Answer to comments from C. Steele

N.B.: We responded to each series of comments in separate documents, and provided as supplement the revised manuscript. Since the changes in the manuscript were numerous, we uploaded two versions of it: one with all modifications visible in the revision mode, and the revised version with all proposed modifications accepted, to provide a more readable overlook of the manuscript. Page and line numbers in our responses refer to the document in revision mode.

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 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.

Authors' response:

We would like to thank C. Steele for her interest in our paper and for her detailed review, which helped us to significantly improve our paper.

Specific comments:

Public comment:

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.

Authors' response and changes in the manuscript:

To clarify the fact that only past measurements were considered at this stage, a sentence was added in the last paragraph of the introduction: page 5, lines 6-10: "Before simulating changes in water demand satisfaction under prospective water use and climate scenarios, we need to show that the modeling approach used is able to represent past variations in the satisfaction of water demand, in space and over time."

The value of this model in its potential use for making projections and as a policy tool is also stated in the prospects (p. 26 line 11):

"Despite the limitations mentioned above, the approach described in this paper will enable the identification of the drivers of water stress in basins that are likely to face rapid climatic and anthropogenic changes in the coming decades. Such information will be extremely useful to policy makers when designing water plans for coming decades. The next step of this work, which is currently being conducted in the framework of the GICC-REMedHE project (www.remedhe.org), consists in using

the proposed integrative modeling chain to simulate and compare the impacts of socio-economic trend scenarios and a wide range of possible future climates."

Public comment:

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.

Authors' response:

The results of this study can be summed up in three points:

-first we present the efficiency of the modeling chain, to prove it was able to simulate past changes in influenced streamflow through the simulation of natural streamflow, water demand, dam management and consumptive use

-then we analyze the causes of past changes in influenced streamflow, by presenting the variations in natural streamflow, water demand and consumptive use over the past 40 years

-finally, we analyze water demand satisfaction in the past and the reasons for its variations.

The first part of the results section (4.1: Efficiency of the modeling chain) was indeed structured as suggested, with points on natural streamflow, dam management and influenced streamflow. However we believe it was important to separate the other two parts of the results section. To try to clarify the structure of the results sections, we changed the title of section 4.2 to "Analysis of the causes of past changes in observed streamflow" and the title of section 4.3 to "Analysis of the balance between water demand and availability".

Authors' changes in manuscript:

-The title of section 4.2 was changed from "Changes in water demand and natural streamflow" to "Analysis of the causes of past changes in observed streamflow" (p.18 line 5)

-The title of section 4.3 was changed from "Sustainability of the hydrosystems: balance between water demand and availability" to "Analysis of the balance between water demand and availability" (p. 20 line 5).

Public comment:

-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 of what is meant by 'water stress' is needed.

-Page 12317, line 9: the authors' first mention of water stress; definition should be given here.

Authors' response:

Agreed.

Authors' changes in manuscript:

Our definition of water stress was added in the introduction at page 3, lines 3-4:

"Here water stress is defined as an imbalance between water demand and availability, i.e. when water demand is greater than supply".

Public comment:

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.

Authors' response:

A paragraph was added in the discussion.

Authors' changes in manuscript:

Paragraph added p.22, line 28:

"To our knowledge other studies did not simulate natural and influenced streamflow at the mesoscale and over this long of a period, and did not achieve a historical reconstruction of water demand and the influence of water use on streamflow over such a long past period. Lanini et al. (2004) designed an integrated model of the Herault socio-hydrosystem, with detailed accounts of hydrogeological processes and decisions on water use. However this model was calibrated and validated over a short period and only covers an area of approximately 100 km^2 in the Herault valley. Collet et al. (2013) successfully modeled influenced streamflow with simulations of water demand satisfaction and consumptive use; however the hydrological model was calibrated using observed streamflow data, i.e. influenced streamflow. In the Ebro basin, some studies such as López-Moreno et al. (2014) successfully simulated natural streamflow in snowmelt sub-basins, and demand-driven dam management, although with water demand unvarying form one year to another and therefore an incomplete integration of drivers of water stress. Finally Milano et al. (2013b) proposed a representation of water stress over the whole Ebro basin, accounting for three storage dams and with a simulation natural streamflow. However the natural streamflow data used for calibration of the hydrological model in this case were based on outputs from another model, and the hydrological model did not account for snowmelt's influence on runoff. Moreover water stress was assessed at a monthly time step, and water demand was considered constant over a 20-year period."

