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Hydrology and Earth System Sciences Discussions

Interactive comment on "Quantifying the impacts of human water use and climate variations on recent drying of Lake Urmia basin: the value of different sets of spaceborne and in-situ data for calibrating a hydrological model" *by* Seyed-Mohammad Hosseini-Moghari et al.

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Dear S. Khatami,

We would like to thank you for your interest on our manuscript and your efforts to help us improve the manuscript. Below, we have provided a point-by-point response to your comments.

Comment#1: In their analysis, authors only considered the time period 2003-2013. It is not clear why both earlier years and the most recent years are excluded from the analysis. It is well-known that there has been significant changes in the basin and the lake water level since late 1990s. Using a statistical change point analysis, Khazaei et al. [in review] identified the year 2000 as the beginning of the period with significant changes in the lake dynamics. So, it is crucial to include all the years post 2000âĂŤthe data availability is not an issue as ULRP now provides researchers with required data. For instance, annual inflows to the lake are highly variable during this period (see the supplement figure), which you also used for calibrating the model. Given such degree of variability, the modelling results are likely sensitive to this variable, and it is important to use the entire time period.

Response: Data required for our analysis, except the data you mentioned, is not available before 2000. For example, GRACE satellite was only launched in 2002, and irrigated area reported by ULRP starts from 2001 and ends in 2012 (see Figure 16 in Kamali et al. (2015)). In addition, there are many gaps in groundwater data before 2000. If we want to work only on long term inflow or estimated water use based on empirical equations, we would have repeated the works were done by Shadkam et al. (2016) and Chaudhari et al. (2018), respectively. Furthermore, Iran Water Resources Management Company release data are available with two/three years' delay. Thus, in addition to irrigated area, in situ groundwater data precipitation are not available for the recent years. To sum up, be sure to perform a multi-objective calibration, we consider maximum time interval.

Comment#2: You have assumed that the irrigated area in 2013 remains the same as 2012. This assumption is wrong and known to be wrong, as we know that the irrigated area has increased since 2012. Chaudhari et al. [2018] estimated the cropland area of the basin using both MODIS and HYDE 3.1 products (also notice the difference between these two products on Fig 3). Land use change is a major driver in this basin. Expansion of the irrigated area has increased the evapotranspiration losses, and consequently lead to less runoff in the basin and in turn less inflows to the lake. Therefore, I expect a useful model to be highly sensitivity to this input, and thus improper handling of this input data could be major source of uncertainty.

Response: Considering Figure 3 in Chaudhari et al. (2018), the cropland area from 2011 to 2013 did not increase everywhere increased but also decreased partially (see south part of the basin). This agrees with our data source.

Comment#3: It is not clear how representative the 248 groundwater (GW) wells data (section 2.23) are for the deep GW withdrawals in the basin. Failing to include deep GW abstraction can bias your results underestimating both groundwater and net abstractions in the basin (Figure 7), which seems to be the case. ULRP [2015] estimated the total amount of groundwater extraction in the basin, in 2013-2014, round 2,200 MCM, of which around 900 MCM were extraction from shallow groundwater and about 1000 MCM from deep groundwater. In your results, the estimated GW abstraction, for the same year, for the most input-comprehensive variant of RS_Q_GW_NA are around 1,200 MCMâĂŤwell below the actual estimation. The groundwater extraction in the basin has also had an enormous impact on the inflowing runoff to the lake as well.

Response: Unfortunately, we cannot find the reference you mentioned. Groundwater abstractions in WGHM are not differentiated between deep and shallow. Anyway, Figure 7 does not show total abstraction but as also described in the text, net abstraction, i.e. groundwater abstraction minus return flow from irrigation to groundwater. For example, based on Table 1, in 2009 abstraction from groundwater was about 2092 MCM (see Table 1) but net abstraction in 2009 is about 1300 MCM (see Figure 7). Hence we did not underestimate groundwater withdrawals if you consider the difference between abstraction and net abstraction.

Comment#4: Lake Urmia is a highly regulated basin, so embedding water withdrawal/consumption in the basin's model is crucial. In doing so, however, you have used the water withdrawal records only for the year 2009. There is a high (year to

year) variability in water withdrawal/consumption in the basin, implicitly indicated by the high variability of the inflow to lake, as presented in your Figures 7 and 8. Year 2009 has one of the least amount of inflows to the lake (Figure 7) implying possibly a high water consumption in the basin. That said, given the variability of water withdrawal/consumption in the basin, including a single year is not sufficient and could lead to significant bias/uncertainty in your modelling results. Further, in using water withdrawal/consumption records be aware of possible inconsistencies between the definition and estimation of water withdrawal/consumption/use by different water agencies/authorities or studies, and the related metrics or model fluxes. Such inconsistencies could lead to methodological fallacies and unreliability of the analysis results [Madani and Khatami, 2015].

