Response to Reviewer # 3

We thank Reviewer # 3 for the detailed evaluation of the paper and the helpful comments, which will further improve this paper. We are confident to adequately address each comment and our reply are highlighted in blue normal font, while the reviewer's comments are in *italic font*.

General comments

This study presents an interesting investigation regarding the human impacts on river discharges and hydrologic droughts risks. To this end, a robust modelling approach was adopted, allowing the authors to assess the changes in streamflow caused by the construction of several reservoirs in the study area. The contribution of this paper, although relevant, is limited by a number of factors that, if addressed, could reveal a greater potential provided by the data.

Thank you for your appreciation of our effort. We have addressed all of your comments in our revised manuscript.

From my perspective as a non-native English speaker, the manuscript is well written but the ideas need to be better presented. For instance, the reader leaves the Methods section unaware of relevant information (model parameter, model calibration, etc) and is surprised with them in the Results section.

Answer: Thank you for this suggestion! We will include more information about the model parameters, calibration procedure in the method section. We have reorganized the Data and Methods section as follows: instead of presenting the Data and Methods in one section, we have separated them: 2. Data, 3. Methods. A detailed description about the hydrological model has been included in the Methods section which incorporates the calibration procedure, the parameter estimation and model efficiency statistics. In addition to this, a detailed explanation of the model can be found in Fink et al. (2013) and Nepal et al. (2014).

Although the general idea is crystal clear to me (to assess the hydrologic impacts due to the construction of dams), the means of doing so need to be clearer. Because the paper relies on three different time series (observations, naturalized and reconstructed discharges), the reader needs to understand how each one will contribute to the analysis. This could be better explained in Data and Methods, as indicated in the list in Specific Comments. Another issue is that it is not clear in Data and Methods if the naturalized discharge refers to the undisturbed discharge from 1980 until the construction of the dams or is a simulated data. There might not be enough time to address all suggestions, but there are some points that require more attention.

Answer: We appreciate the comments. The definition of "naturalized" and "reconstructed" discharges were described in method section 2.2.1 (p 6, line 14 – 24). For our drought risk assessment, we simulated the "naturalized" and "reconstructed" discharge to be able to evaluate the changes of streamflow due to reservoir construction. These simulated time series were calibrated against the observed period. The naturalized discharge is the output from the J2000 hydrological model, which simulates discharge for pristine conditions without the intervention of the reservoirs, while the "reconstructed" discharge is the output of the reservoir simulation model, that accounts for the hydropower operation influences in the streamflow for the same time from 1980 – 2013. In our analysis, observed data – referred to as the measured discharge data at the stations are only used for the calibration and the evaluation of the simulated results.

Specific Comments

Introduction

P4, L12: I believe this sentence is incomplete or "is" should replace "however". Please check that.

We have changes the sentence accordingly, “The climate in the VGTB basin is characterized by a strong rainy season lasting from September to December.”

P4, L13-14: Those ranges are not clear. Almost 65 or 80 %? 70 or 85 %? Is the word “respectively” missing somewhere in this sentence? If you want to specify the range, I do not think this is the best way to do that. Please rephrase.

Thank you for the suggestion. We have rephrased the sentences as follows:

Rainfall during the wet season accounts for 65 to 80 % of the total annual rainfall, with 40 to 50 % of the annual rainfall occurring in October and November regularly causing severe floods (Souvignet et al., 2013).

P4, L14: I believe a “.” is missing at the end of this sentence.

We have corrected it and put a “.”.

P4, L15-16: How often, e.g. n times in the past y year: : :? Is this statement based on the author’s experience or it is possible to cite someone who verified this information?

Thanks for the comment. We have changed the sentences as suggested.

“The extended dry season lasts from January to August and is frequently accompanied by droughts (e.g., in year 1982, 1983, 1988, 1990, 1998, 2005, 2012 and 2013)” Nauditt et al.,2017

P4, L16: Please either replace “month” by “period” or “is the driest month” by “are the driest months”.

