Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-615-AC4, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Economic impacts of drought risks for water utilities through Severity-Duration-Frequency framework under climate change scenarios" *by* Diego A. Guzmám et al.

Diego A. Guzmám et al.

daga2040@usp.br

Received and published: 14 June 2018

Dear editor and reviewers,

We thank you very much for the valuable comments and suggestions about our manuscript "Economic impacts of drought risks for water utilities through Severity-Duration-Frequency framework under climate change scenarios" in HESSD (Hydrology and Earth System Science Discussion). We performed a careful revision to make all suggested changes and we believe the manuscript is now much improved. Please

check if you are happy with the new version and let us know if you have any further questions or additional suggestions. You will find in blue the responses to each comment below. All changes to comply with the reviewers' suggestions were highlighted in yellow in the manuscript. Yours sincerely,

Diego Alejandro Guzman Arias Corresponding author

Anonymous Referee #2 (Received and published: 17 May 2018)

As a reader I struggled to extract what was previously known and available and what is the main new contribution of the study and how the knowledge gained can inform further progress. I will provide some details below. In its current form, the pure case study descriptions are too long and the transferable part too small.

The manuscript will also require a number of clarifications and improvements to structure. These may include:

The comments of the reviewer from number 1 to 6 and 8, emphasize the structure and clarifications that were accepted by us in the new manuscript.

1. Clarifying the actual academic objectives and/or hypotheses.

2. Cutting and cleaning of a lot of unnecessary local and specific information that are not relevant or at least take attention away from the envisioned 'academic exercise' (such as e.g. lines 134-137, but many more also)

3. A clear separation of a generic concise methods section from the prior assumptions and also from the specific results of this study. The description of study area and methods is nine pages, followed by only three and a half pages of results and conclusions. A discussion putting the results into the wider context is missing. This imbalance illustrates clearly that the commonly accepted structure of a science paper is not followed.

4. In the current manuscript, most figures are already referred to in the methods sec-

tion, which then contains already many details on results and is consequently rather confusing. To the reader it is unclear, which exactly are the new results reported here vs material available prior to this contribution.

5. Line 91 - why 'are affected' when referring to past droughts? Otherwise this is water scarcity and has perhaps not to do with drought as a temporary climate phenomenon.

6. This is also something I struggled with - clear definitions would be very helpful to the reader.

7. The threshold appears not close to usually employed drought thresholds which often represent the 10th or 20th percentile of the empirical distribution function of river flow or other hydrological flux or state variables. Where does this demand threshold rank in the flow duration curve? And can the deficit then really be termed drought. Some of the figures indicate a deficit every year. Commonly this may not be considered a drought (as an annual and rare event). A thorough discussion and explanation comparing to the literature is needed on this aspect.

We appreciate the comments. Regarding the threshold, some authors do not restrict the use to indexes or variables of the state of the flow, for example some value of the duration curve. Hisdal et al., 2004 and JH Sung and Chung, 2014, define the threshold from "thresholds of desired performance", in our case the threshold is pre-established by the characteristics of water withdrawal for the SPMR, condition on which you want to evaluate the system. On the other hand, during the introduction the clarification was made about the characteristics of the hydrological drought and what leads to the water deficit, our main focus. This assumption of annual deficits, is clearly an assumption for the implementation of the method, however, during the stage of the calculation of the impact, minor droughts are not considered.

8. The WEAP modelling is presented as part of the study, but there is not enough information on model details to convince the reader of a carefully carried out modelling. It also lacks an assessment of uncertainty. In a climate change application, the most im-

portant information is whether the signal of change exceeds that of current uncertainty. Some of the modelling description uses unusual terminology and phrasing. I suggest to have this proofread by a modelling expert.

Specific comments:

75 - Sentence makes no sense grammatically Modified text within the body of the abstract (p1, l13 to l35) and in other sections of the text through the grammatical revision.

156 - Text unassigned to a header and structurally unclear The text was complemented and organized according to the request of the reviewer.

91 - What are 'establishments'? The term was revised and replaced in lines 82 to 85 of the new text

396-397 unnecessary The text was complemented and organized according to the request of the reviewer.

Table 1 - Those are not 'variables' - wrong terminology Table 2 - should it not be 30, 90 and 90,180? The table was modified.

Table 3 - First two columns don't make sense in the table as they don't vary The table was introduced in the new Figure 6 and the columns were eliminated.

Tables 4-6 would be much nicer as graphs - this illustrate the point: absolute numbers don't matter to an international readership that will not be interested in the specific case, but in sensitivities, systematic differences, trends etc... These tables were changed by Figures 9, 10 and 11 in the manuscript.

Figure 1 - are there numbers in the sub-watersheds? Resolution is insufficient – either show clearly or remove from map Figure 1 was edited and new descriptors were added for better understanding.

Figure 2 - Why show the seasonality if what matters is the demand vs the deficit over the year? We did not find this description in Figure 2 of what was commented by the

reviewer.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-615/hess-2017-615-AC4-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-615, 2017.