Response: In WaterGAP, water consumption, abstraction and net abstraction are computed as a monthly time series taking into account, in case of irrigation, time series of climate forcings, and irrigated area. To improve this estimation, we included basin-specific data on the temporal development of irrigated area (Fig. 4) and the year 2009 value. We believe that this is the best to utilize the existing information. In addition, we determine year to year withdrawal aims to reach minimum difference between in situ and simulated groundwater time series and inflow into the lake. A comparison of the statistical value (Table 1) and Figure 7 indicate that this type of approach is reasonable. Finally, we aware of the definitions and possible inconsistencies.

Comment#5: Based on table 2, in the variant RS_W_GW the parameters α and β vary between 0.45 to 0.47 and 0.47 to 0.52, respectively. That said, by introducing the NA data into the variant RS_Q_GW_NA the parameter ranges expand significantly to 0.29 to 0.56 for β and 0.39 to 0.63 for β. This implies that the model is not benefiting from the additional information content. For instance, there is no significant improvement from RS_W_GW to RS_W_GW_NA evidenced by Figure 8 and metric values on table 4; for years 2003, 2004, 2007, and 2009 the RS_Q_GW variant is even better than the RS_Q_GW_NA in terms of estimating the annual inflow to the lake (Figure 8b).

That is, the model is insensitive to adding new data NA. Instead the new information is compensated by the model parameters. It is an indication of over-parameterisation in your model, which is expected for annual multipliers, that undermines the reliability of the results. While I understand the rationale behind a year-specific parameter value, it is a serious issue. Instead, you should do a more efficient and effective parameter search (instead of manual calibration) finding one or (more desirably) a number of acceptable parameter sets. Then to ensure their reliability you can do a year-by-year evaluation of the model performance, i.e. evaluating the model performance against a given metric for each year separately. Also, it would be helpful to include a schematic of the model structure demonstrating its fluxes, storages, and their interconnection. This can help the reader to better understand the mechanism and process-representation of the model.

Response: We have another viewpoint regarding this issue which a lesson we have learned. The models outputs can be changed by using parameters modification or input modification. As you mentioned, model variants RS_W_GW and RS_W_GW_NA show similar results. It does not mean the model is insensitive to adding new data. It means that the model parameters can compensate errors in input data. Therefore, when the model is run under natural situation, the result is dubious. As a result, a holistic calibration needs a multi objective calibration. We do agree that having individual correction factors for each year can be considered as over-parameterization, and on might consider to have another model variant with temporally constant correction factor. However, this would add to the already high complexity of the manuscript, so we prefer to not show the results of such an experiment. Instead, in the revised version, we will have a new discussion section in which we will discuss the topic of over-parameterization.

WaterGAP schematic well documented in some papers. So to avoid repetition, please refer to Döll et al. (2014) who presented the WaterGAP structure in more details.

Comment#6: It is a well-established fact that CC is an inadequate measure for model

evaluation [Willmott, 1981]. It is especially redundant to use CC together with NSE, as CC is already included in the NSE metric; see the NSE decomposition by Murphy [1988] and Gupta et al. [2009]. Further, NSE puts more emphasis on the larger values, e.g. as Pushpalatha et al. [2012] showed it focuses on the top 20% of discharge flows. Therefore, calibration by NSE introduces bias. So, it's more useful to combine NSE with bias rather than RMSE. Furthermore, other metrics such as Willmott's refined index of agreement [Willmott et al., 2012] and KGE [Gupta et al., 2009] shown to be better than NSE.

Response: We know that a high value of CC does not mean that the model is highly accurate. However, we disagree that "CC is an inadequate measure for model evaluation". CC is suitable for detecting synchronous behavior of two-time series, which is not done by NSE. As shown by e.g. Gupta et al. (2009), NSE reflects already the bias, i.e. if bias is large, the NSE will be low. So it seems redundant to show both the bias and the NSE. About Pushpalatha et al. (2012), please consider they have focused on low flows in daily time scale which time series has too much fluctuation. Hence, their results cannot be extended to the much less variable annual flows we analyzed (see Figure 8). Still, we could extend Table 4 by showing the three components of the KGE (bias, correlation coefficient, variability) in addition to NSE and RMSE.