The sentence is corrected as suggested.

“February to April are the driest months, accounting for only 3-5 % of the total annual rainfall, resulting in severe water shortages and problems with saline intrusion at the coast (Souvignet et al., 2013)”

Data & Methods

P5, L4: Are these records available online? If so, please provide an address and indicate when it was last accessed.

Answer: Sorry for not making this clear. These data are not freely available and were bought them in the scope of a BMBF funded research project (www.lucci-vietnam.info). We now included a sentence in the data section about this as well as the missing acknowledgements.

P5, L5: The map in Fig. 1 shows only 12 rain gauges but here it is said that 16 were considered. Please indicate the remaining gauges on the map.

Thank you for this hint. We have updated the map (see below), which shows the location of 17 rain gauge stations and the text has been corrected in the manuscript.

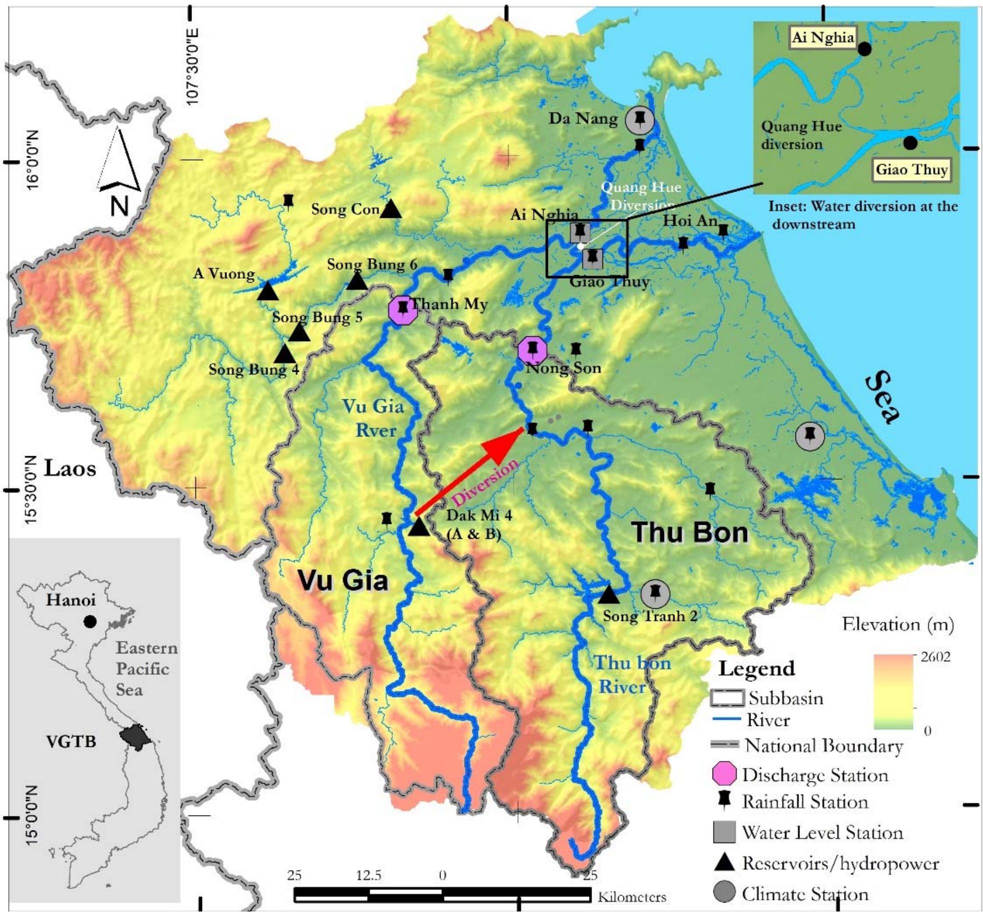

Figure-1: Map of the Study Area

P5, L15: What is the impact of such assumption?

