

The authors highly appreciate the reviewer # 1 for his/her constructive comments and suggestions that helped us to significantly improve the quality of the manuscript.

Hashemi et al. present a modeling study of potential climate change impacts on groundwater resources in an arid basin in Southern Iran. While, the methodology they employ is not new, the authors present results for an area that has not been previously studied. The results are relevant for regional planning purposes because the authors directly consider groundwater management practices in the basin.

While I feel that this study would be of interest to the readers of HESS, in my opinion the manuscript need significant work before it will be ready for publication. The writing needs improvement and the presentation of the methods is incomplete. As a result, the paper can be hard to follow. Also, I think that the results and discussion need to be significantly expanded to thoroughly evaluate the findings. It's not clear to me from the results that there are significant climate impacts and I'm not convinced that the delta method is appropriate for making statements about future flood frequency. However, the authors still conclude that climate impacts will be 'serious' and that the 'number of floods might increase'. In my opinion, the conclusions should be reconsidered and the results section should be revised to more clearly support the conclusions.

Specific Comments

1. In find the title to be misleading because it implies that this paper will present a new modeling approach. However, the focus of this work is really climate change impacts to a specific basin and not validating a new method that can be applied to arid areas in general.

According to the authors' knowledge, there are still quite few studies on climate change impacts on groundwater (GW) resources in which a sequential modeling, coupling climate change model scenarios (CGCMs), surface water model (QBox), and GW recharge model (MODFLOW), has been used. In almost all previous studies, recharge is estimated using either simple regression, as a function of precipitation, or through soil models. However, the one presented here, GW model is used to estimate GW recharge. Therefore, the methodology employed in this study is new and can be considered as an improved method. It is noted that similar methods, but nit the same, have been utilized in previous studies, e.g., Okkonen and Klove (2011), Dams et al. (2012), and Barron et al. (2012).

Moreover, this study is a method development that has been validated for a case study in an arid environment. Further, the adaptation scenarios through the calibrated GW recharge model can be considered as strength in the methodology developed in this study. In order to clarify better, some advantages/disadvantages between the present study and above-mentioned studies are given below.

Okkonen and Klove (2011) used CoupModel, which is a processed based soil model, for assessing water infiltration/water flow through the soil and, consequently, GW recharge. In other words, their approach involved estimation of recharge with a soil model, which allows water infiltration in frozen soil. Then the result was imported to MODFLOW to simulate the GW flow and storage changes. As an outcome, they mainly used GW model to simulate GW flow. Another similar study to the presented manuscript, was carried out by Dams et al. (2012). They also used a physically based hydrological model, WetSpa, coupled with a GW model. In their research, surface water availability and downward water flux through soil

(recharge) were simulated by WetSpa. Then, GW exchange between the GW system and the river and discharge from streams were simulated by MODFLOW. Although they used a one-way coupled surface and sub-surface hydrological model, GW recharge that was estimated by a physically based hydrological model, was not updated by the GW head and flux information from the GW model. This is because WetSpa simulates the GW recharge based on the soil moisture content independent from GW level.

The presented manuscript uses a similar methodology as used by Barron et al. (2012). They evaluated the climate change effects and flow regimes by applying the climate projections scenarios to rainfall-runoff and GW models in south-western Australia. However, they concluded that the methods used in their study are "suitable" for regional-scale (62,000 km²) estimates but to assess local impacts on water-dependent ecosystems and water yield, finer-scale modeling and analysis are required.

Our study shows the capability of one-way coupled surface water and GW recharge models to assess the effects of climate change on small-scale aquifer (60 km²) by applying climate change projections' scenarios to the conceptual hydrological models. In other words, this methodology is developed for linking climate change models' output, surface water model, and GW recharge model to investigate the future impacts of climate change on both surface water and dependent GW system through a sequential modeling approach. Yet the GW recharge model works based upon the GW hydraulic head, the estimated recharge is directly associated with reservoirs' behavior, which reflects both climate change impact and current land and water management in a particular region. Although, the reviewer mentioned that the methodology presented in this study is not new, but he/she did not refer to any similar methodology in other studies. Based on our comprehensive literature review, the type and sequence of numerical models that have been used in this study is new and appropriate for evaluation of GW resources impacted by climate change at the local-scale arid environment. Hence, we suppose it would be good to keep the title as it is.

[Some parts of the above explanation will be added to the revised version of the manuscript (in the discussion and conclusion sections)].

2. The distinction between the effects of management changes and climate change is not clear. I think the authors need to do a better job of isolating these two perturbations to their system and explicitly comparing the impacts of climate relative to changes in pumping and recharge areas.

The authors thank the reviewer for pointing that out. We will try to better separate the effects of management and climate change throughout the revised manuscript.

3. In my opinion, section 2 needs to be rewritten. It is not clear to me whether the point is to summarize the primary groundwater impacts of climate change in arid environments (as the third paragraph and the title would suggest) or to summarize available modeling tools. If the point is to summarizing the findings of others, then this section should be organized around the types of impacts observed and the places people have seen them. Also, in addition to the studies that are discussed in detail, the authors should provide a summary of impacts and the physical mechanisms behind them. If the point is to review potential tools, then the discussion should contrast different modeling approaches and discuss the strengths and weaknesses of each approach in this type of environment. I think

both discussions could add to the manuscript but it would be a good idea to split this section in two so that it is clear.