Comment#7: Also, on P 5 L 25-29, you already explained that the standard WGHM is not calibrated for LU basin. Yet you reported the results of the standard model on Figures 7-8, and discussed it through the results and discussion. including the standard variant in your results and discussion does not serve the manuscript any benefit other than adding to its bulk. Further, it is also a well-known fact that (hydrologic) model cannot be validated [Konikow and Bredehoeft, 1992; Oreskes et al., 1994]. Therefore, using the term validation is both semantically and theoretically wrong. As a matter of good practice, it's been recommended to use the term evaluation instead of validation [Beven and Young, 2013]. Same comment applies to terms such as optimal values and optimal fit throughout the manuscriptâĂŤthere is no optimal set.

Response: One of the main goals of our manuscript, as also expressed in the title, is to determine "the value of different sets of spaceborne and in-situ data for calibrating a hydrological model". We need to investigate the likely improvement of our modelling performance after adding each new dataset. We agree "evaluation" is a better word instead of "validation". We will revise it. However, de Marsily et al. (1992) rejected the claims about the impossibility of model validation. Also about the optimal fit, we believe in reality there is no "global optimal" set for calibrating a hydrological model while we can determine "optimal" set. However, we will use "the most suitable set" phrase instead "optimal set" to prevent any misunderstanding.

Comment#8: In this study you have not investigated the model parameter space other than four calibration variants, while the calibration is done manually. So, first, there is no way to justify that the manually calibrated parameter values are the best-performing calibrations (despite what you said e.g. on P1 L16). Further, no sensitivity nor uncertainty analysis is performed which is nowadays a requirement for publishing a modelling analysis in the hydrologic community. Without any sensitivity/uncertainty analysis the reliability of the modelling results is questionable. Other than the model structure and parameters, the role of data uncertainty is important and should be discussed. For instance, the annual inflows are not the exact inflows to the lake. They are estimates of the last station, sometimes as far as 50 km from the lake.

Response: We agree with you regarding the manual calibration. But in case of multi-objective calibration, also non-manual methods of optimization may not lead to an overall optimum due to the complexity. A manual approach appeared to be more transparent to us. About the uncertainty, we agree. Uncertainty can be entered to modeling in different sections as you mentioned such as uncertainty in input data, model structure and so on. We have to accept uncertainty in in-situ input data such as your example about the inflow into the lake. These uncertainties are inseparable from nature and all the hydrologists know about it. We think our study contribute to elucidating uncertainties in understand freshwater systems by showing the different outcome/simulations

of reality when utilizing, for the four model variants, different observational data types. Ideally, one would be able to quantify uncertainty of observation and include this in the calibration process, but this appears beyond our scope. In the revised version, we will have a new discussion section in which we will discuss different type of uncertainties that affect the study outcomes.

Comment#9: There are fundamental issues in the discussion within the first paragraph of section 3.2. First, adding a new input data into model calibration/evaluation does not necessarily decrease the modelling uncertainty. In fact, by adding each new data you're introducing a new source of uncertainty as there is also uncertainty associated with data themselves; especially in a case like LU basin where the data uncertainty is very high both for ground and remote sensing data. The model parameter equifinality is not necessarily a problem [Savenije, 2001], in fact it can help us to improve our modelling in the face of uncertainty [Beven, 2009]. As all (hydrologic) models are wrong [Box, 1976], model ensembles-as multiple working hypotheses-are better suited than calibrated model with a single parameter set to describe/predict hydrologic systems given the uncertainties [Beven, 2012; Beven et al., 2012; Chamberlin, 1890; Clark et al., 2011; 2012]. Despite your statement, parameter equifinality does not ask for additional data! Adding data, in fact, may even exacerbate the model parameter equifinality, and one cannot make up for parameter equifinality by adjusting the parameter values. For instance, the model structure may not be able to benefit from the additional information content, and therefore the new data is redundant (which seems to be the case comparing the results of variants RS_Q_GW and RS_Q_GQ_NA). What parameter equifinality implies is a more thorough search of the parameter and model space as well as more rigorous model evaluation schemes such as limits of acceptability approach [Beven and Binley, 2014]; even then the parameter equifinality will remain. In other words, each model variant is prone to model parameter equifinality. Further, parts of the discussion in this section are not new lessons (e.g. limitations of global hydrologic models in paragraph 2) and are well-established in the literature. Also, section 3 is very long, sometimes discussing too much details. I think it'd serve your discussion better to provide the top 3-5 main learned lessons as bullet points early on in the section. Then briefly explain each bullet point. The rest, especially modelling technicalities which may be valuable particularly for the reproducibility of the study, could be provided as supplement. In doing so, it's particularly helpful to restructure the result discussion by, first, explicitly discussing the limitations and uncertainties associated with the modelling design and consequent results. Second, discuss what results are therefore more/less reliable given the uncertainties. Third, what are the new findings of your studies and where do the results lie within the extant litertature. Finally, wat are the organic future research questions/directions based on what you learned and what lacking in the current literature.