Answer: We agree with the reviewer that this was not clearly explained. The flow diverted through the Quang Hue channel is strongly dependent on tidal changes and seasonal variation in streamflow of both Thu Bon and Vu Gia. Also, the impact is minor compared to the reservoir operation generated discharge from the tributaries. We did not incorporate the dynamics of the channel as there are no daily data on diversion flow. Our results also show that the contribution from tributaries to Vu Gia downstream of Thanh My are more relevant for the discharge at Ai Nghia. We included such an explanation to make this clear.

The following text is incorporated in the hydro meteorological data section in the revised manuscript.

There is no control mechanism how to release the exact amount of water (see Table S1, the routing rules of water diversion by MoNRE) diverted from Vu Gia to Thu Bon through Quang Hue channel. Hence, we in our study we assumed that Ai Nghia station is located upstream of the diversion of the Quang Hue channel to avoid complexity. We found that the proxy station can be well represented to capture the influences of reservoir impact on the downstream without having any potential errors, as it accounts for the overall water balance. It further helps to avoid any associate uncertainty due to the diversion which however is quite dynamic and difficult to predict without rigorous monitoring campaigns.

P7, L3: J2000 needs data on "land use, soil, geology, ….:" It is not mentioned how these information were acquired. Model parameter description was completely overlooked.

Answer: Thank you for your comment on the data issues for the J2000 model. We have not mentioned this in the original text as the data used for the J2000 model were described in Fink et al., 2013.

Soils: Soil map (1:100,000) (National Institute of Agriculture Planning and Protection, 2005) + 150 soil profile descriptions in the catchment to derive soil-model parameters for the various soil classes described in the map. Geology map (1:100,000) (Department of Geology and Minerals of Vietnam, Hanoi, 1997). Land-cover classification of Landsat images for the year 2010 (Avitabile et al,. 2016). The digital elevation model (DEM) was derived from contour lines and points from a digital map (scale 1:50,000) using the topography to-raster algorithm of ArcGIS. The resulted DEM had a resolution (cell size) of 25 m.

The following text has been incorporated-

A detailed description of the spatial (e.g., soil, vegetation, digital elevation model, land use and geology) and hydro-climatic data used for hydrological model was described in Fink et al., (2013, p 1828).

Model parameters and calibration procedure were not presented as the focus of this article was mainly to show the drought risk assessment based on the coupled modelling approach. These are described in Fink et al., (2013) and Nepal et al., (2014) and can also be found under http://jams.uni-jena.de/documentation/. However, we will include the parameter estimation values as supplementary material in the revised manuscript.

Table 1: Parameters selected for the model calibration (other parameters of the model were left to default values during calibration)

Calibrated parameters	**Short description**	**calibrated Value**	**Range**
soilMaxDPS	Maximum depression storage capacity	2	1.0 - 5
soilMaxInfSummer	Maximum infiltration in summer	40	1 -200
soilMaxInfWinter	Maximum infiltration in winter	100	1 -200
soilDistMPSLPS	MPS/LPS distribution coefficient	0.68	0 - 1
soilDiffMPSLPS	MPS/LPS diffusion coefficient	0.4	0 - 1
soilConcRD1	Recession coefficient for overland flow	1.2	1.0 -3.0
soilConcRD2	Recession coefficient for interflow	3.5	2.0 - 10
soilPolRed	Potential reduction coeffiecient for aET computation	3	1.0 - 10
soilMaxPerc	Maximum percolation rate	20	1.0 - 20
gwRG1Fact	Adaptation of the fast groundwater outflow	1	0.1 - 10
gwRG2Fact	Adaptation of the baseflow	0.4	0.1 - 10
gwRG1RG2dist	RG1-RG2 distribution coefficient	0.5	0 - 1
flowRouteTA	River routing coefficient	10	1 -100