We agree that this section is ambiguous and the aim of this section needs to be better explained. Therefore, we tried to rewrite the entire section focusing more on potential tools and different modeling approaches and discuss the strengths and weaknesses of each approach, as this was the primary intention for this section. Accordingly, the title was modified to "Review of different modeling approaches in assessing climate change impacts on groundwater resources". The revised section (will replace the former one in the revised manuscript) is as follows:

"The choice of modeling approach to assess the climate impacts on GW is dependent on the system complexity and modeler preferences. For assessing climate change impact on recharge, the easiest way may be to use a simple regression model, which is used to predict recharge rate where annual recharge is assumed to vary linearly with annual rainfall (Barron et al., 2010). For this, GCMs simulated precipitation rates are used to predict inflow to a calibrated GW model (e.g. Hanson and Dettinger, 2005; Surinaidu et al., 2013). In a similar way, Surinaidu et al. (2013) employed a GW modeling approach to estimate the aquifer parameters and GW flow in a large river basin in humid southwestern India. In their study, the net recharge from all hydrological components and GW discharge in the studied catchment was estimated based on empirical equations derived between rainfall data and GWL. Accordingly, the average GW recharge coefficient was estimated to be 11% of annual rainfall. They also applied linear regression between historical rainfall and river discharge data to estimate the potential surface water available in the future, which then was added to the annual estimated recharge for the future climate scenarios.

According to this method, the predicted recharge is mainly based on direct precipitation, which may not be accurate, particularly, in arid regions where the direct rainfall recharge is expected to be less than 1.0% of the corresponding rainfall amount. In addition, GW recharge has a random behavior depending on the sporadic, irregular, and complex features of storm rainfall occurrence, land cover and land use variability, soil moisture, and geological composition (Şen, 2008). This leads to nonlinear relationships between precipitation and recharge. Further, Ng et al. (2010) defined that for most climate alternatives, predicted changes in average recharge are larger than the corresponding changes in average precipitation primarily due to temporal distribution of precipitation change (over seasons and rain events) and a complex mix of climate and land-surface factors. Another major limitation of this approach is the lack of subsurface conceptualization of the system (Surinaidu et al., 2013). However, this method can be assumed to be a viable approach for the subsurface scarce-data region of the world. In contrast, it can be concluded that this is an inappropriate approach for assessing GW resources impacted by climate change in arid environments.

Some researchers have used the linear storage method (Robock et al., 1995; Barron et al., 2010) to predict recharge based on a series of descending storages to estimate the water storage in soil layers. It is assumed that the direct areal recharge to the aquifer occurs from a combination of weather data, stream, soil characteristics, vegetation, and land cover data. Some wide spread models using this method are HELP and SWAT (e.g., Jyrkama and Sykes, 2007; Toews and Allen, 2009; Abbaspour et al., 2009). Eckhardt and Ulbrich (2003) carried out a regional climate change impact study on plant transpiration induced by changes in

atmospheric CO₂ concentration and its consequences on GW recharge and stream flow for a central European low mountain range by SWAT model. They concluded that the resulting effects on mean annual GW recharge and stream flow are small due to the balance between the increase in plant interception and ETP due to the temperature rise and the reduction of stomatal conductance resulting in decrease in transpiration.

The linear storage models define the role of soil moisture for ETP by distinguishing radiation-limited regimes and soil moisture-limited regimes (Seneviratne et al., 2010) in which a near-surface layer of soil is modeled as a storage that can be filled by infiltrating precipitation and exposed by evaporation and runoff if filled up (Handerson-Sellers et al., 1993). The storage models represent an important attempt to capture soil moisture limitation on ETP in climate models. Nevertheless, studies have revealed that the model parameterizations are inadequate as plants' transpiration beside soil moisture is not included in the calculation leading to over-estimation of ETP (e.g., Handerson-Sellers et al., 1993; Robock et al., 1995). In addition, the bucket models do not consider interception storage and geographical variations in soil and vegetation parameters that may result in over/under estimation of runoff quantity (Seneviratne et al., 2010).

There are also integrated physically based hydrological models that consider water exchange between surface water, unsaturated, and saturated zones within the model frame, e.g., MIKE-SHE, MOHISE, HydroGeoSphere, CATHY, and ParFlow (e.g. Brouyère et al., 2004; Goderniaux et al., 2011; Goderniaux et al., 2009; Ferguson and Maxwell, 2010; Stoll et al., 2011; Sulis et al., 2011). van Roosmalen et al. (2009) used an integrated process based surface-groundwater model to study the intricate, nonlinear relationships between the land surface, unsaturated, and saturated zones under changing conditions through the large number of parameters by MIKE-SHE. Their study showed that climate change has the most substantial effect on the hydrology of a large-scale agricultural catchment in Denmark.

Although, the integration of surface and subsurface flow in the same model presents a better conceptualization and accuracy in simulation of water interaction between surface water and GW. Yet, it should be mentioned that, proper calibration of integrated physically based hydrological model performance depends highly on the quantity of collected data, which may result in less accuracy in data-poor regions. Hence, this method may not be applicable in many arid developing countries, as the monitoring system is often not sufficiently developed to properly characterize the system, e.g., the Middle East.

Kløve et al. (2013) stated that the quantification of climate change impact on GW reservoirs and recharge rates can be explored by GW models with future climate scenarios acquired from GCMs (e.g. Hanson and Dettinger, 2005; Dams et al., 2011; Leterme et al., 2012). Okkonen and Kløve (2011) carried out a sequential simulation of three models to estimate the temporal and spatial variation in surface-groundwater interaction. For this, they used the Watershed Simulation and Forecasting System (WSFS) model to estimate areal precipitation and temperature and to simulate the surface water levels in lakes and rivers in a cold climate watershed in Finland. The output of the WSFS model, precipitation and temperature, was used as input to the CoupModel to simulate aquifer GW recharge rates. The simulated surface water flow and recharge rate were finally imported to MODFLOW to simulate the GW flow, surface-groundwater interaction, and to predict the GWLs change in view of future climate change scenarios. Although, they used CoupModel to estimate recharge rate, the estimated value is based on precipitation and temperature and not surface water availability. Barron et al. (2012) employed extensive couple modeling to project the future GW

recharge and GWL at regional scale in Australia. In their study the coupled surface-groundwater model was first calibrated for the period 1975-2007 and the climate sequences were then used as input to the calibrated models for the projection of impacts on runoff and GW balance. They concluded that the methods used are suitable for regional-scale estimates but to assess local impacts on water dependent ecosystems and water yields, finer scale modeling and analysis would be required.

