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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Water restrictions under climate change: a Rhone-Mediterranean perspective combining 'bottom up' and 'top-down' approaches" *by* Eric Sauquet et al.

Anonymous Referee #1

Received and published: 26 October 2018

The paper "Water restrictions under climate change: a Rhone-Mediterranean perspective combining 'bottom-up' and 'topdown' approaches" presents a study that uses decision scaling (Borwn et al., 2012) to evaluate future water restrictions pattern in southeast France. I think the topic fit the scope of this journal, but I have several major concerns on this manuscript.

First, the novelty of the paper is questionable. Applying a bottom-up approach such as decision scaling to evaluate climate change impact uncertainty is not a new topic in the field. Although authors might argue that presenting the climate response surface in WR (not streamflow) is relatively new, I do not see any additional information regarding

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policy inform that can be generated from this result. Some visualization techniques used in this paper could be attractive (such as using color to represent mean value and size to represent the s.d.) but I failed to understand the overall scientific contribution of this paper.

Second, the modeling framework is extremely unclear. Authors use Section 4 to explain their method but they spent a lot of space to explain decision scaling which is other people's work. They briefly mention the rainfall-runoff model they used but no details about the actual water restriction level modeling framework (Section 4.3). They explain their concept of computing WR in fair details (which is helpful) but I still do not understand how they build the WRL model. What are the input and output of this model? What parameters can be calibrated in this model? How to authors link this model with the rainfall-runoff model? Information about these is partly provided in Section 4.3 but hard to follow from a reader's perspective.

Third, lack of in-depth discussion on the policy implementation. Given that authors use WR in the climate response surface, one can expect that authors should use a lot of space to link their results to drought policy implementation or some information about the adaptation action. However, only a short discussion of WR has been provided at Section 5.5. Given that this is not a methodological paper, these in-depth discussions become the critical point to prove that this paper is worth to be published because readers around the world can learn from this study and apply it to their own drought management policy.

Finally, the structure of the manuscript and English is extremely difficult for readers to follow. The general outline of the paper follows a typical modeling paper while authors introduce their study area and data than their model. However, as I mentioned above, the modeling framework especially for the "Water restriction level modeling framework" is not clear at all. Also, there are general equations list in the results section (Section 5) and irrelevant results (Line 432-474) presented in the result section. There are A LOT of grammar errors and typos that make the manuscript hard to read. This is surprising

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that one of the co-authors is from the UK.

I do not think this draft reach the standard of HESS in its current form. I have several detailed comments below.

Line 34 - What do you mean by “changes” Climate or human activities? Line 35 - What kind of drought? Climatic? Hydrologic? Or economic? Line 86 and 88 - You are arguing with yourself. In Line 86 you said water is abundant globally but Line 88 you said water resources are under high stress. Line 90 - Why 43% is high proportion? It is less than the half. Line 96 - You never explain what is “Drought management plan?” Line 111 and 115 - You are arguing with yourself agian. If water restriction decisions are frequent (Line 115), why these catchments are with minor human influence? Water restriction decisions are human influence. Line 173 to 174 - Will the selection of index affects all of your results? You should discuss this in the discussion section. Line 186 - Why cross a threshold is unsustainable? How do you know it won’t come back? Quantify sustainability is a difficult challenge and if you don’t know what it is, you should not use the word. Otherwise, you should define sustainability. Line 190 - What do you mean by intersection? Line 215 - I don’t see any calculation related to irrigation water use in your 4.3? How you do this? Line 254 - Don’t understand what you mean. Line 262 - Why not use the worst WRL as indicators? And also why not just use daily time step as your rainfall-runoff model? Why change it to 10-day? Line 263 to 265 - English is so weird. Line 302 to 303 - I do not understand what is your point here. If you know this, then why don’t you model that? This means you understand that just a hydrologic model is not enough to do this type of modeling but you still do it and write a paper about it. This just implies that your model is not only WRONG (as all models are) but also not very USEFUL. Line 357 - What drivers? I thought in climate change studies, T and P changes are drivers. Line 358 to 359 - I don’t understand your English. Line 402 - Typo. Line 432 to 474 - I do not understand why you have these results here which are not related to WR. Line 788 - There is no need for Figure 2. Line 797 - The explanation of Figure 5 is unclear. This result in my second major comment regarding

the modeling framework. A better explanation needed. Line 801 - The results are weird here. If your GR67 model is good according to your NSE and Kling–Gupta efficiency, why GR67 and HYDRO show different results in a lot of place in this figure? Does not make sense. Line 819 - If “2” and “3” are similar, why you need to separate them into two categories? Line 822 - The figure at the lower-right corner is unreadable.

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