The parameters that were calibrated are affecting three domains of the model. The most important ones are governing the simulation of the soil processes. The soilMaxDPS governs how much water can be hold back on the soil surface before surface runoff occurs. The soilMaxInfSummer and soilMaxInfWinter are there to influence the maximum infiltration in the dry and rainy season. The soil characteristics in the model are described by a dual porosity approach were the large pores (excess water) and the medium pores (usable field capacity) are represented in two different storages (MPS and LPS). The parameter soilDistMPSLPS is influencing the distribution of infiltrated water between LPS and MPS. SoilDiffMPSLPS affecting the diffusion from LPS to MPS. The recession coefficients soilConcRD1, soilConcRD2 are influencing the travel time of the runoff components surface runoff and interflow. The reduction of actual evapotranspiration (actET) to potential evapotranspiration (potET) is influenced by the soilPolRed which is a shape parameter for the actET, potET function according to the actual soil moisture conditions. The Groundwater runoff components (fast groundwater and base flow) influence the two recession adaption coefficients gwRG1Fact and gwRG2Fact. The distribution between these two components is influenced by the gwRG1RG2dist distribution coefficient. The recession in the river network is affecting the simulated recession in the river network.

Results

Although I appreciate straightforward analysis, section 3.1 is rather simplistic. Model calibration should not be done based only on statistics (R2, Nash, etc: : :). I would like to see a plot comparing simulated and observed discharges and a sensitivity analysis.

Answer: Thank you for this suggestion. Due to the high number of figures presented in the paper we could not show the hydrograph simulation performance. We therefore include the following figures to this response:

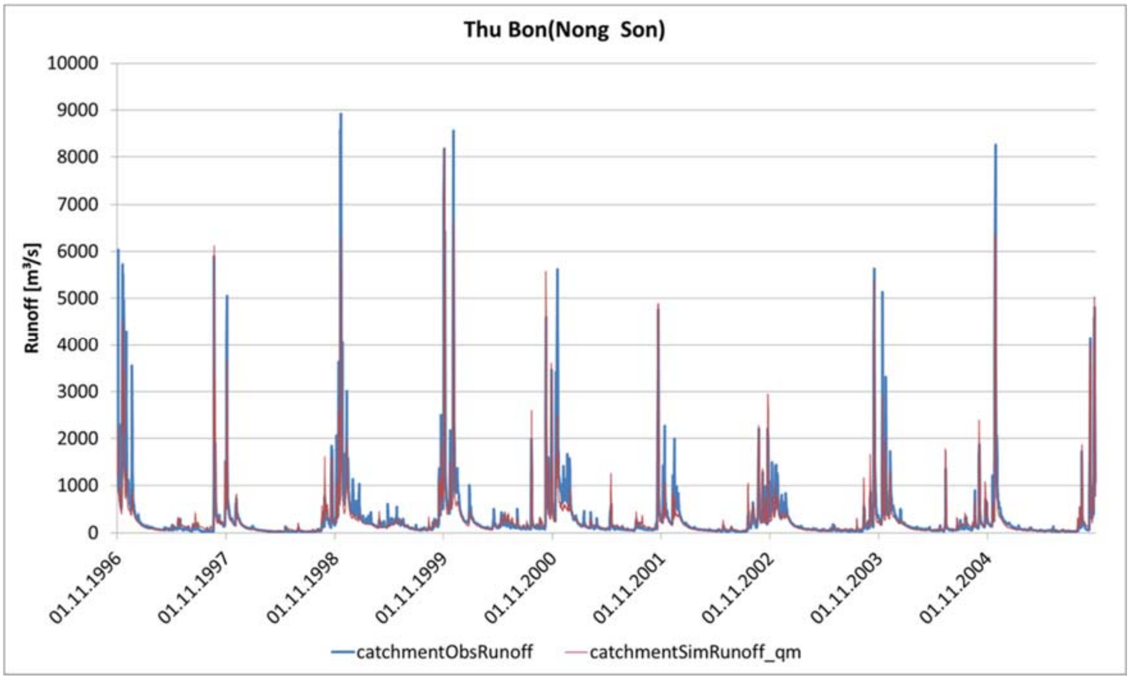

Figure 2: Hydrograph simulation compared to observed discharge (1996-2005) for the Thu Bon river (Nong Son station)