Public comment:

A specific point is that units in m vs. hm and to have units consistent throughout the paper (including figures).

Authors' response:

In this paper, units were chosen according to their use. In general, meters (m^3/s) were used for daily or 10-day time step and hm for annual averages of volume. We did convert m^3/s to volumes at each time-step to compare water demand to availability, however we believe it is clearer to present streamflow in m^3/s than in hm^3/10-days.

Public comment:

Page 12316, line 15-17: the sentence structure on feels a bit awkward; consider revising.

Authors' response:

Agreed. Changes were made on p.2.

Authors' changes in manuscript:

"Observed changes in discharge were explained by separating human and hydro-climatic pressures on water resources: respectively 20% and 3% of the decrease in the Ebro and the Herault discharges were linked to human induced changes"
was changed to:
"Observed past variations in discharge were explained by separating anthropogenic and climatic pressures in our simulations: 3% (20%) of the decrease in the Herault (Ebro) discharge were linked to anthropogenic changes." (p. 2 lines 7-9)

Public comment:

Page 12319, line 22-23: This sentence needs commas to aid readability.

Authors' changes in manuscript:

This sentence was deleted in the revised version of the manuscript (as part of our effort to clarify the introduction).

Public comment:

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 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.

Authors' response:

Agreed.

Authors' changes in manuscript:

The sentence "The first step in designing appropriate adaptation strategies for water resources management is developing a modeling approach able to represent pas variations in the satisfaction of water demand, in space and over time" was changed to "Before simulating changes in water demand satisfaction under prospective water use and climate scenarios, we need to show that the modeling approach used is able to represent past variations in demand availability, in space and over time." (p. 5 line 6)
Also the following sentence was added: "Also, giving historical context and explaining the past variations in water demand satisfaction can help understand the vulnerability of hydrosystems to climatic and anthropogenic changes and design appropriate adaptation strategies." (p. 5 lines 10-12).

Public comment:

Page 12319, line 3: In this sentence, there is a possible issue with tense agreement.

Authors' changes in manuscript:

We have replaced 'contrasted' by 'contrasting' (p. 5 line 16).

Public comment:

Page 12320, line 4-5: I am unclear of what this statement means, please clarify.

Authors' response:

The different scales, stakes and water management issues of the two basins constrained us to develop a more generic and robust model, to avoid the drawbacks of a site-specific study.

Authors' changes in manuscript:

Some clarifications were added at the end of the last paragraph of the introduction p. 5, lines 17-20 (the added text is in italic):

"(…)the Herault basin (*2 500 km^2*, France) and the Ebro basin (*85 000 km^2*, Spain). This constrains its conception to different *spatial* scales, stakes and *water management issues (mainly population growth, irrigation and tourism for the Herault basin, and irrigation for the Ebro basin).*

Public comment:

Page 12321, line 4-7: This sentence feels a bit clunky; consider rewording.

Authors' changes in manuscript:

The sentence "Since additional data (e.g. wind and humidity) were too scarce to calculate Penman Monteith ET_0 at the scale of the Ebro basin from 1969 to 2009, the Hargreaves empirical equation was used to calculate ET_0 at a daily time step." Was changed to "Since additional data for the calculation of Penman-Monteith ET_0 (e.g. wind and humidity) at the scale of the Ebro basin from 1969 to 2009 were too scarce, the Hargreaves empirical equation was used to calculate ET_0 at a daily time step." (p. 6 lines 9-12).

Public comment:

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.

