Response to Reviewer #2

We thank Reviewer # 2 for the profound 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 describing the planned revisions of the manuscript are highlighted in blue normal font, while the reviewer's comments are in *italic font*.

The paper implemented a number of models and assessment method to quantify and highlight the role of reservoirs in the upper part of the change in hydrological drought in downstream of Vu Gia Thu Bon river basin (VGTB), in central Vietnam. By comparing the naturalized and reconstructed data at four discharge stations, a significant consequence of reservoir operation was found in different time scales. Not only duration and frequency, but also the severity of drought was considered with use of threshold approach. This makes the paper completely compatible with the third scoop of HESS, which aim to investigate the influence of human activity to some particular aspects including droughts. Although considering the natural- and impounded-flow has been widely used, but the successful simulation and combination of a rainfall-runoff model and a reservoir modelling based a good foundation for further study facing with the poor data observation.

We thank you so much for the recognition of our work and related effort.

1. ***General Comments:***

The abstract does summarize from context to method and major outcomes of the study. However, it could be more precise if the author either remove or better express the third sentence in the second paragraph without mentioning the local stakeholders.

We agree with you that the formulation is too weak for an abstract. We reformulated the sentence also responding to reviewer # 1:

"We found a stronger hydrological drought risk for the Vu Gia river supplying water to the City of Da Nang and large irrigation systems especially in the dry season."

The introduction provides a good summarized background of the topic, so that the reader can quickly obtain the wide range of application for this issue. A certain number of former researched are mentioned to strengthen the objectives. However, it would be worth if the author reveals other works in which (fully or partly) implemented the same methodologies. The objectives are clearly stated in line 17 – 26 (page 3) with a main goal and four mini-ones.

We thank you very much for the comment. Although we have not found any publication in the literature which fully follows our methodologies, there are some, which partially do which we have incorporated in the introduction section. We have incorporated the work of López-Moreno et al., (2014); Wagner et al. (20017) and Wada et al. (2013).

The study area is fully characterized in part 1.1 to help the reader, who are not familiar with tropical climate, catch the major identities. The status of observation data, hydropower plants

and reservoirs are described in section 2.1, and are very essential to understand the circumstances in VGTB. Besides the spelling mistakes (see specific comments section), a redundancy of information is found in two parts. The author may wish to combine 1.1 and 2.1 (as suggested above) to avoid double explanation about hydrological gauges.

Thank you for this constructive suggestion to shorten and merge the two sections. We agreed that the repeating information is unnecessary. As per suggestion, we have merged section 1.1 under Section 2.1 and shortened the text. We furthermore included a method section as separate section 3.

Moreover, a few points need better coherence, for example: The author offers no explanation of why he chooses data set for his calculation in the period 1980–2013, whilst the discharge data available since 1976 (page 4, line 23).

Please apologize for not making this clear. The temperature data, which is needed for J2000 calibration, does start in 1980. Therefore, we could not use earlier data from other variables. We included an explanation in section 2.2.

Quang Hue channel (page 5, line 15) actually diverts water from Vu Gia to Thu Bon in flood season only, thus, the author could obviously avoid this connection by explaining that this work considers the drought season, rather than assume that "Ai Nghia locate upstream of the diversion of the Quang Hue…" (line 16).

Generally, the Quang Hue Channel in VGTB has diverted water from Vu Gia to Thu Bon all over the years, but the diversion varies between the seasons. For example, during the summer or dry season, sometimes it diverts 20% of its water while in the flood season this amount increases significantly. Please see the reference document (ICEM, 2008: PP 105). Furthermore, in the supplementary document (S1), we have included the diversion rate. Therefore, we have chosen a proxy location of Ai Nghia, to calculate the impact. In the revised text version, we will explain this diversion in detail to avoid misunderstanding.

The definition of "Flood season" and "Dry season" mentioned in page 6 (line 4) may need a source. Otherwise, the current operation rule in VGTB defines them differently (please refer Decision 1537/QD-TTg released on 07/09/2015, Decision of Inter-reservoir operation rule in VGTB).

Thanks for the suggestion. We have incorporated the references from the technical documents for Dak Mi 4, A Vuong hydropower operations rules. (MOIT, 2011 & 2012)

Table 1 mentioned in this part is expected to use the up-to-date statistic. Since they were listed in 2008, the year of operation is not matched completely. Dak Mi4, for example, is said to start operating in 2011, but the actual activation was in 2012 which also mentioned in the results part and figure 5 later on.

