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Discussions

Interactive comment on "Methodology based on modelling processes and the characterisation of natural flows for risk assessment and water management under the influence of climate change" *by* Sara Suárez-Almiñana et al.

Anonymous Referee #1

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This manuscript introduces a methodology to assess the effect of climate change on water resources systems by using a model chain connecting climate model results, a semi-distributed rainfall-runoff model and a water management simulation model. While I think the themes dealt with in this work are relevant and interesting for the scientific community, I am afraid the manuscript requires a severe degree of revision before it can be considered for publication in HESS. To be accepted for publication in any journal, the first thing the authors should do is a thorough language and structure revision. At present, the text is clumsy and difficult to read. In addition, I found some

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relevant information out of place, e.g., the mention to the improvement introduced in the study in page 4, which should appear in the conclusions section too (I will come back to the added value of the study later), or the description of the drought risk indicator calculation in section 4.3 that should be in methods (I will come back to this too). Regarding the content of the manuscript, my first major concern is its absolute lack of focus. Section 1 introduces a series of concepts mostly disconnected from each other and somehow irrelevant for the rest of the manuscript. Half of the introduction talks about climate services and how to deal with the data they provide, yet no further mention is made to them later in the text either in the discussion of the conclusions section. The last 20 lines of the introduction more or less describe the problem the authors want to study and one could discern what the research objective is. However, it is not the task of the reader to guess the objectives of research work. The authors must explicit what they want to achieve with their work and communicate this to the reader in an efficient and straightforward way. I would like to add that developing a methodology is not an objective in itself but rather a tool to pursue the answer to a research question. Concluding that the methodology is general enough to be applied in other case studies would be an acceptable conclusion though. I do not have major concerns about the methodological approach of the research: climate→hydrology→management, but there is an evident need to improve the clarity of exposition in terms of sections arrangement and description of methods. With regard to sections arrangement, I have the impression that sections 2 and 3 are not separated appropriately. I would suggest that the case study was presented first. The Jucar River Basin is an extensively studied catchment in literature, especially from this research group. Still, I think the system deserves having its own section. Afterward, the whole section 2 and subsections 3.1 through to 3.3 should be merged in a single "materials and methods" section. Coming now to the description of methods, I think the part between lines 89 through to 115 requires a better explanation, including justification of figures 1 and 2. Line 89 reads: "In this section, a distinction between the current assessment in the management of water resources and the analysis of risks was made, despite of being intimately related". Disregarding the quality of this sentence, there is nothing in section 2 that actually deals with that distinction unless the reader is imaginative to say the least. My assumption is that the authors call one thing (current way) to management made on the basis of current/past climate analysis and they call another thing (risk analysis way?) to the assessment of management under future climate, and they argue that the two approaches should be integrated. I do not understand the reasons for such extravagant differentiation of a traditional present versus future analysis that does not add anything conceptually new to the current state-of-the-art. Now is when things get spicy. The authors mention that the novelty introduced in this research "lies in the characterization of future natural inflows and the combination of the management and risk assessments". But, what is new about determining streamflow under future climate conditions and compare it against present conditions? I want to think this must be a writing error from the authors as I do see more value to the results they present further than just comparing two situations. Continuing down the line, I think section 3.3 is the core description of the methodology. I suggest it appears earlier in the text and that it uses a more generalized language only mentioning that modules from the Aquatool software will be used (substitute Aquatool modules' names by generic names, e.g., EVALHIDÂň→rainfall-runoff model, MASHWIN→stochastic streamflow series generator, SIMGES→water management simulation model). Next, I suggest sections 2.1 and 3.1 are merged into a single 'current and future climate (or better name)' section. Section 3.2 could be a subsection of the new merged section. By the way, the words precipitation and temperature do not appear so often in the text and are not that long to require using an acronym. I suggest you revise this. Finally, all the models that are actually part of the Aquatool software would be better together under an 'Aquatool modeling package (or better name)' section. I would like the authors to clarify their position with regard to bias correction. By the way, figure 3 is an absolute mess, it should be revised for clarity. The authors claim in the discussion that "working with the raw data would lead to unfavorable results for the future since the underestimation of flows in the headwaters is notable, this fact may also lead to alarming conclusions about the future hydrology in this basin". But, in figure 6, we see that bias correction actually changes one problem for another, especially at the resource generating catchments of Alarcon and Contreras. While the uncorrected data fits visually well precipitation between March and September (dry months), underestimating it during the wet months in the winter, the bias-corrected data overestimates spring and summer precipitation while still underestimating winter precipitation. This is potentially a problem if the extra amount of water in the summer introduced with bias correction exceeds the winter deficit of the uncorrected data. I think this might be explored. The descriptions in 4.2.1 and 4.2.2 correspond to the methods section. The authors should limit to describe the results in these sections. Moreover, I think the section would benefit from merging figures 7 and 8, although I am not sure whether the first column in figure 8 should be maintained for what I will mention next. Figure 9 shows the mean rates of streamflow change for the three future periods with regard to the reference period. Did the authors check the rate of change between reference and future periods of non-corrected flows from option B? I think this might be revealing. Also, the size difference between graph A and graph B in figure 9 should be revised. Regarding the final step of the methodology, relative to water management simulation, I think the authors should justify better the added value of using stochastic modeling when they already have a reasonable amount of data (several 30 years series from various climate models) to perform the statistics relative to water storage. I think the results from using the water management simulation model should appear before the ones from the stochastic simulation and, in any case, both results should be comparable (e.g., calculating the drought indicator, or the exceedance probability in September). The exceedance probability at the beginning of the irrigation season might be a relevant result too. Anyway, the results of this section show how the relevance of the bias correction is dampened through the modeling chain. Considering limitations and additional uncertainty introduced by bias correction highlighted by Ehret et al. (2013) and the results of studies like Muerth et al. (2013) who argue about the utility of bias correction in model chains, I think the authors lost a good opportunity to contribute to the existing debate on the added value of bias correction in the modeling of climate change impacts on water resources. I would not like to finalize my review mentioning that the authors make the wrong use of the term tendency throughout the whole text to my understanding. Mostly because the authors do not show whether their results really follow any trend and whether this is significant.

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