In this approach, couple modeling is an appropriate method taking into account the generated runoff produced by climate scenarios. This approach can be assumed to be the most appropriate for predicting recharge in arid areas where surface runoff is the major water supplier. However, the appropriate choice of GW model to adequately estimate the recharge rate is fundamental due to insufficiently observed subsurface data in most parts of the arid world.

To the authors' knowledge there are very few studies on climate change impacts on GW resources in which the effects of projected rainfall on surface runoff and its consequences on GW recharge are considered, particularly for arid areas and at a local-scale (>100 km²). In view of this, we used an extended couple modeling approach for studying climate change impacts on GW resources and adaptation scenarios in an arid region of Iran. We applied a methodology that is able to estimate GWL fluctuation, based on already calibrated rainfall-runoff and recharge models. For this, three GCMs scenarios, A1B, A2, and B1, were used as input to a coupled one-way surface-groundwater model for the periods 2010-2030 and 2030-2050. The novelty of this research is to apply several artificial recharge and pumping scenarios employing the calibrated GW model for recharge rate in order to identify the possible GW management alternatives. In the adaptation scenarios through GW modeling, variable pumping for irrigated farmlands and management scenarios through floodwater harvesting systems were undertaken."

4. This study relies on a single GCM the Canadian Global Coupled Model. While I understand the need to keep the number of simulations manageable, I think the authors need to justify the choice of a single GCM more thoroughly. For example, how do the deltas derived from this model compare to the suite of CMIP3 projections? At a minimum, the authors need to expand on the discussion to explain that this is just one GCM and that there can be significant variability between GCMS.

This is a much valid comment, especially considering the amount of freely available GCM data nowadays. The reason we chose to use only one GCM is related to the scope of this study being to develop a new methodology that is capable of evaluating climate change impact on GW resources in arid areas. We did not want to divert the focus of the study from the methodology and so we chose to present a simple case study. Therefore, only one GCM output was used to test the developed methodology. As the reviewer suggested, we will point out the choice of a single GCM and methodology development more thoroughly in the revised version of the manuscript.

5. There is no discussion of uncertainty in this paper. The authors present these results as their prediction for the future, but they do not address the fact that this is just one GCM. Ideally, the authors should compare multiple models. However, even if this is not feasible a more rigorous discussion should be added to address this point.

The primary aim for the present study was to develop a new methodology for assessing the climate change impact on GW resources. Hence, a case study was used to test the methodology. On the other hand, due to the interesting properties of the studied region, i.e., artificial recharge system and flood prone region, a more thorough explanation and discussion regarding the climate change impact on GW resources was given in the manuscript. Nevertheless, the authors agree with the reviewer regarding the use of only one GCM outputs and missing discussion in conjunction with uncertainty. Therefore, the below section will be added to the revised version in the discussion section.

“Uncertainty analysis

Dams et al. (2012) argue that the uncertainty on climate change projections is rather high due to the uncertainties in future greenhouse gas emission prediction. The uncertainty is even larger when the climate change projection scenarios are integrated into hydrological models as the accumulation increases of various sources of uncertainty in coupling approaches. Therefore, there are many sources of uncertainty in climate change impact studies (Kay et al., 2009). However, in the present study, we only address the four crucial sources of uncertainty including 1) Emission scenario uncertainty for which there is no universally accepted methods of reducing the uncertainty as the future world economy is still uncertain (Wilby and Harris, 2006). 2) Uncertainty associated with climate model, which can be reduced by comparing the output of different climate model projections as they give the highest probability outcome. Thus, the multi-model average projection results are applied for each climate variable in the hydrological models (e.g. Serrat-Capdevila et al., 2007; Stoll et al., 2011; Dams et al., 2012; Barron et al., 2012). This methodology is an appropriate approach for reducing the climate model uncertainty in our study as we only used a single GCM output. Consequently, our study’s contribution to the field of climate change impact studies is to develop a methodology for assessing the GW reserve impacted by projected climate change scenarios rather than reducing climate change projection uncertainty. Nevertheless, reducing the climate model uncertainty is a new research area for further study by applying the same methodology. 3) Uncertainty in adaptation of the projected scenarios to the studied area, i.e., the delta-change. Studies (e.g. Hay et al., 2000; Kay et al., 2009) have shown that the delta-change approach is likely to under-estimate the range of uncertainty simulated from GCMs as this method is based on the changes in mean climate variables. 4) Hydrologic model structure and parameters uncertainty. Uncertainty and model sensitivity analysis regarding the GW recharge model were documented in Hashemi et al. (2012; 2013). Regarding the rainfall-runoff model, there is a degree of uncertainty mainly associated with input parameters, as they are collected from an adjacent basin, however, with good similarity to the studied basin. Nonetheless, agreement between the simulated and observed discharge values for the river, yielded convincing model calibration and validation results (Fig. 6).”

6. The use of the Baba-Arab station as a surrogate for the study area needs to be better justified. For example, is the precipitation equal for these basins?

Thank you for your suggestion. Accordingly, the text on page 11804, line 21-22 was modified as follows.

“Because of the newly established meteorological stations in the GBP (since 1995), the

longest rainfall record exists in the area (since 1972) at the Baba-Arab meteorological station, 15.7 km southwest of the GBP. Thus, this station was used for climate change projections. It is noted that the annual rainfall average difference between the two stations (GBP and Baba-Arab) is less than 5 mm taking into account the various measured-data period for both stations.”

7. Section 3.4 is hard to follow. I think it would be helpful if to start off with an outline of what data is needed and why before getting into the details. Also, I would consider moving the data discussion after the description of the modeling approach.

We agree with the reviewer for moving the observed data section after the description of the modeling approach. The below text will also be added to the manuscript (Methods and materials section).

“The analysis of climate change effects on surface and GW resources were based on the: 1) delta change approach, 2) hydrological modelling, and 3) GW modelling. Accordingly, the application of different modelling and climate change projections were based on: 1) climate scenarios, 2) atmospheric data, i.e., rainfall, temperature, and ETP, and 3) hydrological data, i.e., flood records and GW hydraulic heads.”