Thanks for the remark about the table. We have corrected and updated the table.

The Dak Mi 4B actually does not play important role in this work. It is not mentioned in the body of the paper, except in page 5, line 21. The author may wish to explain why it disappears in the paper, because Dak Mi 4 reflects to both Dak Mi 4A&B or only Dak Mi 4A.

Dak Mi 4B is a runoff hydropower plant, i.e., it does not have any storage functionality but uses the turbine discharge water of Dak Mi 4A to generate energy and release to Thu Bon river. Hence, we have not accounted this for model evaluation. However, as for the other hydropower reservoirs, we considered its operation in the integrated model. We will explain this in the revised manuscript.

The method part spreads in almost two pages which give general description about JAMS/J2000 HRU, HEC-ResSim, combined modeling and Hydrological Drought Assessment.

We thank Reviewer for the encouraging feedback.

Besides the suggestion for re-order the sub-sections (see major comments), this part could be a bit improved if:

The performance of efficiency statistics for the J2000 is mentioned here and also provide the "significance level" if possible, rather than explain them in the result part (page 9, line 2–7). As a reader, I may question how is the goodness of E2, R2…. which are shown in Table 2 and 3?

Thank you for your constructive comment. We agree to bring this into the method section 3.1. The efficiency statistics have been incorporated in the text and the evaluation scale was explained.

the sub-section 2.2.4 is shortened and the reason of choosing tc = 3 days, or zc = 10 % is given. Since they are not presented in the result part, a question of whether the equations and its components are really needed to write in details?

Thank you for the suggestion. We agree with the reviewer that the sub-section 2.2.4 should be shortened and that the equations need to be deleted. We intend to shorten the text by e.g., deleting the equations and associate extra text to explain this and considering the text by addressing key points how the threshold method is applied here. In the revised text we have explained why we have chosen different values for selecting the threshold level as well as the pooling process and minimum days of drought as well.

D-3: the definition of "hydrological year" (page 8, line 7) may be required to make the reader not confuse with the one "water year" which start at beginning of flood season. In line 11–12, it is defined as "the starting of the wet season", but in the line 11–12 (page 4), the rainy season last from September to December. The author may wish to either better distinguish them or unify one term (if they are same)

We want to thank Reviewer # 2 for these valuable comments. First, the definition "hydrological year" has been clarified in the text and we apologize for the mistake in the original manuscript. We defined that the hydrological year starts when the streams, channels or rivers are receiving water after the dry season. In VGTB, August can be referred to as the starting month, because from this month on the flow is starting to increase from its preceding month (Fig. 2). Therefore, we have defined beginning of August is the start of our hydrological year. Secondly, when considering the seasonality, September to end of December is considered as flood season and

the dry season lasts from January to August. This argument has been addressed and included in the revised manuscript .

If the using of data set in each model is explained in this part, rather than in results. (also done in line) and the changes were made.

Thanks for the suggestion. We understand that the using of the data set in each model should be explained in the method part. The proposed changes have been implemented in the revised manuscript

The results are in an appropriate presenting, which follow sequentially the methodologies. The good point of this part is the way to deal with the data shortage, which is very common in this catchment, and they way to have long-term impact of reservoirs. I think this is very good approach. The amount of result is sufficient to the interpretation as well as compatible to the given objective. However, some sentences in this part are seen that should belong to the method- or discussion section. For instance, the explanation of how using data for model or the number of reservoirs in simulation may be better located in methodology, or the line 28–33 (page 9) should belong to discussion, and so on. There are few comments for this part as below:

Thanks for these remarks and the appreciation. We fully agree with the modification proposed by the Reviewer regarding reorientation of the results into the method or discussion section. The uses of data in the model have shifted into the methodology section, for example, we have removed old section 3.1 J2000 Hydrological model calibration to simulate reservoir inflow and naturalized discharge and merged this into method section (new) 3.1. Jams/J200 HRU based Rainfall- Runoff model

The author used data for J2000 HRU is from 1996–2005 to obtain the parameters but do not explain why that period but no former or later one.

