Hydrol. Earth Syst. Sci. Discuss., 12, C3738–C3747, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C3738/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Satellite signal shows storage-unloading subsidence in North China" *by* J. P. Moiwo and F. Tao

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Received and published: 18 September 2015

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Dear Author Your manuscript has received three reviews. I would like to invite you to respond to these reviews as soon as possible in the public discussion. Once you have answered all reviews, I will take a decision regarding the revision of your manuscript. The public response to the reviewers does not need to address in detail technical language comments (it is sufficient to say that you will address these comments in the revised version).

Best regards

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Interactive Discussion

Discussion Paper

Bettina Schaefli, handling editor

Dear Handling Editor,

We thank you for successfully handling our paper till this stage. We have extracted the reviews in the public discussion and have responded to the issues raised in the reviews. We will also be addressing the issues in the revised version of the paper. We only hope that the revised version of the manuscript will be suitable for publication in the journal HESS.

Yours truly,

Juana P. Moiwo (Corresponding author)

Anonymous Referee #1 Received and published: 27 July 2015 The paper focuses on the detection of groundwater depletion in Northern China. Using GRACE and GPS data, the authors try to discriminate the groundwater storage change linked to physical changes of the aquifer systems, i.e., aquifer compaction. The original idea of the paper is interesting. However, the paper is difficult to read. It is not organised efficiently. For example, explanations on how the authors come to such large scale land subsidence estimations and how they interpolate GPS measurements are lacking. The paper needs more work before being published. However, the research presented here is interesting and we should see more studies using GRACE and concordant data such as GPS in the near future. The subject is well placed inside the journal scope. I encourage the authors in pursuing their work. It could become an interesting manuscript by following the recommendations I provide.

Response: We are very grateful to Referee #1 for the detailed assessment of our paper and for encouraging us to pursue the work further for publication in HESS. We will reorganize the work and follow the recommendations provided in both the general and specific comments. We hope that the revisions provided therein will be enough to allow publication of the paper. We once again say thank you.

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General comments: 1. It would be interesting to add few lines on the implication of discriminating the storage loss related to physical changes within the aquifer systems. Why does it matter for hydrogeologists? (i.e., unrecoverable storativity loss, the aquifers will never store as much if levels recover).

Response: As recommended, we will add a few lines (under Introduction) on the implications of discriminating storage loss related to physical changes within aquifer systems, including the examples given above.

2. It is important to use the common language shared with other authors of the field. I also recommend restoring the terminology used by other GRACE users (e.g., SWS is Surface Water Storage, SMS is Soil Moisture Storage). There is also an overuse of new unknown acronyms (e.g., WSD, LCS, GWSA, etc.). The paper should be reviewed by a native English speaker.

Response: We will use the common language shared by other authors in the field and avoid overuse of new unknown acronyms. The paper will also be revised by a native English speaker.

3. There are basically five figures showing the same results. The results are shown in monthly averages, seasonal averages and yearly averages. There is no need for so many figures for the GRACE part. It is also hard to discriminate in-situ measurements from GRACE-derived observations within the figures.

Response: We will limit the figures to seasonal and yearly averages but will keep the discussions on the average monthly and seasonal analyses to illustrate both the GRACE storage and field-measured storage behavior temporarily.

4. 'Based on GPS data analysis: Please show results of the analysis. Maps? Where are located these GPS measurements? How these points represent the large scale land subsidence? At the very least, one figure should be added on GPS data location and results.

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Response: Based on the recommendations of Referee #3, we will reduce the study area to Beijing Environ, an area that is small enough to be representative of the GPS data but equally big enough for GRACE data application. With that, we will show the location of the acquired GPS data and also provide a map derived from the results of the GPS data processing.

Specific comments: Fig. 1: Please add the study area shape on the map and units in the lower left caption.

Response: In the redefined study area, we will add the shape of the region in question along with the units in the lower left caption.

L16-17 p.6044: The authors are referring to an infrastructure development project in the abstract without giving any explaining on it. Maybe this is better in the discussion rather than the abstract.

Response: This will be deleted.

L3 p.6047: GLDAS is not a mission, it is a dataset.

Response: Correction will be adopted.

L1 p.6048: This is potential ET.

Response: Correction will be adopted.

L17 p.6057: 'Since there are hardly any occurrences of earthquakes or large-scale earth-faults in the region' and then in the conclusion: 'Droughts, degenerated water/wetland ecosystems and earthquakes are variously associated with long-term WSD'. Please conclude for what you observe in your study area first.