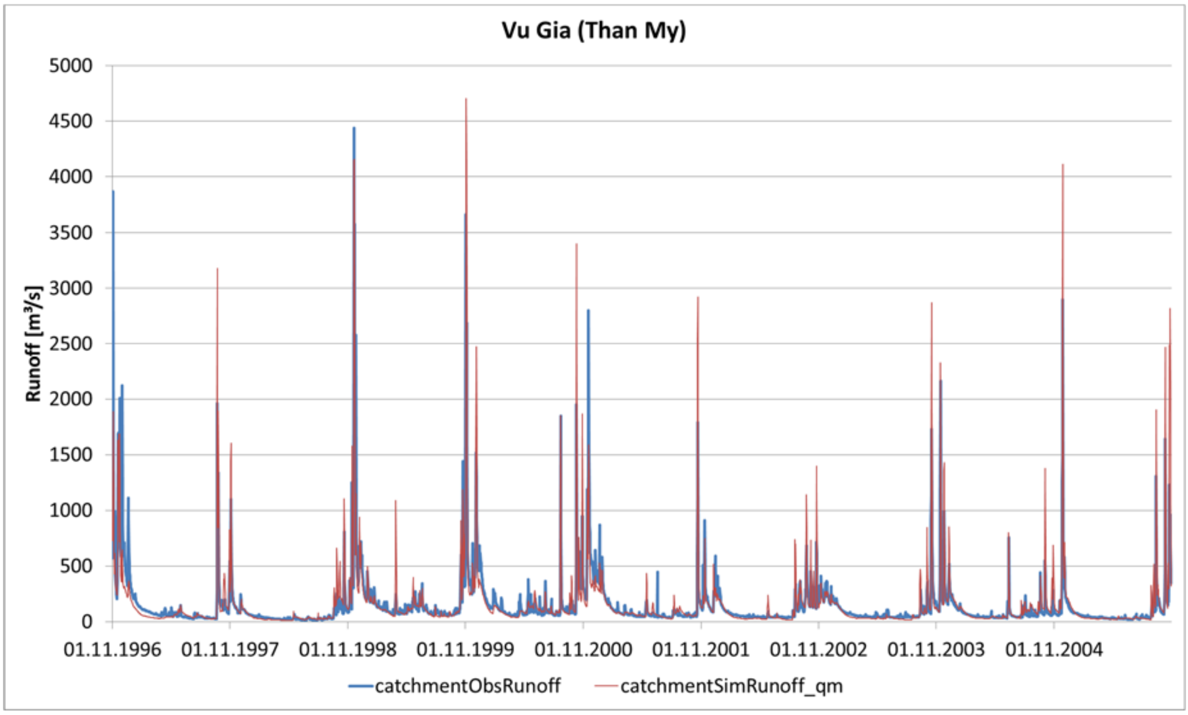

Figure 3: Hydrograph simulation compared to observed discharge (1996-2005) for the Vu Gia river at Thanh My station. Also please refer to Fink et al (2013).