8. The spatial element of this work is really unclear to me. How many observation stations are there in the basin for precipitation and temperature? Is it assumed that the climate variables are constant over the domain? If not how do they vary spatially?

Thank you for pointing this out. The following text will be added to the revised version of the manuscript.

“As the studied area is relatively small, it is assumed that the climate variables are constant over the entire area for the studied period by using only one meteorological station’s data.”

9. Section 4.2 also needs a better introduction. Rather than going back to the motivations for modeling and the potential climate change impacts, I think the authors should outline the approach that is taken here and the general goals for the modeling exercise. This would be a good point to reference Figure 3.

As suggested by the reviewer, the first paragraph (line 2-7) was removed from the section. The second paragraph was modified and will be included in the beginning of the section in the revised version.

“Both rainfall-runoff and GW models were calibrated and validated using observed data. Then the projected climate variables for scenarios A1B, A2, and B1 were used as input to the rainfall-runoff model. In the next step the projected runoff was used to simulate GW recharge in both near and far future. It is noted that as the GWL in arid and semiarid areas is often rather deep and the variation in adjacent surface water level is not affected by GW discharge, there is no need for two-way coupling and, thus, one-way coupling between surface runoff and GW reservoir would suffice (Ataie-Ashtiani et al., 1999). Accordingly, a sequential surface-groundwater modeling was undertaken to project the surface runoff, GWL, and GW recharge for the future studied periods. Hence, the analysis of climate change impacts on the

GW reservoir in the studied region was based upon the projected runoff and GW recharge as the results of rainfall-runoff (Qbox) and GW (MODFLOW) modeling. In the final step, four different adaptation and management scenarios for GW artificial recharge and abstraction rates were applied to the calibrated GW model in order to assess the GW safe yield for the next 40 years (Fig. 3)."

10. The models are not well described. I think there needs to be additional documentation on the equations used by the models, the assumptions that are made and the way information is passed back and forth. Also, the description of the domain is lacking any mention of total extent, spatial resolution and boundary conditions.

Full details concerning the development of the conceptual GW model, model design (e.g., discretization, definition of boundary conditions, and model input parameters), calibration and verification, and simulation results (GW flow and estimated hydraulic parameters) are documented elsewhere (Hashemi et al. 2012; Hashemi et al. 2013). The purpose of this section is to provide an overview of the critical components of the model so as to provide a framework for using the model for climate-change impacts on GW resources. Anyway, more details in conjunction with GW modelling can be given based upon the respective reviewer's request.

In regard to the rainfall-runoff model, the below text will be added to the revised version.

"Qbox is a general tool for hydrological modeling of water movement through a river basin. In principal, the model is constituted by different linear storages placed vertically above each other (Fig. X). The model is built on the continuity equation (Eqs. 1 to 4) for each box and a number of auxiliary relationships. In the model, all soil-water located between the soil surface and the GW table is treated as if it was in a storage (or reservoir). The percolating water from each storage is recharging the upper storage or reservoir. The level in the storage rises due to infiltrating precipitation and sinks due to evaporation. In other words, water leaves the upper storage as evaporation, which is directly coupled to the atmosphere, as deep percolation, and as runoff (outflow) to a river system. Not until the storage is full, which corresponds to water content reaching field capacity, the water from the storage contributes to runoff. Water surplus percolates to deep GW. Accordingly, the continuity equation for a soil box can be described as:

$$\frac{dh_{soil}}{dt} = p - e - f; \quad e, f = fun(h_{soil}) \quad (1)$$

where h_{soil} = amount of water in soil-water storage, p = precipitation, e = evaporation, and f = GW generation or percolation, which is modelled as rate of filling of simple or double reservoir. The rate of recharge, f , is limited to f_{max} , i.e. $f = \max(f_x, f_{max})$. This means that although much rainwater enters the soil, the soil-water does not contribute to GW recharge at a faster rate than f_{max} . The recharge formula is:

$$f_x = p \left(\frac{h_s}{h_{fc}} \right) \quad \text{when } h_s > h_{stop} \quad (2)$$

where h_{fc} represents field capacity, i.e. a representative soil water storage when the entire zone is at field capacity. There is also a parameter h_{stop} , which has the function of completely stopping GW recharge when the soil water storage drops below h_{stop} . The rate of evaporation depends on potential evaporation (ETP) and as a function of soil-water content, $e = ETP f(h_{soil})$.

$$e = pe \left(\frac{h_s}{h_e}\right)^\alpha \text{ but } e = pe \text{ when } h_s > h_e \quad (3)$$

where α is an exponent, less than 1, and h_e is the soil moisture above which the evaporation takes place at the potential rate. The rate of percolation depends on rain intensity and soil-water content, $f = f(p, h_{soil})$. The part of the rainfall, which has a higher intensity than the maximum infiltration capacity of the soil, i.e. after some time the difference between the rain intensity and the hydraulic conductivity at saturation, contributes to Hortonian surface runoff. Hence, outflow at the bottom of the soil-water box forms inflow to the runoff box. This runoff contribution must first fill up depression storage before real surface runoff occurs. Calculation of surface runoff can be made using a non-linear reservoir or a reservoir with a small time constant. An addition to the rainfall-runoff model is to use a deep GW box. Though, water percolates from the runoff storage into the deep GW storage. The continuity equation for the runoff storage and the deep GW storage can be described as:

$$\frac{dh}{dt} = f - q - i_p; \quad q = \sum q = fun(h_{soil}) \quad (4)$$

where q is surface runoff and i_p is deep percolation.