Authors' response:

We agree that the fact that we applied two different formulae to calculate potential evapotranspiration is debatable. Considering data availability the common formula we would have used is based only on temperature (Oudin et al., 2005) and not necessarily adapted to agronomic studies. The wind factor is important especially when studying variations of evapotranspiration over time: for example we tested both Oudin and Hargreaves formulae in the Ebro basin. The Oudin formula produces a lower potential evapotranspiration, but with a stronger increase in time than the Hargreaves formula in which the wind factor weighs in. Moreover, simulations of water demand and dam management led to more realistic results with the Hargreaves formula than with the Oudin formula. Thus we preferred to use the most accurate formula applicable in each basin, rather than a common formula for both basins. While minimum and maximum temperature data were available in the Ebro basin, only average daily temperature was available in the SAFRAN grid over the Herault basin, thus we could not use the Hargreaves formula in the Herault. One could also argue we could have calculated the Penman-Monteith ET_0 in the Ebro by estimating other parameters, but this, in our opinion, would not necessarily have been more precise than using the Hargreaves equation with the wind factor which was, furthermore,

calibrated over large areas of the Ebro basin (se Martinez-Cob & Tejero-Juste, 2004 and Garcia-Vera & Martinez-Cob, 2004).

Reference: Oudin, L., Hervieu, F., Michel, C., Perrin, C., Andréassian, V., Anctil, F. & Loumagne, C. (2005): Which potential evapotranspiration input for a lumped rainfall-runoff model?: Part 2 – Towards a simple and efficient potential evapotranspiration model for rainfall-runoff modelling, J. *Hydrol.,* 303, 290-306.

Authors' changes in manuscript:

We added a brief explanation of our choice to calculate ET0 in section 2.2, p.6, lines 15-20:

"If a common formula in both basins were to be used, considering data availability it would have been based only on temperature (e.g. Oudin et al., 2005) and not necessarily adapted to agronomic studies. Moreover, simulations of water demand and dam management led to more realistic results with the Hargreaves formula than with the Oudin formula. Thus we preferred to use the most accurate formula applicable in each basin, rather than a common formula for both basins."

However interesting this point, we propose not to further modify the manuscript by adding additional discussion on the calculation of ET_0, to avoid lengthening an already relatively long manuscript. Considering the more general scope of this study, we hope this choice will suit the referees and the editor.

Public comment:

page 12321, line 11-14: all variables need to be defines; TMOY was missed.

Authors' changes in manuscript:

P. 6 lines 22-27: T_{MOY} was change to T_{MEAN} in equation (1) and the meaning of T_{MAX}, T_{MIN} and T_{MEAN} were added in the caption.

Public comment:

Page 12322, line 1: it is unclear whether the temperature values stated for each basin were yearly averages or min/max values.

Authors' response:

We agree this could be confusing. We modified the two sentences regarding temperature and precipitation contrasts in the Herault basin.

Authors' changes in manuscript:

P. 7 line 5-6: We changed "The temperature ranged from 6°C in the winter to 20°C in the summer(…)" to "The average seasonal temperature in the period 1971-2009 ranged from 6°C in the winter to 20°C in the summer(…)".

P. 7 line 7-10: We changed "Temperature and precipitation follow a north-to-south gradient in the basin, ranging from under 8°C to over 15°C and from over 1600 mm/y to less than 600 mm/y" to "Temperature and precipitation follow a north-to-south gradient in the basin: average annual temperatures range from 8°C in the north to over 15°C in the south, and average precipitation ranges from over 1600 mm/y in the north to less than 600 mm/y in the south."

Public comment:

A short explanation of what is meant by "… typical Mediterranean regime…" would be helpful.

Authors' changes in manuscript:

P. 7 line 18: The word "typical" was deleted and an explanation was added in brackets : "The Herault and its tributaries have a Mediterranean regime *(severe low flows in summer and high flows in fall and winter with potentially severe floods in the fall)*, while hydrological regimes in the Ebro basin vary from…".

Public comment:

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.

Authors' response:

The decrease in winter precipitation and increases in fall precipitation are between the 1971-1980 and the 1981-2009 periods. This clarification was added in the sentence.

Authors' changes in manuscript:

P. 7 line 29: "between the two periods" was added in the sentence: "Seasonal disparities were identified in the precipitation trends: while winter precipitation decreased by approximately 40% *between the two periods* in both basins, fall precipitation increased by 21% over the Herault basin and by 12% over the Ebro basin."

Public comment:

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").

Authors' response:

These comments were accounted for and the manuscript was modified accordingly.