Response: In L17 P6057, which is under the main title "Results and Analysis" and subtitle "land subsidence", we were simply trying to build a case for the possible causes of land subsidence in the study area. We tried to eliminate the possibility that land subsidence in the region was due to factors other than water storage depletion. Then under

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"Discussion", it is normal to put one's research findings in context of other people's research findings. That is all what we tried to do, which could not be contradictory.

Anonymous Referee #2 Received and published: 28 July 2015 The paper has a major scientific significance and the subject being discussed is multidisciplinary. Since land subsidence is related to aquifer which is both a geologic and a hydrologic system, the type of formations in the study area should be furthermore introduced in the paper. The magnitude of the subsidence in relation with the aquifer media (matrix) should be assessed. Could the authors claim the same conclusions if the formations were merely clayey, sandy or silty? Variability of land subsidence with the type of aquifer formation, not only the groundwater head lost due to extraction stress!

Response: We would also like to thank Referee #2, first of all, for the positive remark on our paper, and then for the crucial suggestions. Due to the variability of land subsidence and limited availability of GPS data, we will then limit the study area to the Beijing Environ of North China Plain (for which area we have GPS data). This was also recommended by one of the Referees in the public discussion. With the much smaller study area, we will provide additional information on the formations in the study area and discuss the variability of land subsidence with various formations.

Some minor syntax review should be carried out by the authors and/or a third party (i.e., line 25 page 6051) to improve the clarity of the paper. The authors must be consistent with the vocabulary used in the paper (i.e., hydraulic conductivity not hydrologic conductivity).

Response: We will enlist the service of a third party for consistency in syntax and vocabulary to enhance the overall clarity of our paper.

Line 22 page 6061: I wonder how using salt water could be of any good at mitigating land subsidence by enhancing GWS. On the contrary, the result could be pollution and loss of valuable, good quality groundwater.

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Response: This will be removed.

Anonymous Referee #3 1. General comments This paper focuses on the water storage change and subsidence in North China region, which are caused by excessive groundwater exploitation. Although the topic of the study is suitable to the scope of this journal, there are following problems in the paper.

Response: We thank Referee #3 for finding the paper suitable for HESS and for also bringing to light critical issues on the paper. In the best way possible, we will address the issues raised in the revised version of the paper.

(1) The comparison between groundwater storage anomalies derived from field observation and from GRACE and GLDAS has already studied in detail with similar approach by Feng et al. (2013). Although the Shandong region was not included in Feng et al. (2013), the water decrease in the region does not remarkable.

Response: Irrespectively, the decline in groundwater storage in this study was large enough to be related to subsidence in the region. This is significant for research, planning, policy judgement, food production and social stability in the region.

(2) It is interesting to connect land subsidence observations with groundwater withdrawal and loss of aquifer volume. However, the authors use only one station (Beijing) data. It is too rough to estimate the average value of the study area using the one station data. The subsidence in Beijing is extremely large compared to other areas, and does not represent the value of the whole study area. I suggest cutting most part of the topic of (1), and discussing more critically the topic of (2). One way for the revision is that the authors focus on the small area (but large enough to GRACE resolution) of Beijing and the surrounding area. Another way is to use multiple (at least several) GPS station data sets, which distribute uniformly over the large study area.

Response: This remark is crucial and we are grateful for the highlight. Because reliable long-term GPS data are hard to locate in the region, we will reduce the study area to

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the Beijing Environ, which is large enough for GRACE data application.

2. Specific comments (1) Introduction 1) pp. 6046, l. 2: “There are several other reports”: The authors should review previous studies in more detail. Especially, they should state what has already done as well as what is the new discovery in this paper.

Response: We will give additional details under “Introduction” on the findings from other studies on groundwater withdrawal and land subsidence in the area.

2) pp. 6046, l. 16: “field measured groundwater and soil water storage”: It is unclear whether the soil water storage data shown in Figure 2 to 6 field measured data or GRACE/GLDAS-derived data.

Response: In Figure 1 (bottom right plate), measured soil water storage (otherwise given as soil moisture storage in other studies) is plotted against GLDAS-derived soil water storage as validation analysis. Following this, the plots of soil water storage in the subsequent figures are based on GLDAS-derived soil water storage. It is just this straightforward.

(2) Materials and method 1) pp. 6048, l. 21 – pp. 6049, l. 20. This part is not required for the discussion in this paper and should be removed.