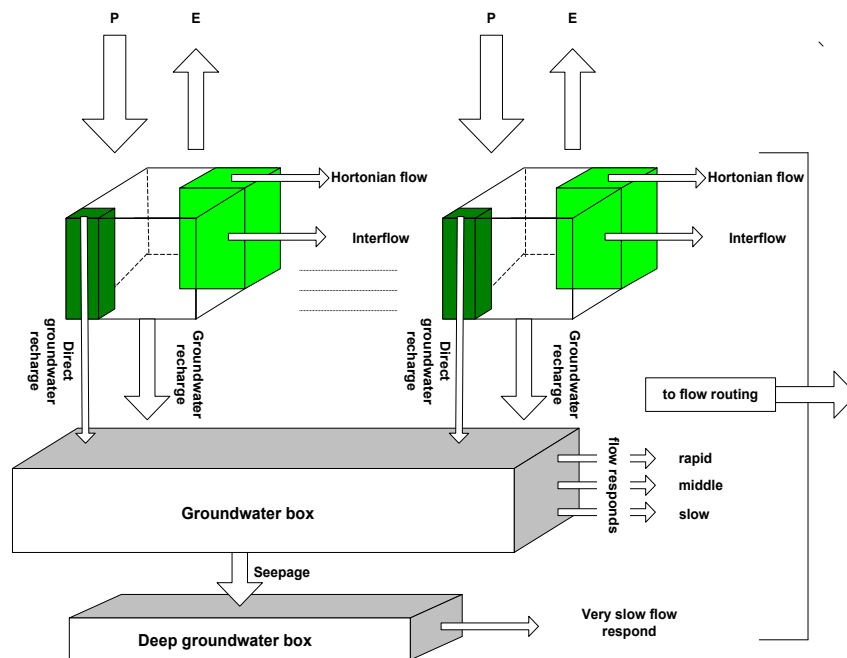


Figure X. Structure of the Qbox rainfall-runoff model (source: Iritz, 2014).

11. I am concerned about the statements made about future flood probability. The delta approach does not capture changes in weather patterns and drivers of extreme events. Of course, if you are scaling precipitation there is the potential for more rain events that cross a 'flood' threshold, but this is not the same as predicting an increased likelihood of extreme events. The authors conclude that, "more intense rainfall will occur in both future periods" (11815, 6). With this methodology I think you can conclude that it will be wetter in the future. However, you cannot say anything about how the precipitation will be distributed within storm events.

The authors thank the reviewer for his/her thoughtful comment. We agree the word "intense" is an improper term for climate change projection using delta-change approach. Therefore, we have rewritten the sentence according to the reviewer's suggestion.

"It can be concluded that based upon scenario B1, the area is wetter in both future periods resulting in more flood events."

12. In my opinion, the results and discussion sections are weak and need to be significantly expanded. Essentially the results for the entire paper are presented in a single figure (Figure 7). There is no discussion of the differences between the climate change scenarios or quantitative comparison of climate change impacts and management scenarios. Also, Figure 7 only shows the first future period, there is no presentation of the second future period. The tables are helpful, but I think there could be additional graphical analysis of the differences between scenarios.

As it was stated on page 11815, line 24-25, that the same rate of GW drawdown was seen in both near and far future. So, only the results of the near future projection are depicted in Fig. 7 (due to the similarity of GWL drawdown in both periods).

We also think that due to the large number of figures in the manuscript, it is better to skip this figure.

13. Throughout the paper it is noted that all of the groundwater recharge results from flooding. However, it is not clear how floods are simulated in the model. Does the inundated area and total recharge change with precipitation rate?

As mentioned in response to the comment number 10, the detailed simulation results of the GW modelling are presented in other publications. According to those publications, in the case of flood event, all the artificial recharge areas plus river channel are considered to be fully flooded. However, it was calculated that (Hashemi et al., 2013) in extreme rainfall events (three events during a 14-year period) more water was recharged through ephemeral river channel relative to artificial recharge areas. In the case of normal rainfall events, artificial recharge areas showed a significant contribution (80%) in total recharge (page 11810, line 23-25). In the present study, the average recharge rate value for all recharge zones (3 recharge zones) was assigned to the model for future projections (page 11811, line 1-2).

Technical Corrections

Abstract:

- 11798, 7: Define HBV

HBV was replaced by "a conceptual rainfall-runoff model"

- 11798, 13: What do you mean by 'might increase'? Do you mean 'are projected to increase'?

Thank you for the correction. The text was modified as follows.

"As a results of rainfall-runoff modeling, the number of floods are projected to increase under the B1 scenario."

- 11798, 3: Delete 'a' before 'proper'

Deleted.

- 11798, 15: This is confusing because in the last sentence you said that the number of floods will increase but here you are saying that there is no recharge impact?

Based on previous studies, it is apparent that there is no linear relationship between flood events and recharge. This issue was also addressed on page 11801, line 18-24.

1. Introduction:

- 11799, 9: Insert 'an' before 'increase'

Inserted.

- 11799, 13: 'no change or noticeable decrease and increase' is confusing, reword this sentence.

The sentence was modified as follows:

"Majority of climate models predict noticeable decrease in precipitation and increase in temperature in the arid Middle East."

- 11799, 16: Delete 'The' before 'climate'

Deleted.

- 11799, 18: What 'economic situation' are you referring to? This is the first time economics are mentioned.

The sentence was modified as follows:

"Hence, adaptation is needed to cope with changing water resources in view of the economic situation of the region. This is because the regions' countries are mostly considered as developing countries where building large hydraulic infrastructure might not be consistent with their economic situations."

- 11800, 2-5: This is confusing. I do not understand how you 'apply projected recharge periods' to a hydrologic model. Do you mean you are only simulating these periods with their model? Also please define what a recharge period is before you discuss it.

Thank you for the comment; we have rewritten the sentence as follows:

"A common approach for subsurface hydrology prediction is to use the results acquired from General Circulation Models (GCMs). This involves downscaling of the projections from a course-grid scale of a GCM to a finer scale, creating time series of future possible precipitation and other climate variables to apply to hydrological models in order

to simulate surface runoff and possible GW recharge.”

- 11800, 7: Define ‘ETP’ before you use it.

It was already defined on page 11799, line 20.

- 11800, 7-9: I do not understand what this sentence is saying. In the previous sentence you say that groundwater impacts from climate should be indirect. However here you say that because of this you need integrated models?

What ‘above processes’ are you referring to? Do you mean the hydrologic processes like runoff and ET or are you talking about climate processes?

Thank you for the comment. So, as the entire paragraph sounds confusing we decided to remove this paragraph from the manuscript.