Thank you for this comment and we apologize that our explanations have not been clear. We have chosen this time frame because we used the observed streamflow before hydropower came into operation. In section 3.1, we changed the sentence: “The J2000 model was calibrated and validated for the gauging station Nong Son for the period of 1996-2005which was an undisturbed period before the reservoirs were constructed in 2009.

The Reservoir Modelling is taken for four out of eight reservoirs, but results of Song Con 2 is missing in this part, although it is shown in Fig.5

We appreciate the Reviewer comment on Song Con 2 hydropower. This has now been addressed adequately and was adopted into the revised manuscript

E-3: The value of E2, R^2… in Table 3 may need further explanation in terms of calculation or comparison.

Thanks for the suggestion. We have included the explanation in the results section of the hydropower modelling

The paper has a very long and detail discussion with three main questions, from the applicability of the off-line coupling model to the potential uncertainties it may occur. Two limitations are discussed in this section, that makes the paper have a comprehensive view. However, it seems

to me that the section 4.2 and 4.3 are more related to the technical issue, about the appropriateness of this linkage to the same issue, rather than the understanding of changes quantified. Since the title and the objective stress on quantifying human impacts on hydrological drought, I expect this will be the major part of discussion. The current argument would be helpful in a paper, which focus more on the linkage. Besides, no figure or table was mentioned in the discussion part, this would raise the question to the reader that how related the results and the aim of paper are. As pointed out above, there is some writing in results presenting discussion, thus, I think the author may wish to restructure them to make the discussion section more relevant to the objective. For example, Figure 7, 8, 9 contains the most important results to the given goal, thus, they should be discussed in this part. In addition, I would suggest to reduce section 4.2 and 4.3 if the paper is required to be shortened.

Thank you for this valuable suggestion. We agree that there is a lack of discussion about the key points of the paper. We therefore have changed it by including a new section 4.1 on "Quantifying human impacts" discussing the key results with reference to the figures 6 to 9. In addition to this, we have included Table 4, "Impact of human alterations on drought intensity and changes of flow in the VGTB for the period from 1980- 2013 on an annual and seasonal scale".

		Nong Son	Giao Thuy	Thanh My	Ai Nghia
a) Drought duration (%)		-17.17	-30.43	37.08	27.20
b) Changes of flow (%)					
	Ann	19.46	10.09	-37.82	-17.41
	Dry	43.3	27.23	-44.67	-7.91
	Wet	10.84	3.61	-35.03	-21.10
c) Changes of flow (in m^3s^{-1})					
	Ann	51.52	38.32	-51.66	-52.14
	Dry	45.65	42.51	-26.43	-9.97
	Wet	63.25	29.93	-102.12	-136.47

Table. 4. Impact of human alterations on drought intensity and changes of flow in the VGTB for the period 1980-2013 on an annual and seasonal scale. a) Drought duration is calculated based on percentage changes of the number of drought days from naturalized condition to reconstructed condition (Fig 9). b) Changes of flow (%), are calculated based on the percentage changes of the mean flow between the Naturalized and Reconstructed streamflow for the corresponding time frame. c) The changes of flow are calculated based on mean differences of reconstructed streamflow from the naturalized mean flow. The positive value indicates increasing flow or drought intensity in relation to the naturalized condition.

As of Reviewer # 1, questions about the applicability of the section 4.2 and 4.3 in this research, therefore we have agreed to shorten this.

The first two sentences of the conclusion are more likely suitable for introduction rather than in conclusion. The first paragraph re-shows the methods and they are quite general, thus, it might be redundant in my view. In this step, the author may wish to relate the methods and the principal findings to help the reader have the substantial closure. I do not think that mentioning to "the reports from local stakeholders" is needed in this section, it could be better to relate to the

discussion. The uncertainties expressed here in five lines making the conclusion less concise. The last paragraph shows clearly outcomes of this paper.

We agree that repeated introductory sentences are redundant in the conclusion. We tried to follow the general guidelines how to write a conclusion by summarizing the key issues of the paper.

We now shortened these introductory sentences and also the ones referring to a potential uncertainty analysis. The updated conclusion will be included in the revised manuscript.

The literature cited is relevant to the study. I suggest to unify the order of team papers chronologically before alphabetically as guided by HESS. Furthermore, the author could also reduce the references list by choosing the ones that used for the discussion later on.

