

Interactive comment on “Simulating long-term past changes in the balance between water demand and availability and assessing their main drivers at the river basin management scale” by J. Fabre et al.

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General Comments

Fabre et al. have written a detailed account of the process they used to effectively simulate the past changes in water demand and availability of two basins. The two basins chosen contrasted in key ways: spatial extent, water availabilities and demand, and data available. This enabled the reader to contextualize the considerations taken into account in the model design and the produced modeling estimates. The paper

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provides a useful ‘inside’ look into the importance of designing a model, sorting and using large amounts of data, and the past changes in water availability based on type of demand or change in climate.

In general, the paper was informative but needs to be condensed and a few issues need to be addressed. In the introduction, the authors mention the model’s usefulness for making predictions and as policy tool, but future predictions are not simulated in this paper. More clearly stating the potential use of the model may emphasize its importance. But it needs to be clear that only past measurements were considered at this stage. Unfortunately, the results section appeared a bit jumbled and may need to be restructured. I would suggest merging some of the subsections into points concerning natural streamflow, influenced streamflow, and dam management.

The term ‘water stress’ is mentioned throughout the paper, but it is never clearly defined (decrease in discharge, decrease in stream level, decrease in storage?). A definition for what is meant by ‘water stress’ is needed. I am curious how the simulations presented in this paper compare to other similar modeling studies; a comparison might add validity to the modeling results presented in this paper. A specific point is that units in m vs. hm and to have units consistent throughout the paper (including figures).

The following is a list of specific points for consideration or comment.

- Page 12316, Line 15-17: The sentence structure on feels a bit awkward; consider revising.
- Page 12316, Line 9: The authors’ first mention of water stress; definition should be given here.
- Page 12319, Line 22-23: This sentence needs commas to aid readability (i.e. “..., particularly in the Mediterranean region, ...”)
- Page 12319, Line 25-27: The authors imply the implicit first step in developing basin usage strategies is to give it historical context. While this is an important basis for

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designing policy, there are multiple approaches that could be taken. It needs to be clear this is a possible first step and was used by the authors based on specific reasons.

- Page 12319, Line 3: In this sentence, there is a possible issue with tense agreement (contrasting vs. contrasted).
- Page 12320, Line 4-5: I am unclear of what this statement means, please clarify.
- Page 12321, Line 4-7: This sentence feels a bit clunky; consider rewording.
- Page 12321, Section 2.3: Two different methods were used to calculate ET; it would be nice to have a clear explanation of differences and benefits/consequences to each method. Also, an indication of where it is difficult to compare the two methods or ET estimation based on the assumptions applied to each method.
- Page 12321, Line 11-14: All variables need to be defined; TMOY was missed.
- Page 12322, Line 1: It is unclear whether the temperature values for stated for each basin were yearly averages or min/max values.
- Page 12322, Line 13: A short explanation of what is meant by "...typical Mediterranean regime..." would be helpful.
- Page 12322, Line 23-25: In this sentence, it is unclear whether the increases in precipitation are from the average value or the total precipitation increase over the 28-year timeframe.
- Page 12323, Line 24-26: This sentence needs to be reworded.
- Page 12324, Line 5-10: This sentence is too long; splitting it into two would make it clearer.
- Page 12324, Line 19: Colloquialisms should be avoided (i.e. "nowadays").
- Page 12325, Line 21-23: The authors provide a list of assumptions they made in setting up their model. The third assumption stated seems to be quite broad and I

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think it could be better explained/defined. Perhaps the authors could comment on this and state how much/percent of water is assumed to return to the river.

- Page 12325, Section 3.1.1: It's evident there is an assumption being made about the interconnection between groundwater and surface water. There was no discussion of groundwater in the paper and I was curious if the authors could comment on this.
- Page 12326, Line 4: The authors state: "The spatial distribution... was mapped to correctly determine...". As wonderful as models are, using the term "determine" may give them too much credit. Perhaps estimate would be a better choice.
- Page 12331, Line 22-24: The authors state that "(c)omparing changes in natural and modified streamflow between 1971 and 2009 enabled climate variability to be distinguished from anthropogenic pressure as causes of the decrease in streamflow observed in both basins." This makes it sound quite simple to distinguish the two, I think the interdependence may be more difficult to discern than this statement implies. Perhaps a rewording or a greater explanation is necessary?
- Page 12332, Line 15: The authors specify that the three demands faced shortage and then reiterate that this includes industrial demands. I think "industrial demands" should be deleted.
- Page 12333, Line 20-21: The authors state: "(o)verall, the results were satisfactory except for some downstream sections in which discharge was significantly modified and few calibration data were consequently available on natural streamflow." Perhaps this could be stated earlier and these sub-basins discussed less throughout the paper?
- Page 12333, Line 15: This sentence needs to be reworded; perhaps simulated reservoir levels would work better.
- Page 12335, Line 1: Could the authors choose a better term than "less well" to describe the dam simulation results?
- Page 12337, First paragraph: The authors explain the correlations between anthropo-

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logical impacts and streamflow based on Figure 9. In the Ebro basin, the correlations are not as clear. Could the authors comment on this?

- Page 12337, Line 14-16: This sentences needs to be reworded or the statement should be further explained.
- Page 12337, Line 18: There is redundancy in this sentence; delete "in the Ebro basin".
- Page 12345, Line 12-14: I am curious whether the authors plan to make any changes to the model based on the limitations they outlined in Section 5.2. What would the changes be?
- Page 12353: Aesthetically, Figure 1 might look better if the images were labeled as a) and b) and the descriptions were given below in the figure description.
- Page 12354: I am curious about the use of "priorities" in Figure 2. AWD was highest in both basins and yet it's listed as 3rd priority. Perhaps the authors could explain this?
- Page 12355: In Figure 3, a space is missing between the words "section" and "for".

Overall this paper clearly framed the modeling process utilized and effectiveness/limitations of the modeling water availability and demand in the Ebro and Herault basins. The authors presented an interesting topic, but I am not sure how much new information was presented. I am interested to see the results of the next study (running different potential scenarios) and I wonder if this paper would be better as background information to that paper. Despite my comments, I believe this paper should be published.

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