Response: We disagree with your statement that adding observation data into models increase uncertainty because such data helps to understand more correctly the system behaviors. Unfortunately, we do not really understand your statements about "equifinality". While the approach for using parameter ensembles is attractive (and there is currently research done on ensemble-based calibration of WaterGA), Beven and Freer (2001) did state that equifinality leads to high uncertainties in the hydrological modeling. Further, Hamilton (2007) indicated that equifinality is only the result of parameter abuse which would prevent using a proper selection of parameters. In addition, Ebel and Loague (2006) showed that equifinality is not an unsolvable issue if we used wide range of observational data in modeling. Also Hamilton (2007) and Efstratiadis and Koutsoyiannis (2010) stated that by a multi-objective calibration we can deal with this issue. Thank you for suggesting an improved structure of section 3 that will help us for writing the revised version of the manuscript.

Comment#10: On P 2 L 12-16 you stated that the decreasing trend in precipitation (P) and increasing trend of temperature (T), and thus increased evaporation, has very likely to contributed to the decrease in the lake volume. This is also reported as one of the main reasons for lake degradation on P3 L12. This statement is debatable. First, in our recent analysis [Khazaei et al., in review], we showed that the decrease in P

and increase of T is not considerable in explaining the shrinkage of the lake; nor the decrease in T can be associated directly with an increase in lake evaporation. The major driver of the basin, as stated before, is the land use change and the substantial expansion of cropland areas. This has led to increase in the irrigation hence less available runoff as for the lake inflow, and also caused a major increase in evapotranspiration. The following sentence (last sentence of the paragraph) does not explicitly indicate the greater role of human activities in the lake desiccation compared to atmospheric climate change, which is the common finding of the most of the studies in this area [AghaKouchak et al., 2015; Aneseh et al., 2018; Stone, 2015; Torabi aghighi et al., 2018; Vaheddoost and Aksoy, 2018]. On P 24 L 34 you concluded that "climate change must be constrained to prevent strong decreases of precipitation and runoff". It is not clear to me what you mean by constraining climate change here. Also, as discuss previously the role of human activities are more substantial in the lake's fate than atmospheric changes. Further, on P 3 L 2-3, you have discussed the role of drought in the lake's water level decline. First, the term drought is ambiguous, and it should be further specified what type of drought is discussed; atmospheric, hydrologic, agricultural, ecologic, or anthropogenic. Second, the analysis by AghaKouchak et al. [2015] indicated no considerable trend in droughts, at 0.05 significance level, during the past three decades. They argued that the region has undergone even more severe multi-year droughts in the past that did not cause a major change in the lake's surface area. They, therefore, cautioned against overrating the role of drought on the drying of the lake and disruption of its water balance.

Response: We clearly stated in the first part of the introduction that both climate and human activities affected the LU basin and later review in sufficient depth the studies that, taken together, indicate just that, that it both drivers are important, and each study finds different weights for the two depending on the time frame and variables considered and the study approach. For example, as described in the manuscript, Shadkam et al. (2016) showed that the role of the climate on inflow into the lake is significant. Farokhnia (2015) who assessed the impact of land use change and climate variability on Urmia Basin found that climate impact is the main reason of inflow reduction (This research is in Persian his results in English reported in Shadkam et al. (2016)). Some of the studies that found an insignificant impact of climate might have been too simplistic, such as AghaKouchak et al. (2015) who only assessed the precipitation amount while also a changes in precipitation frequency and pattern are important (which is included in our study). While the studies you mentioned considered only monthly or annual time series, the studies we mentioned above worked with daily time series. As a result, the former missed the effect of changes in pattern or precipitation frequency within a month. This changes were recently reported by Bavil et al. (2018) and Hosseini-Moghari et al. (2018) based on analysis of daily precipitation. Bavil et al. (2018) stated the frequency on precipitation event less than 5 mm/day increase significantly over basin during past decades. As a results, we can see two trends in the precipitation data. The first trend is a decreasing trend in annual precipitation and the second trend is related to ineffective precipitation events (less than 5mm/day) which have an increasing trend. Ineffective precipitation means that the precipitation value just increases surface soil moisture and after a while will evaporate. Thus, with increasing ineffective precipitation events we have less runoff with same amount of monthly precipitation. To sum up, we found that the drought studies (or precipitation trend studies in monthly or annual time scale) which assessed drought events over the basin work on monthly data. Hence, they cannot consider the change in daily precipitation pattern within a given month.

