS changes in five hotspots. Uncertainties represent the 95% confidence intervals.

Figure 6: The residual time series of spring soil moisture and associated ENSO in region 3 during 2002-2017.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-281/hess-2019-281-AC1-supplement.pdf

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