Sensitivity Analysis of the calibrated parameters

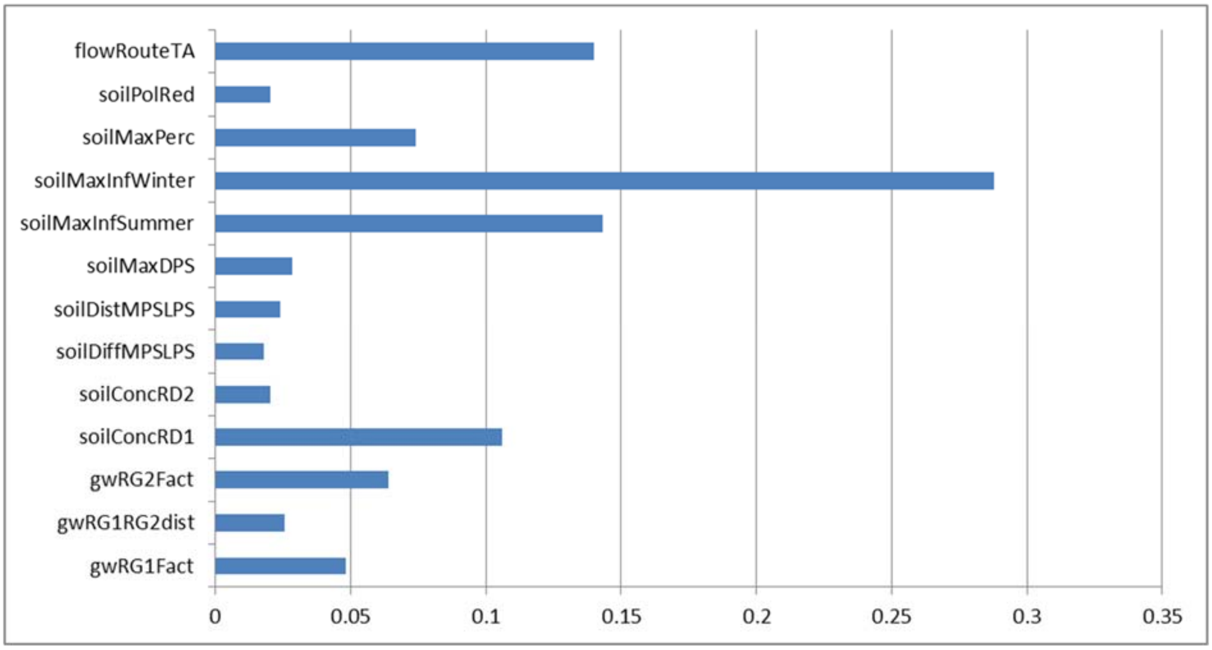

Figure 4: Sensitivity of calibrated parameters for the Thu Bon catchment (gauge Nong Son) with the Nash–Sutcliffe efficiency criterion.

Based on 1000 Monte-Carlo simulation we estimated the sensitivity of the parameters used for calibration. The method performed for this sensitivity analyses is the “regional sensitivity analysis” (RSA) (Hornberger and Spear, 1981) utilizing the Nash–Sutcliffe efficiency criterion. This method estimates the impact of a parameter and its interactions with model outputs (Nepal et al. 2012). The results in Figure 1 shows the importance of the different calibrated parameters for the simulated runoff according to the Nash–Sutcliffe efficiency criterion. This example shows that the parameters with the highest sensitivity (infiltration parameters, surface runoff coefficient and river runoff coefficient) are parameters which have their main influence on quick runoff components and peak flow. In contrast to that, the parameters which affect the overall water balance are less important (e.g. SoilDiffMPSLPS, SoilDistMPSLPS, soilPolRed). One reason is the use of the Nash–Sutcliffe efficiency with is focusing on the high flows, another the extreme water surplus in the rainy season where the evapotranspiration plays only a minor role.

Item 3.2 What are the results in the 1st paragraph? I suggest moving the proper parts to Methods and leave only the information that concerns the reservoir modelling process.

Answer: thank you for this suggestion. We agree that this part belongs to the Methods section and shifted it to in the method section.

I’m not comfortable with using the Q simulated by J2000 as reference just because “there are no gauging stations at Ai Nghia and Giao Thuy”. First, if what you have at Ai Nghia and Giao Thuy are water level stations that could not be used to derive river discharge estimates because of tidal effect, how is the tidal effect accounted for in your J2000 model? If it hasn’t been considered, how does that decision affect your analysis or it doesn’t affect at all? Also, how far upstream the tidal has some influence?

Answer: Thank you for these comments. The data of Ai Nghia and Giao Thuy stations show the following key constraints:

1. Ai Nghia station is subject to flooding during the rainy season, therefore, discharge during the rainy season cannot be used with the given rating curve.
2. The Giao Thuy station is influenced by tidal waves (although it is located 38 km upstream from the sea), therefore the water level data of Giao Thuy station cannot be accurately presented by a rating curve.
3. The most important constraint is the dynamic water diversion. As explained previously, there is no control mechanism which measures the exact amount of water diverted from Vu Gia to

Thu Bon through Quang Hue channel. It is therefore difficult to predict how much water is diverted without long term measurements. This leads to an increased uncertainty in the water level data.

