

Review of the paper

“Quantifying human impacts on hydrological drought using a combined modelling approach in a tropical river basin in Central Vietnam”

by Firoz et al.

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.

1. General comments

The paper is quite standard structured with five main parts excluding references and shows a good smooth of writing, which helps the reader in capturing the main ideas in both title and written content. However, there are still a few concerns as presented below.

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.

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.

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. 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).
- 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).
- The definition of “*Flood season*” and “*Dry season*” mentioned in page 6 (line 4) may need a sources. 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).
- 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.

- 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.

The method part spreads in almost two pages which give general description about JAMS/J2000 HRU, HEC-ResSim, combined modeling and Hydrological Drought Assessment. 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 E_2 , R^2 ... which are shown in Table 2 and 3?
- the sub-section 2.2.4 is shortened and the reason of choosing $t_c = 3$ days, or $z_c = 10\%$ 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?
- 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)
- the using of data set in each model is explained in this part, rather than in results.

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:

- 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.
- 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
- The value of E_2 , R^2 ... in Table 3 may need further explanation in terms of calculation or comparison.

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.

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.

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.

2. Major comments

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

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:

- Page 2, line 10 and 11: the double hyphens need to make sure as being necessary.
- Page 2, line 23, a comma is missing after the blanket
- Page 2, line 29: “runoff” not “run-off”
- Page 2, line 33: Wang and Hejazi (2011) not (Wang and Hejazi, 2011)
- Page 6, line 16: a double space found between “model” and “was”; line 28: “it is” not “It is”
- Page 9, line 1: data were not datawere
- Page 10, line 18: $E2 = 0.74$ or $\log E2 = 0.74$
- Page 11, line 15: Thanh My not Ai Nghia
- Page 11, line 17: Fig. 7b not Fig. 7B
- Page 26, figure 2: Giao Thuy not Giao Thu
- The format should be unified. For example, many paragraphs in page 1, 13, 14, 15 and 16 have left alignment.

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:

- Coordinates: in page 4, line 1, coordinates of VGTB (“6° 55’–14° 55’ N” not “6o 55’–14 o55’ N”).
- Symbols and equations:
 - page 4 and the rest of the paper: spaces must be included between number and unit, e.g. 47 % not 47%.
 - page 4, line 3: km^2 not $\text{km}2$
 - page 4, line 9: tons-ha or tons ha^{-1}
- 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.
- 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.
- Figures and tables:
 - Figure 7 presents the percentages of changes but did not explain how this value is calculated
 - Figure 9: Giao Thuy not Giao Thu

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

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.