

Interactive comment on “Monitoring Groundwater Storage Depletion Using Gravity Recovery and Climate Experiment (GRACE) Data in the Semi-Arid Catchments” by Nizar Abou Zaki et al.

Anonymous Referee #3

Received and published: 30 January 2019

I have reviewed the manuscript by Abou Zaki et al. which evaluates groundwater storage changes in the Bakhtegan catchment in south-western Iran. I find their results interesting; however, I don't feel that the manuscript is appropriate for publication in its current form. I have several primary concerns. First of all, the novelty of this work is not clear. As other reviewers have pointed out, there are a myriad of studies using GRACE to evaluate groundwater storage changes and compare with well data. In my opinion the authors do not do a sufficient job putting this work in the context of that body of research and highlighting what is novel about this study. Second, the methods are unclear and there are inconsistencies in the descriptions and terminology that make it very difficult to determine exactly what is being calculated and compared. Finally,

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there is no uncertainty analysis and the authors have not demonstrated that the trends they report are statistically significant. I have outlined more detailed comments below. However, given these significant deficiencies I recommend rejecting the manuscript.

Title: This is really a study of one basin not a study of semi-arid basins in general and the title should be revised to reflect this.

Introduction: There is some reference to previous GRACE work in the introduction, but additional discussion is needed to better explain how this specific location adds information to the comparisons that have already been done.

Methods: There are many points in the methodology that are inaccurate or are not clear. I have listed some examples below:

- In equation 3 how is porosity accounted for?

- There is no detail on how the well data was combined to get the groundwater storage estimates and what hydrogeologic properties were used.

-It is also unclear how the ET rates were calculated. What time period were the 'average ET rates' averaged over? And how was land use data used to adjust these values for agricultural land and forests? A figure showing the ET with some uncertainty bounds on ET would be helpful.

-In equation 5 why do the GW and total Water components have the MI subscript but not the other two?

-Equation 5 talks about total storage but to be consistent with GRACE these should be changes in storage correct? I don't see anywhere here where the total groundwater storage is calculated.

-More details on the surface storage calculations as well as soil moisture would also be helpful. For example, where are the stream gauges located? How exactly were storage changes calculated taking into account the Water Atlas report?

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-It looks like Equation 2 assumes that all of the net precipitation goes to recharging groundwater. What about runoff? How is this accounted for?

-Page 8 Line 1: Please specify exactly what 'the GRACE data' you are referring to here.

-Page 8 Line3: Contrary to the text, It looks like KB is actually the relationship between groundwater volume changes and precipitation not GRACE? Is this correct?

-Page 8 Line 11: How was monthly net precipitation calculated from GRACE? This does not make sense to me.

Terminology: The terms groundwater storage, and groundwater levels are used interchangeably in the manuscript. I think the authors should be more careful in the definition and use of these terms. This especially needs clarification because the storage changes are expressed in length units which can give the impression that they reflect groundwater depth changes, when they do not.

Additional Comments:

-Page 10 Lines 13-16: The authors note it is difficult to calculate volumetric losses from the groundwater measurements. However, this is one of the key goals of the study and is necessary for a meaningful comparison to GRACE. As noted above, I would like additional details on how groundwater storage was calculated and what the uncertainties in this calculation are.

-Page 11 Line 12: What constitutes a 'good fit'? Can you be more precise in how this was quantified?

-Page 14 Lines 15-17: This sentence doesn't make sense to me, please revise.

-Page 14 paragraph starting on lines 18: This discussion is a bit hard to follow. I think it would fit better in the methods section where the details on how groundwater storage changes are calculated should be included. It's not clear from this discussion how the

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confined and unconfined units were treated in the delta S calculation.

-Discussion Page 15 Lines 3-5: it is unclear how the combination of these datasets provides unique information on water use. Mainly what has been presented here is a comparison between methods and it's not clear that the difference between these two reflects anything other than uncertainty in all of the water balance components. Please clarify what you mean by this.

-Page 16 lines 14-15: I disagree that 7.6 mm is a 'severe water loss' even for a semi-arid area. Can you provide some justification for this classification?

-Page 16 line 18: The analysis presented here does not do anything to prove that 'confined fossil aquifers' were reached so this should not be included in the discussion.

-The discussion section is really broad and connects out to too many things not covered in the results. I recommend refocusing the discussion around the findings of the paper as well as the uncertainties and assumptions that were made and the potential implications of these limitations.

-The authors talk about water level drops of 10 -30 m over the 10 year study period which but then have annual trends of 7.6mm. In order to rectify these measurements we need a better understanding of the physical properties of the aquifer. Without knowing this it is difficult to understand how reasonable the trends in volume are.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-471>, 2018.

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