- 11800, 12: 1% of what?

The sentence was modified as follows:

“In most arid and semiarid environments, direct recharge from rainfall is considered to be less than 1% of the total rainfall.”

- 11800, 20: Should be ‘flash floods’ not ‘flash flood’

Corrected.

- 11800, 27: What complex natural processes are you referring to? I found this discussion to be vague and I think that it would be improved by adding a paragraph to explicitly walk through the physical interactions and processes that you are alluding to.

The entire paragraph was modified according to below:

“Kløve et al. (2013) noted that for climate change impact studies on GW systems an integrated multidisciplinary monitoring approach is necessary in order to better define the interaction between all hydrology components and land use management. Though, the acquisition of atmospheric, surface water and GW data, together with land use changes and water extraction are fundamental. Then, modeling is needed to link these complex processes.”

- 11800, 28: The last sentence of this paragraph does not make sense. What do you mean by ‘acquisition of land use changes’?

The entire paragraph was modified including the last sentence (please refer to the previous comment). Furthermore, land use changes would affect the hydrologic processes of a particular region i.e. converting natural forest to irrigated farmland that leads to less infiltration and water losses through crop transpiration, for example.

2. Review of climate change impacts on groundwater resources

- 11801, 7: Define ‘GWL’ before you use it.

It was already defined on page 11799, line 29.

- 11801, 23-24: Explain why this is the case.

Primarily, due to changes in temporal distribution of precipitation (over seasons and rain events) and a complex mix of climate and land-surface factors.

- 11801, 25-26: Where did this study take place?

In a large river basin in southwestern India (will be added to the revised version).

- 11802, 21: Cathy and ParFlow are two other examples of integrated models that should be included in this list.

Thanks for the suggestion. They will be included in the text together with relevant references.

- 11802, 26: 'the most substantial effect' compared to what?
Compared to recharge.

- 11803, 1: I doubt that Klove et al. (2013) were the first to connect groundwater models with downscaled GCMs. The way this sentence is written it sounds like you are crediting them with pioneering this approach.

This was not the intention for this sentence as we have cited to the studies carried out in 2005; 2011; and 2012 immediately after this sentence. We have just quoted a statement from a review article by Klove et al (2103).

- 11803, 4: What do you mean by couple modeling and why is it appropriate?
Couple modeling between surface and GW resources. The appropriateness of the coupling approach was described in the modified text in section 2 of the manuscript (please refer to the response to the Specific Comments No. 3).

- 11804, 1: Define 'local-scale'
Less than 100 km². Also, the studied aquifer occupies 25% of a major aquifer named Shib-Kuh aquifer.

- 11804, 5: Explain what you mean by 'coupled one-way'. What variables are being passed?

The sentence was modified as follows:

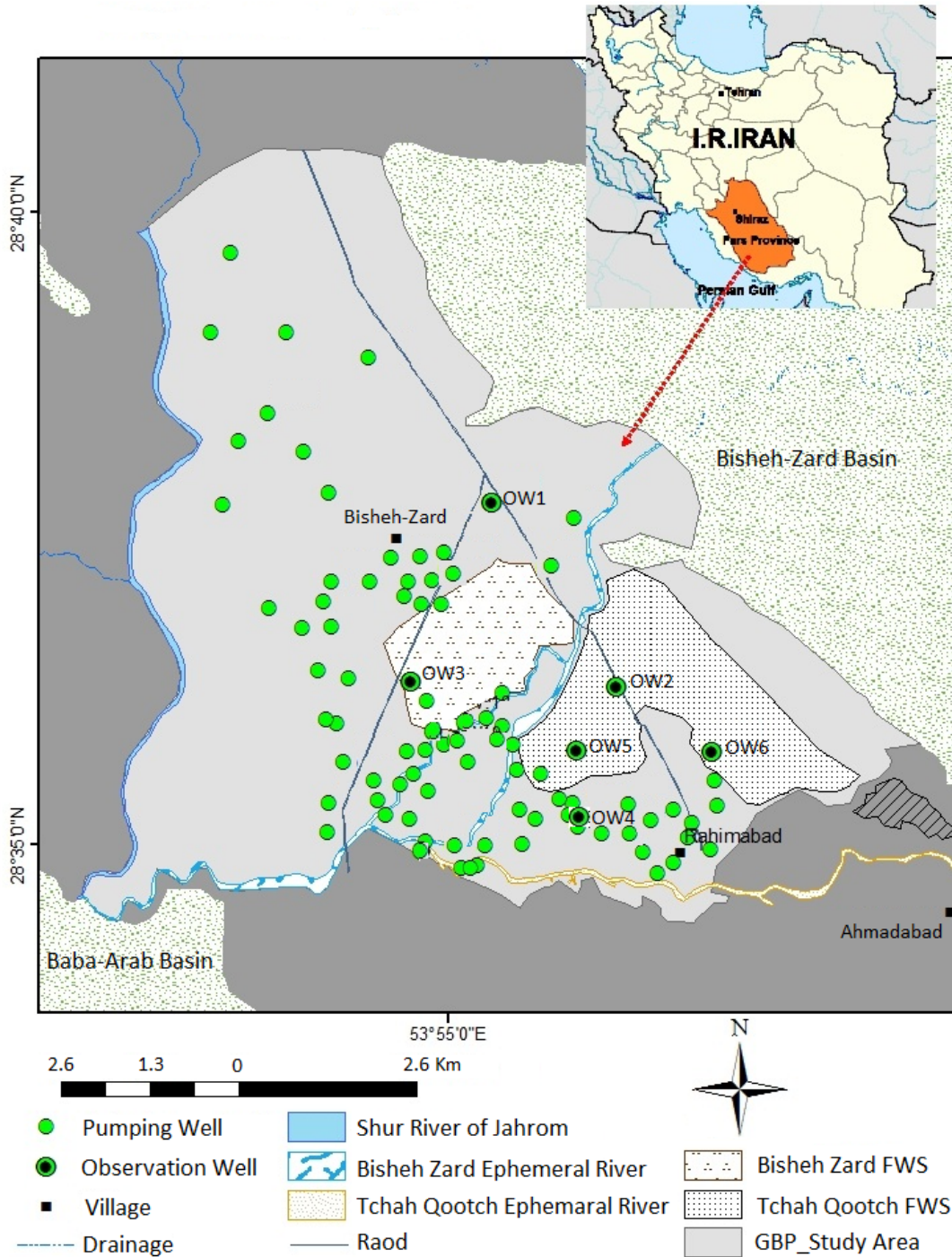
"For this, three GCMs scenarios, A1B, A2, and B1, were used as input to a coupled one-way surface-groundwater model as it was assumed there is no interaction between surface water and GW resources due to deep GW level, for the periods 2010-2030 and 2030-2050."