Thanks for the suggestions, we will follow the reference guideline of HESS. Please allow us to keep the references used for the introduction as we would like to deliver a general state of the art on how human impacts on discharge can be quantified in the scope of this paper.

2. Major Comments-

Regarding to the structure: I recommend reordering a few parts. In detail, the section 1 (introduction) had sub-section 1.1, but the other 1.2 could not be found. Furthermore, since the introduction is expected to provide the literature and objectives only, the author may wish to group sub-section 1.1 and 2.1 in section 2. The methodology could either combine with the data or be a separated section. In case, the author wish to keep them as ordered, the sub-section 1.1 could join as a part of section 2.1. The results section is well presented the introduced methods consecutively, except sub-section 3.3 and 2.1.1. The author may wish to switch part 2.1.1 for 2.1.3 to make the reader easier to follow the next section. I also suggest to re-locate some parts in results (as presented above) to help the reader find easier to follow.

We thank Reviewer #2 for the constructive feedback. We have taken them into consideration and agree to combine the sub-section 1.1 and 2.1 in section 2. Based on your suggestions, we have reorganized the paper as follows: 1. Introduction, 2. Data and Study Area, 3. Methods, 4. Results, 5. Discussion and 6. Conclusion. We have further considered to relocate some of the results in the discussion section.

Because the author mentions in both the title and the objective that to quantify the human impacts on hydrological drought using a combined modeling approach, I expected that the impact quantified and off-line coupling are both discussed, and the former one is likely the major theme. However, in the current paper, little mention of this impact (quantity and reason) is made in the discussion. I recommend strengthening the discussion by linking to the results (figures and tables) and making it more relevant to the objective.

Thank you for this valuable suggestion. As previously mentioned, we have updated the discussion by including a new section 4.1 on “Quantifying human impacts” discussing the key results with reference to the figures 6 - 9. In addition to this, we have included Table. 4, “Impact of human alterations on drought intensity and changes of flow in the VGTB for the period from 1980 - 2013 on an annual and seasonal scale”.

I recommend shortening the section 1.1, 2.1, 4.2 and 4.3 as explained above, to make the paper more concise.

As explained earlier, we have merged section 1.1 and 2.1 under section 2, and 4.2 and 4.3 is shortened and merged into one section

3. *Specific comments*

The paper is written in a good expression of English. I have no objection about this issue. However, there are still some minor remarks given:

1. *Page 2, line 10 and 11: the double hyphens need to make sure as being necessary.*

We have changed this accordingly.

2. *Page 2, line 23, a comma is missing after the blanket*

We have accepted your comment and changes were made accordingly.

3. *Page 2, line 29: "runoff" not "run-off "*

Ans- Thanks for the comment, change was made accordingly.

4. *Page 2, line 33: Wang and Hejazi (2011) not (Wang and Hejazi, 2011)*

Ans- Thanks for the comment, change was made accordingly.

5. *Page 6, line 16: a double space found between "model" and "was"; line 28: "it is" not "It is"*

Ans- Thanks for the comment, changes were made accordingly.

6. *Page 9, line 1: data were not datawere*

Ans- Thanks for the comment, we have changed this accordingly.

7. *Page 10, line 18: E2 = 0.74 or logE2 = 0.74*

Ans- Thanks for the remarks, It will be loge2 = 0.74, and the correction was made.

8. *Page 11, line 15: Thanh My not Ai Nghia*

Ans- Thanks for the comment, we have corrected it as Thanh My.

9. *Page 11, line 17: Fig. 7b not Fig. 7B*

Ans- Thanks for the comment, we have changed it as Fig. 7b.

10. *Page 26, figure 2: Giao Thuy not Giao Thu*

Ans- Thanks for the comment, We have changed as Giao Thuy.

11. *The format should be unified. For example, many paragraphs in page 1, 13, 14, 15 and 16 have left alignment.*

Ans- We have corrected the formatting for the mentioned pages.