Independent of the submitted manuscript, we have in the meantime analyzed the inflow into the two main dams (Bokan and Mahabad) between 1992-2013 where human water use or cropland development is insignificant. We also analyzed the daily precipitation over the basin based on 63 rain gauge stations during 1992-2013. We calculated the anomaly of annual precipitation and depth of precipitation less than 5 mm as the ineffective precipitation (the sum of precipitation for all precipitation events less than 5 mm/day during a year) (Fig. 1a). This figure shows the ineffective precipitation increased significantly meanwhile annual precipitation decreased. This increase

in ineffective precipitation is not reflected in drought analysis at monthly or annual time scale. Fig. 1b shows inflow into the dams along with annual precipitation anomaly. As shown in the figure since 2005 onward the difference between precipitation and inflow to dams has increased. To sum up we believe that most studies which reported insignificant climate impact did not consider ineffective precipitation and change in precipitation patterns and as a result, they underestimated climate effects over the Lake Urmia basin. In addition, the impact of increased temperature was not taken into account, different from what happens if hydrological models are used for the analysis.

[Fig. 1]

We agree that the wording “constraining of climate change” is misleading. In the revised version, we will change it to “global-scale mitigation of climate change by reducing greenhouse gas emissions”.

Comment#11: This is a technical note: you have used CC and NSE (P 9 L 11-12) to cross compare precipitation and temperature records of difference sources. First, I assume by CC you meant Pearson CC, which should be explicitly mentioned. Second, both CC and NSE are sensitive measures, i.e. a few number of large outliers can significantly change their values; especially for skewed distributions. It is better to use (more) resistant alternatives such as Spearman ranked correlation (instead of Pearson correlation) and Willmott’s refined index of agreement [Willmott et al., 2012] (or ideally normalised the data using a transformation such as Box-Cox, first, and then compare the time series distance).

Response: Thank you. We will clarify in the text that Eq. 5 computes the Pearson CC. Further, Spearman ranked correlation is usually used to determine the correlation between ordinal variables. Spearman ranked correlation evaluated monotonic relationship between two variables. In monotonic relationship, the variables changes do not necessarily happen with a constant rate. Thus, for instance, if both variables are increasing, but change rates are not consistent, the Spearman coefficient equals +1 while Pearson correlation is less than +1. Therefore, when we compare the same variables such as precipitation the rate of change also is important. Hence, in our opinion, the Pearson CC is the most suitable indicator in our case as we do not correlate e.g. runoff and precipitation. In the revised version, we will therefore additionally provide the Spearmann ranked correlation coefficient and the Willmott index.

Comment#12: P 2 L 7: this is an unsubstantiated claim. As far as I know there is no (reliable) evidence on the degree of awareness regarding this issue. Please remove it, or provide the evidence.

Response: We will remove the sentence.

Comment#13: P 25 L 9: It is better to explicitly acknowledge the organisations that provided you with GRACE and climate data, and the URL links if available.

Response: We will do so.

Comment#14: P 28-29: The URLs in the ULRP references are not accurate. Please update them.

Response: We will update them in the revised version.

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Thank you very much again for your time and hope our response have been resolved your ambiguities.

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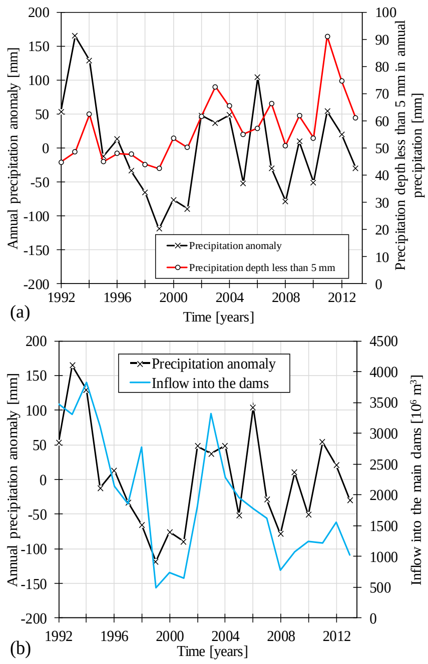

Fig. 1. Time series of annual precipitation anomaly and precipitation depth less than 5 mm (a), and annual precipitation anomaly and inflow into the main dams (b).