In addition, this issue, one-way coupling, was also addressed on page 11809, line 8-11.

3. Description of the study site and observation data

- Figure 1 is not sufficient. I think you need a map that outlines the domain in addition to showing the location of all observations points, the recharge basin and the surface water bodies referred to in this section.

Thank you for the suggestion. Accordingly, the below figure will replace Figure 1 in the revised version of the manuscript.



- 11805, 17: Define 'FWS' before you use it.
Abbreviation will be added to the text on page 11805, line 17.
- 11805, 20-21: Reword 'which then the'
"which then the" changed to "in which" as follows:
"The system diverts surface runoff from the ephemeral rivers onto the consecutive recharge basins "in which" the floodwater infiltrates down and percolate to GW reservoir."
- 11805, 26-27: Over what period of operation did this decline occur?

"after 1996" will be added to the text.

- 11807, 15-7: I still don't know what you mean when you say that the 'GW recharge model only works with the flood periods'.

The recharge package in MODFLOW is used to simulate a specified flux distributed over the top of the model i.e. recharge basins and river channels, and specified in units of length/time i.e. flood period or flooding time. In another word, the magnitude of flood i.e. flood discharge rate per time unit, cannot be specified in the recharge package however, can be incorporated into simulation by extending or diminishing the recharge areas.

- 11807, 26: How are the aquifer hydraulic parameters estimated?

Aquifer hydraulic parameters estimation are explained in detail in Hashemi et al. 2012 and 2013.

4. Methods

- 11808, 10-11: What do you mean when you say, "all future GCMs output data were assigned for the two twenty-year periods"?

The sentence was modified according to below:

"It is noted that all future CGCMs output data (from 2010 through 2050) were divided into two twenty-year periods, 2010-2030 and 2030-2050. Accordingly, the most recent twenty-year historical data, between 1990 and 2010, were used to generate climate data for hydrological projections by repeating this twenty-year observed data in both periods of climate scenario."

- 11808, 18-19: This sentence is unclear. Are you saying that you will have 365 values for each of the 36 grid cells?

*No, only one value (average) out of 36 grids for each single day (in total 365days*20years). The sentence was also slightly modified as follows:*

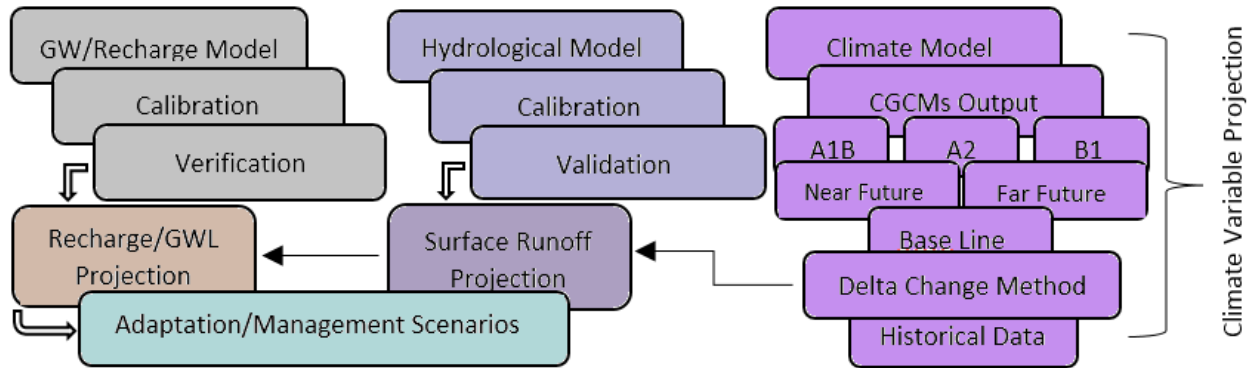
"In the first step, as the local conditions may be varied from what we observe from large scale and in order to regionalize the CGCMs outputs of the entire country, average daily values of all 36 grids covering the whole country were calculated. The calculations were done for both baseline and future scenarios in order to achieve only one value for each single day out of 36 grids."

- 11808, 22: Where is this data collected and how many stations are there?

As mentioned before (page 11806, lines 25-27), the observed data were collected from the nearest climatic station to the studied area, named Baba-Arab, with more than 40 years recorded daily data.

- Figure 3 needs improvement. I suggest having different shapes or shading to differentiate between, models/tools, datasets and actions.

Thanks for the suggestion. The figure was modified as below.



- 11810, 11: 'intention' not 'intesion'

Thank you. Corrected.

- 11810, 14: Please explain GMS.

The below text will be added to the revised version of the manuscript.

"A GW flow and recharge model was developed for the GBP using Groundwater Modelling System 9.1 (GMS). GMS is designed to provide tools throughout all aspects of the modelling process, e.g. pre-processor and post-processor, which can be used for various GW numerical modelling operations (Owen et al., 1996). To provide an integrated environment within GMS, a complete graphical user interface for the MODFLOW-2000 (Harbaugh et al., 2000), among other models, has been developed within GMS. For estimating aquifer hydraulic parameters and simulation of GW flow in the studied hydrogeological basin, a conceptual numerical model was created using the MODFLOW-2000 finite-difference model."

- 11810, 27: What is your definition of a 'flood event'?