Response: We believe that this does not adversely divert the focus of the paper and we will retain it.

2) Figure 2 to 6 and Table 1. Storage anomaly and storage change are shown in the figures and the table. However, storage changes are just shown, but are not discussed. Therefore they should be removed. Furthermore, it is not required to show all the figures (i.e., monthly, seasonally, yearly) because the scope of this paper is long-term storage variation only.

Response: For analysis of land subsidence, storage change is not as relevant as storage anomaly. Thus we will drop storage change in the Figures. Following the comments, we will also refocus our analysis on only seasonal and annual trends to depict

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storage depletion and land subsidence in the study area.

3) pp. 60051, l.8-16. The description of this part is not a standard style of GRACE analysis and should be rewritten. It is strange to apply Wahl's Gaussian filter after applying Swenson's filter. The authors should also mention which version of the GRACE data used in this study (e.g., release 4, release 5).

Response: The entire paragraph will be rewritten, Wahl's Gaussian filter will be removed and the version of GRACE data used stated.

4) pp. 6053, l.20-21. What is the difference of "average random error" and "average error"?

Response: This statement will be rewritten. It was supposed to be random error and average error.

5) The authors should state the method of GPS data analysis in more detail.

Response: We will provide more details (with maps) on the GPS data analysis.

(3) Results and analyses 1) pp. 6055, l. 25-27. Is the phase difference really due to the problem of GRACE sensitivity in short-term?

Response: At least this was what we arrived at, based on our analysis. It could also be due to errors in the observation data, the GRACE/GLDAS data or in the data processing method used. This will be added in the revised version of the paper.

2) pp. 6057, l.17-20. The authors state that "land surface deformation could only be caused by abstractions of groundwater, hydrocarbons or coal. Thus GPS data product of relative LSC is used to analyze for land subsidence due to loss of water storage in the region". Is the effect of abstractions of coal and hydrocarbons negligible in the GPS data?

Response: While the effect of hydrocarbon or coal extraction could be significant in Shangdong Province, that of groundwater abstraction can be significant in the north-

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ern region of the study area. However, the focus of the paper is subsidence due to groundwater abstraction; which will be made clear in refined study area in the revised version of the paper.

3) pp. 6058, l.1-12. The authors stated that it must be treated with caution to the averaged land subsidence value derived from the only one GPS station data. In spite of this, they used this value for a critical discussion in the next section. I think it is quite over discussion.

Response: As the study area will be refined to a limited region, the discussions that follow in the revised version will be more appropriate.

4) pp. 6058, l.13-23. This part should be moved to the discussion section.

Response: The stated part will be moved to “Discussion” section.

(4) Discussion 1) pp. 6060, l. 11. “SWS”: Is that obtained by field observation or GLDAS-derived value?

Response: GLDAS-derived SWS should be adjusted for irrigation in cultivated regions, thus the statement here is for the adjusted GLDAS data.

2) pp. 6060, l. 23-28. The authors have already stated the same thing in pp. 6059.

Response: This repeated statement will be removed.

3. Technical corrections (1) pp. 6051, l.16: “the bottom right” → “the bottom left”.

Response: Correction will be implemented.

(2) Figure 1, caption of the bottom left figure: What is the meaning of “GRACE averaged monthly total water storage anomaly”? Is that the linear decrease trend throughout the observation period? It is very difficult to read the spatial variation from the figure because of the color problem. Full color should be used instead of black and white.

Response: Yes, it is the linear trend for the observation period. This statement will be

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added and plotted in full color for clarity.

(3) Table1: There are no columns corresponding to the description “the column highlighted grey” in the footnote of the table.

Response: This will be removed. It existed in the original version of the paper.

(4) pp. 6054, l.21: p and alpha are not defined.

Response: These are very common statistical symbols and are therefore not normally defined.

(5) Figure 2, Figure 3, Figure 7: Please use a common horizontal scale in each figure.

Response: Recommendation will be adopted

(6) pp. 6059, l.20: “number pf” → “number of”.

Response: Correction will be adopted.

(7) pp. 6059, l.18 and l.19: “km3” → “km3/yr”.

Response: Correction will be adopted.

(8) pp. 6059, l.27-29: “mm”! “mm/yr”, “km3” → “km3/yr”.

Response: Correction will be adopted.

(9) pp. 6060, l.18: (Probably) Eq. (2) → Eq. (3)?

Response: Correction will be adopted.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 6043, 2015.