Authors' changes in manuscript:

P. 8 lines 17-18: "(…) and 30% of the country's meat production (CHE, 2011) and supports a dynamic agro-industrial sector" was changed to "(…) and 30% of the country's meat production (CHE, 2011). Agriculture in the basin contributes to a dynamic agro-industrial sector".

P. 8 lines 25-28: "In the recent past, increasing demand in both basins (since the 1970s, the population has doubled in the Herault and irrigated areas have increased by 30% in the Ebro) and drier conditions have led to water shortage events." Was changed to "Since the 1970s, the population has doubled in the Herault and irrigated areas have increased by 30% in the Ebro. Increasing demand and drier conditions have led to water shortage events in both basins."

P. 8 line 38: "Nowadays the CHE (…)" was changed to "The CHE now (…)".

Public comment:

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 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.

Authors' response:

The second paragraph of section 3.1.1 was modified to clarify the steps of the calculation of water shortage and the way in which we accounted for withdrawals and return flows. The quantification of consumptive use and return flows is explained in section 3.3.2.

Authors' changes in the manuscript:

The second paragraph of section 3.1.1 ("Interactions between water resources and demand…the remaining water returns to the river through leaks in urban and irrigation supply networks or as treated effluent") was changed to:

"At each demand node, water demand is compared to water availability (based on streamflow and reservoir levels). If water availability is equal to or higher than water demand, then water withdrawals are equal to water demand for all types of demand. If water availability is lower than water demand, then restrictions are applied to limit withdrawals. According to the order of priority defined locally, restrictions are first applied to AWD, then OWD, and lastly to UWD. Water shortage is calculated through the difference between water demand and effective water withdrawal. Only a part of the water withdrawn is actually used, the rest is considered to return to the sub-basin outlet as return flow. The quantification of consumptive use and return flows is explained in section 3.3.2. Natural streamflow is thus modified by dam management, water withdrawals and return flows." (p. 9 lines 23-31)

Public comment:

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.

Authors' response:

The lack of discussion on groundwater was also pointed out by other reviewers. We agree this is a deficiency and we included more details and discussion on our account of interconnections between groundwater and surface water in the revised version.

Also in section 3.3.2 we added an explanation of our account of return flows: it assumes a strong link between surface and groundwater since part of the losses from supply networks and inefficient irrigation techniques were considered to return to surface flow at the outlet of each sub-basin: "For each type of water demand, a part of the water withdrawn was considered to return to the environmental and, *in fine*, to the surface outlet the sub-basin in which the water was pumped." The part to return to surface flow was determined, as explained in section 3.3.2: "Return flow rates were tested from 0 to 1 with a step of 0.1 and were calibrated by optimizing goodness of fit criteria including NSE on low flows (…)".

Authors' changes in manuscript:

A paragraph was added in the discussion section (p. 25 line 35 - p. 26 line 16):

"Another point to consider is the assumption made on groundwater simulation and the groundwater-surface flow links. The hydrological model used in this study does not fully account for groundwater and groundwater-surface flow links. It simulates streamflow coming from both surface and groundwater sources. Other models that properly account for surface water-interactions exist (e.g. Pulido-Velazquez

et al., 2007; 2012). However, the application of such models at the scale of the Herault or the Ebro basin is complex, considering the heterogeneity of hydrogeological contexts (e.g. the Herault basin varies from schist to karstic and alluvial zones), and the availability of piezometric data, too sparse in space and in time to allow acceptable calibration and validation of a more complex hydrological model over the 40-year period. Considering the small contribution of groundwater to water supply in the Ebro basin (4%, see CHE (2011)), this modeling issue probably has a limited impact of the simulation of streamflow. In the Herault, the sub-basins of the Vis at Saint-Laurent, Herault at Gignac and Lergue at Lodeve have developed karstic systems, and low flows are comprised almost exclusively of streamflow from karstic springs. Our simulations somewhat underestimate low flows of the Vis at Saint Laurent and the Herault at Gignac, which could be explained by the inability of the GR4j model to represent to complex karstic system. However the main water withdrawal in these areas is by far the Gignac irrigation canal, which takes water directly from the Herault river upstream from Gignac. In the downstream sub-basin of the Herault (the Agde area), most of the water supply comes from the alluvial aquifer a few meters from the river bed. In our simulations, we assumed these withdrawals impacted surface flow directly, since surface and groundwater flows have been showed to be tightly linked in this area (Weng and Dörfliger, 2002)."