The inundation of ephemeral river channel and artificial recharge basins that are normally dry throughout a year.

- 11811, 2: What is the 'recharge parameter' and how exactly is it used?

"Recharge parameter" was replaced by "recharge rate".

"Recharge rate was estimated separately, during a 14-year calibration period (1993-2007), for all artificial recharge basins and river channels (Hashemi et al., 2013). For future projections, the average estimated recharge rate ($m\ day^{-1}$) was assigned into the model."

- 11811, 6: reword 'recharge is taken place'

"recharge takes place"

- 11811, 22: How is groundwater abstraction modeled in your setup?

Besides gathering well pumping discharge data from the Fasa district water organization, a lot of time was spent measuring the discharge rate in each pumping well using a water discharge gauge (Jet ruler) and determining the exact location of the pumping wells using a GPS. Collected data (average discharge rate for each well) was used for the unsteady model simulation. Since there were no available data for agricultural return flow, 10% of total discharge from the pumping wells was subtracted as a discharge input to the unsteady model. However, it was modified during the calibration process of each

unsteady-state model in order to achieve the best agreement between observations and simulations (Hashemi et al., 2013).

- 11811, 27-28: What do you mean by ‘in order to consider the efficiency of the system’?

The sentence was modified as follows:

“In the second scenario, pumping scenario was assumed the same as scenario one, but the artificial recharge areas was decreased by half in order to consider the influence of the FWS on the GW reservoir taking into account the size of the system.”

5. Results

- 11812 19-20: This sentence is confusing, please reword.

The sentence was modified as follows:

“This is because the GW model was calibrated based on available monthly-recorded data.”

- It is really hard to see the differences in Figure 4. Perhaps add the deltas to this figure.

Figure 4 includes deltas as well. The differences are also given (in percentage) in the text.

- 11813, 16: Why are you reporting the temperature changes as percentages? I though the deltas for temperature were additive not multiplicative.

We agree with the reviewer as the deltas for temperature was addressed on page 11808, lines 23-24 and it was shown in Figure 4 as well (“In the case of temperature data, the observed values (historical data) were scaled by adding the calculated differences between baseline and future scenarios”). However, in the result section the percentages were calculated based on the overall decrease in projected temperature in comparison with the historical data in order to be consistent with the presented result of precipitation.

- 11816, 1: From Figure 7 it looks like the starting water table levels for the future scenarios match the 1993 levels (~1143) not the 2010 levels. Is this correct?

As explained in the figure caption, the left y-axis represents the GWL for historical data (dash line) and the right y-axis represents the projected GWL (bold line).

- Figure 7: The titles should say recharge ‘areas’ not ‘sources’ to be consistent with the text.

Figure title was modified accordingly.

- Figure 7: Where are these groundwater levels taken?

They are depicted in the new version of Figure 1.

- 11816 27-28: This sentence is confusing, please clarify.

The sentence was modified as follow:

“However, the GWL increasing rate is still less than the declining rate recorded during the historical period.”

6. Discussion

- 11817, 6: What do you mean by ‘mainly a one-way coupling’? What aspects of this are not one-way? This is something that could be made clear in a revised version of

Figure 3.

'mainly' will be deleted in the revised version of the manuscript. We also believe that this is clear in the revised version of Figure 3 as the direction arrows are all toward one direction only.

- 11817, 14-15: What is the difference between a 'significant' increase and a 'slight' increase? Please clarify what you mean here.

The sentence was modified as below:

"The projected climate variables show slightly increases in rainfall under B1 and A2 scenarios in the near and far future, respectively."

- 11817, 19-20: I'm confused throughout this section on whether the climate impacts are serious or not. Earlier you say only slight, insignificant changes, but here you are saying serious. What classifies as a 'serious' impact? Also, I would like to point out that this is just one model so saying that xyz 'will' happen seems to be a bit of an oversell. I would prefer climate impacts 'are likely to' or 'are projected' to rather than 'will'.

This part of the text was modified as follows:

"In general, the projections show no significant increase or decrease in rainfall for the next 40 years however, based on emission scenarios and delta-change approach, the future climate are likely to replicate the climate patterns of the last 20 years (1990-2010). On the other hand, it is evident that the climate of the studied region during the historical period (1990-2010) has been suffering by drought in which there is already 40 mm decrease of rainfall in comparison with the average recorded rainfall from 1971 through 1990 (Fig. 5). The projections reveal that reduced rainfall amount is likely to be the predominant condition up to 2050. Thus, water stress seems to continue substantially affecting the studied region."

- 11817, 21: 'events increase', not 'event increases'

Corrected.

- 11817, 25: Tables 1 and 2 should be introduced in the results section.

As we shortly discuss the results presented in both tables in the discussion section, we prefer to keep it in this section.

- 11818, 14-16: This is a jump. I would delete this sentence.

This argument is based on previously observed city-ward migration of rural people in the studied area in the mid-1950, which was due to the over-pumping of the scarce GW resources in GBP.

- 11819, 3-5: This needs to be explained in detail earlier in the paper.

The statement is based on Hashemi et al., 2013, which is also cited in the same paragraph as reference.

- 11819, 5: 'results' not 'result'

Corrected.

- 11819, 16-17: Are you saying that all of your results are wrong because you can't reflect changes in GWL from recharge with numerical modeling?

As we mentioned in the text, this could be a possible explanation that opens a new window for further investigation. It is also stated that the modelling results are based on the available observed data and numerous field investigations. Yet, the modelling results can still be assumed substantial unless otherwise a new research provides further insight to the studied aquifer.

- 11819, 18: There was no discussion of the model calibration. How did you determine that your model is 'well calibrated'?

Model calibration/verification is well documented in Hashemi et al., 2012 and 2013.

7. Summary and Conclusions

- It sounds from paragraph 2 like you are saying there are no significant climate impacts. However in the abstract you present increased flooding as one of your primary findings.

We do not see any contradiction between the presented findings in the abstract and conclusions. However, in order to be more consistent in both sections, "increase" will be changed to "slightly increase" in the abstract (line 13).