We agree that the model cannot consider hourly tidal effects. Our purpose was to assess drought risk at this point in dependence of upstream climatic, human and catchment related circumstances. We assessed water availability at the daily, monthly, seasonal and yearly timescale for the irrigation system entrance. Therefore, hourly tidal information will not affect the results of this study.

Second, I don't agree the J2000 produced "robust" results without at least seeing a Qsim vs Qobs plot. It is comprehensible that observational data availability is often an issue and, sometimes, we need to appeal to simulated data. However, the authors need to discuss the potential implications of this choice.

Answer: we agree with your suggestion and incorporated a section on uncertainties related to these simulations in the discussion. Below (in the Discussion section) you find a Qsim versus Qobs plot including an uncertainty band.

P9, L25: specify that these "very good agreement" refers to A Vuong reservoir.

Answer: thank you for this hint. We have included the name of A Vuong.

Section 3.3: Again, some information do not belong to Results. From my point of view, only the lines after L17 report results per se.

Answer: thank you for this suggestion. We agree that this part also belongs to the Methods section and shortened and shifted it to methods section.

P10, L26-27: This is the first time it is mentioned that the reconstructed streamflow was compared against observations. This should be explained in Methods.

We agree with you and explained this in the method sections.

P10, L27-28: This is the first time it is mentioned to which period corresponds the reconstructed streamflow (RS). Up to this point, it seemed that the RS was for the early 2010s.

Answer: We agree with the reviewer, we did not explain in the methods section that the models were calibrated against the observed daily streamflow available for the years 1980 to 2013. As the reservoirs were constructed after 2009, we needed to use the "pristine" streamflow to calibrate J2000 and the reservoir impacted streamflow after 2009 to calibrate HEC RESSIM. We have incorporated this explanation to the Methods section.

Discussion

The authors recognize the uncertainties that need to be addressed but provides only a qualitative overview about them. It would be enlightening to know how those uncertainties affect the results. Perhaps less important (or greater) hydrologic changes would be found. These possibilities should at least be mentioned.

Answer: We agree with your comment and we now mentioned this in the discussion. Fink did an uncertainty analysis with the Nong Son data using 1000 model runs. Figure 5 shows the 5 % best simulations in the grey shaded area; the blue line indicates the measurements. The blue line represents the observed discharge and moves within the uncertainty band which is an indicator for a robust modelling. The graph shows the largest uncertainty during high flows and the recession phases.