References:

Pulido-Velazquez, D., Sahuquillo, A., Andreu, J., Pulido-Velazquez, M, 2007. An efficient conceptual model to simulate water body-aquifer interaction in Conjunctive Use Management Model. *Water Resources Research* 43: W07407, doi: 0.1029/2006WR005064

Pulido-Velazquez, D., Sahuquillo, A., Andreu, J., 2012. A conceptual-numerical model to simulate hydraulic head in aquifers that are hydraulically connected to surface water bodies. *Hydrological Processes* 26, 1435-1448.

Weng P., Dörfliger N., 2002. Projet PACTES — module: contribution des eaux souterraines aux crues et inondations; site de l'Hérault. BRGM/RP-51718-FR.

Public comment:

Page 12326, line 4: (…) Perhaps estimate would be a better choice.

Authors' changes in the manuscript:

P.10 lines 15-19: we changed "determine" to "reproduce" and also amended the text for more clarity, following another referee's comment: "The spatial distribution of water demand and availability was mapped to correctly *reproduce* the *spatial heterogeneity* of water shortage in each study area: *water stress assessments can vary depending on the spatial or temporal scale (Boithias et al., 2014). Thus it is essential to properly account for the main heterogeneities in water demand and availability.*"

Reference:

Boithias, L. Acuña, V., Vergoñós, L., Ziv, G., Marcé, R. & Sabater, S. (2014). Assessment of the water supply:demand ratios in a Mediterranean basin under different global change scenarios and mitigation alternatives. *Sci Tot Environ* 470-471, 567-577.

Public comment:

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?

Author's response:

We fully agree with this comment. We changed this sentence to qualify our statement.

Authors' changes in manuscript:

The sentence "(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" was changed to: "The part of natural streamflow and anthropogenic consumptive use variations in the cause of the decrease in streamflow observed was approached by comparing variations in simulated natural and influenced streamflow." (p. 14 line 28 to p. 15 line 2))

Public comment:

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.

Author's changes in manuscript:

Agreed. "industrial demands" was deleted.

Public comment:

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 theses sub-basins discussed less throughout the paper?

Authors' response:

Some of these downstream sections, particularly in the case of the Herault basin, are key supply nodes to be considered in the water demand/availability balance of the basin. Therefore we found it important to discuss them throughout the paper. However, note that although the natural streamflow in these specific sub-basins was poorly simulated, they generally hardly contributed to the total discharge at their outlet, and total influenced streamflow was generally correctly reproduced (see e.g. validation of simulated influenced streamflow for the Herault at Agde, figure 7).

Public comment:

Page 12334, line 15: This sentence needs to be reworded; perhaps simulated reservoir levels would work better.

Author's changes in manuscript:

The sentence “Figure 6 shows the results of the simulation of reservoir levels in comparison with observed reservoir levels in the two basins” was changed to “Figure 6 shows the simulated reservoir levels in comparison with observed reservoir levels”. (p. 17 lines 1-2)

Public comment:

Page 12335, line 1: Could the authors choose a better term than “less well” to describe the dam simulation results?

Author’s changes in manuscript:

This sentence was deleted in the revised manuscript.

Public comment:

Page 12337, First paragraph: The authors explain the correlations between anthropological impacts and streamflow based on Figure 9. In the Ebro basin, the correlations are not as clear. Could the authors comment on this?

Author’s response:

This paragraph was meant to show the increase, in both basins, of the impact of consumptive use on streamflow: the first graph of Figures 9a and 9b (“Annual water consumption”, title changed to “Annual consumptive use” in the revised manuscript) shows that annual consumptive use has increased, in absolute terms, over the period. Moreover, we calculated the percentage of natural streamflow that was consumed by water use. This percentage increased in both basins.