The paper basically follows the manuscript composition guideline (given by HESS) in terms of mathematical requirements. There are however some typical errors found in the manuscript:

a) *Coordinates: in page 4, line 1, coordinates of VGTB ("6o 55'–14o 55' N" not "6^{o} 55'–14 ^{o}55' N").*

Ans: Thanks for the comments, we have corrected this as suggested.

b) *page 4 and the rest of the paper: spaces must be included between number and unit, e.g. 47 % not 47%.*

Ans: Thanks for the comment, we have corrected all the number and units, as suggested in the text.

c) *page 4, line 3: km2 not km^{2}*

Ans- Thanks for the comment. We have changed it accordingly.

d) *page 4, line 9: tons-ha or tons ha^{-1}*

Ans- Thanks for the comment. We have changed it accordingly.

e) *Numbers: neither dots nor commas are permitted as group separators, except that the number start with the ten-thousand digit (given by HESS). Thus, 2598 not 2,598 (page 4, line 6) and so on.*

Ans: Thanks for the comment, we have corrected all the numbers as suggested.

f) *Using of hyphens (-) and en dashes (–) are quite often confused. In most cases in this paper, hyphen is used as en dash and it should be better distinguished. For example: 65-80% (page 4, line 13) should be written as 65–80 %, and so on. Please refer guideline (given by HESS) to make them correct.*

Ans: Thanks for the remarks and observations. We have changed hyphens (-) to en dashes (-) as suggested.

Figure and Tables:

g) *Figure 7 presents the percentages of changes but did not explain how this value is calculated*

Ans- Thanks for the comment. We agreed that it needs a bit more explanation in addition what we have explained in the text. So the correction we have made as follows:

The percentage of changes of flow is calculated based on the percentage changes of the mean flow between the Naturalized and Reconstructed streamflow for the corresponding time frame. We have incorporated this in to the Figure 7a.

h) *Figure 9: Giao Thuy not Giao Thyu*

Ans- Thanks for the correction. We have changed this as Giao Thuy

Abbreviation of:

a) *figures should be unified: e.g. Figure 5 (page 10, line 22) or Fig. 5 (page 9, line 24, 28)*

Ans: Thanks for the suggestions. We have changed it as Fig. 5 and the guideline has followed for the rest of the manuscript.

b) *letter should be first introduced. For example, MAM and JJA (page 12, line 3) are understood that March-April-May or June-July-August, but it could make confusing to the reader when first read them.*

Ans: Thanks for the remarks. We have introduced to the abbreviated letters in the revised manuscript.

Overall, I think the off-line coupling results are considered that novel enough for publication in HESS scope. This is extremely helpful in terms of transferability to the similar river basin dealing with data shortage or poor observation network as Vietnam. However, since the linkage approach is getting more common nowadays, the paper may expect to prove some more related studies to make sure that this work more original. By the stage of publication, all the comments on this manuscript obviously need to make clear.

We thank you so much for the recognition of our work. To address your suggestions, we have included some more recent literature, showing the coupling approach to evaluate the reservoir impact on the streamflow changes. However, there is no literature which exactly follows our approach. Below is the list of the new references that we incorporated into the revised manuscript.

López-Moreno, J. I., Vicente-Serrano, S. M., Beguería, S., García-Ruiz, J. M., Portela, M. M., and Almeida, A. B.: Dam effects on droughts magnitude and duration in a transboundary basin: The Lower River Tagus, Spain and Portugal, Water Resour. Res., 45, 6, doi:10.1029/2008WR007198, 2009.

Wagner, T., Themeßl, M., Schüppel, A., Gobiet, A., Stigler, H., and Birk, S.: Impacts of climate change on stream flow and hydro power generation in the Alpine region, Environ Earth Sci, 76, 33, doi:10.1007/s12665-016-6318-6, 2017.

Wada, Y., van Beek, L. P. H., Wanders, N., and Bierkens, M. F. P.: Human water consumption intensifies hydrological drought worldwide, Environ. Res. Lett., 8, 34036, doi:10.1088/1748-9326/8/3/034036, 2013.

Other references

ICEM: Strategic Environmental Assessment of the Quang Nam Province Hydropower Plan for the Vu Gia-Thu Bon River Basin, Prepared for the ADB, MONRE, MOITT & EVN, Hanoi, Viet Nam, 205 pp., 2008.

MOIT: Decision Number 6801/QD-BCT, Decision on Dak Mi 4 Reservoir Operation, Ministry of Investment and Trade, Socialist Republic of Vietnam, Hanoi, Viet Nam, 2011

MOIT: Decision Number 1997/QD-BCT, Decision on A Vuong Reservoir Operation, Ministry of Investment and Trade, Socialist Republic of Vietnam, Hanoi, Viet Nam, 2012