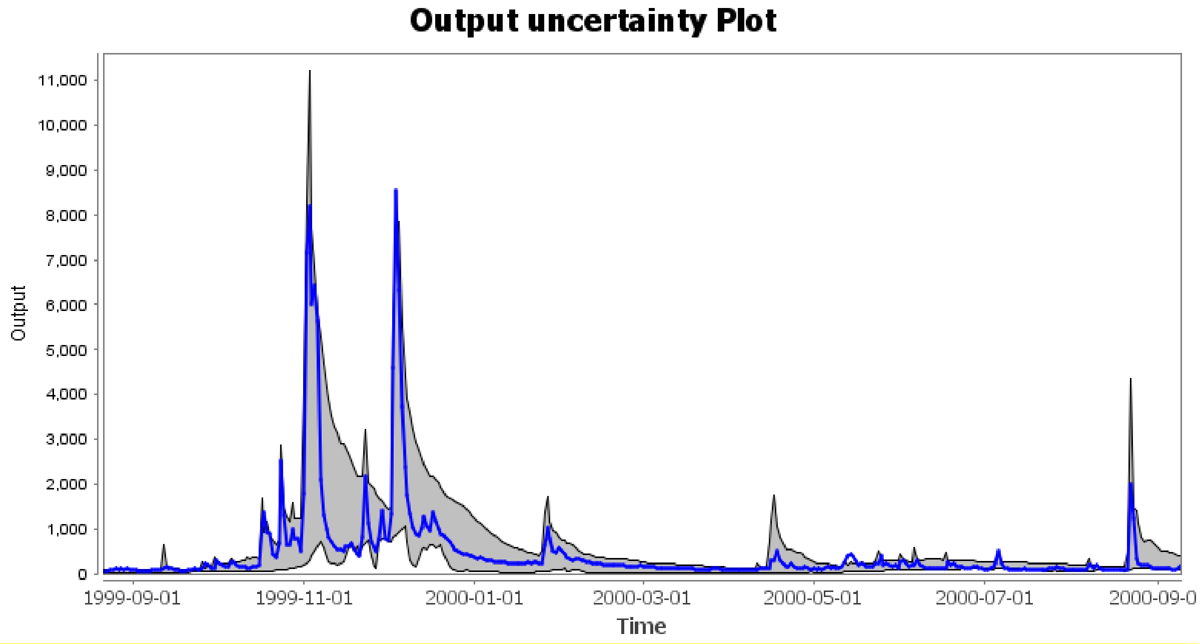

Figure 5: 5 % best simulations (range grey shaded) versus observed discharge (blue line) at Nong Son.

Section 4.2 - The authors claim that the limited rainfall data are related to the difficult access to the basin headwaters where there is no rain gauges. I wonder what could be learned from remotely sensed precipitation. Would such estimates bring more uncertainties than the regionalization methodology adopted by J2000?

Answer: Thank you for this suggestion. Yes indeed in some cases satellite based rainfall estimates do perform better in closing the water balance than observed data. Since we wanted to analyse long term effects we needed longer time series as the station data which were available from 1980 till 2013In the scope of this study we have not considered satellite based rainfall estimation products as they are only available for shorter periods (Zambrano-Bigiarini et al., 2017).

P14, L1: Please cite some examples to support this claim.

…..R2 of the regression line is used to determine if the relation between the rain and the altitude is strong enough to be used for the modelling. A threshold value of R2 of 0.75 is typically used for this decision (Krause 2001, Nepal 2012, Biskop 2016).

P14, L33: This sentence should be in Methods.

Answer: Thanks for the comment, we shifted it to the Methods section.

Conclusion

This section should be more elaborated, showing what was learned and concluded regarding each goal listed in the Introduction. The authors could also consider renaming it to Summary (and Conclusion) as most of it is not really conclusion but a summary of the results.

The authors were too cautious in concluding the main point of this study, which is to provide evidence about the positive/negative impacts of the dams on hydrologic droughts in the study area. This should be explicitly stated here.

Answer: We agree that the conclusion is not containing the key findings in terms of the objectives. We updated the conclusion accordingly in the revised manuscript.

Technical Corrections

There are several problems regarding the citations. For instance, in Page 2, Line 22, it should read “Räsänen et al. (2012) quantified” instead of “(Räsänen et al., 2012) quantified”. Similar issues are found throughout the manuscript:

-P2, L24

We have corrected it as “Räsänen et al. (2012) quantified”

- P2, L32

we have corrected it as “Wang and Hejazi (2011)”

- P3, L20

We have deleted extra “(” in the text

- P4, L10: extra “(”

We have deleted extra “(” in the text

- P4, L13: extra “.”

We have deleted extra “.” in the text

- P12, L26

We have corrected it as “Adam et al. (2007)”

- P13, L3

We have corrected it as “Nauditt et al. (2017)”

- P13, L18

We have corrected it as “are described e.g. in Walker et al. (2003); Refsgaard et al. (2007); Beven and Binley (1992).”

- P13, L30

We have corrected it as “Krause (2002)”

- P14, L14

We have corrected it as “Mateus and Tullos (2016)”

-P15, L4

We have corrected it as “by Nepal et al. (2014)”

References:

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