Authors’ changes in manuscript:

P. 19 lines 12-19, the paragraph was changed to: “Figure 9 also shows that the consumptive use increased between 1971 and 2009 in both basins, as an absolute value and in comparison with natural streamflow: average annual consumptive use increased from 11 hm^3/y (1% of mean annual natural streamflow) in the 1970s to 27 hm^3/y (2% of mean annual natural streamflow) in the 2000s in the Herault basin, and from 3 830 hm^3/y (24% of mean annual natural streamflow) in the 1970s to 5 000 hm^3/y (38% of mean annual natural streamflow) in the 2000s in the Ebro basin. Annual water consumption stabilized in the 2000s in both basins.”

Public comment:

Page 12337, line 14-16: This sentence needs to be reworded of the statement should be further explained.

Page 12337, line 18: there is redundancy in this sentence; delete “in the Ebro basin”.

Authors’ changes in the manuscript:

The sentences “Although the annual anthropogenic impact was higher in the Ebro basin, it reached 30% of natural flow at a 10-day time step in the Herault basin between mid-July and mid-August in the 2000s (Fig. 9a). While the impact of human activities was highest in the summer in the Herault basin, the storage role of reservoirs in the Ebro basin is clearly visible in figure 9b in the Ebro basin, anthropogenic impacts decreased in July and August, when withdrawals were made from reservoirs”

were changed to:

"In the Herault basin the impact of human activities was highest in the summer: it reached 30% of natural flow between mid-July and mid-August in the 2000s (Fig. 9a). In the Ebro basin the storage role of reservoirs is clearly visible with anthropogenic impacts decreasing in July and August, when withdrawals were made from reservoirs (Fig. 9b)." (p. 19 line 20)

Public comment:

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?

Authors' response:

Despite the limitations described in Section 5.2, the results showed that we rather successfully simulated influenced streamflow and reservoir levels over a multi-decadal past period, while considering significant variations in anthropogenic and climatic drivers. The complexity of this task implies necessary simplifications and uncertainties in the simulations. Nevertheless, dealing with the limitations discussed at the end of the paper at the hydrological mesoscale and with a multi-decadal time depth would constitute several separate studies, which we do not plan to deal with in the immediate future.

Considering more flexible restriction rules (see discussion paragraph p.26 lines 19-25) may be an option to consider for future simulations, however the first simulations of water stress under prospective water use and climate scenarios will be run with the model as presented in this manuscript.

Public comment:

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.

Authors' changes in the manuscript:

Agreed. Figure 1 was changed accordingly.

Public comment:

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?

Authors' response:

The priorities indicated in figure 2 refer to management rules that apply in case of insufficient water supply: restrictions are applied first to agricultural water withdrawals (since they are last in the order of priority), then to industrial withdrawals, and lastly to urban water withdrawals. These management rules differ from water allocations, and from the quantity of water actually withdrawn.

Authors' changes in the manuscript:

We modified section 3.1.1 to better explain the use of withdrawal restrictions and priorities in the modeling framework. We replaced the second paragraph ("Interactions between water resources and water demand are accounted for by the following assumptions…as treated effluent" with the following paragraph (p.9 line 24):

"At each demand node, water demand is compared to water availability (based on streamflow and reservoir levels). If water availability is equal to or higher than water demand, then water withdrawals are equal to water demand for all types of demand. If water availability is lower than water demand, then restrictions are applied to limit withdrawals. According to the order of priority defined locally, restrictions are first applied to AWD, then to OWD, and lastly to UWD. Water shortage is calculated through the difference between water demand and effective water withdrawal. Only a part of the water withdrawn is actually used, the rest is considered to return to the sub-basin outlet as return flow. The quantification of consumptive use and return flows is explained in section 3.3.2. Natural streamflow is thus modified by dam management, water withdrawals and return flows."

Public comment:

Page 12355: in Figure 3, a space is missing between the words "section" and "for".

Authors' changes in the manuscript:

Space added in caption of Figure 3.

Public comment:

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.

Authors' response:

Thank you for your interest in this paper. This paper is indeed a prior step to the modeling of different potential scenarios, however we believe it also presents a fair amount of new information (see outcome section and last response to Referee #3). Presenting the use of this model in prospective studies will imply quite detailed methodological explanations (e.g. climate and water use scenario building) and an in-depth analysis of simulation results, hence we believe this could not all have fitted in one single paper.