# Answer to comments from Referee 3 (L. Menzel)

**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.

**<u>General referee comments:</u>**

*There are several global water models which are applied to simulate water availability and water demand on a large scale (on a country base or for large river basins). Although they deliver an assessment of the balance between water demand and availability on the large scale, their spatial resolution is too coarse, and the underlying data are not detailed enough to represent processes and developments on the hydrological mesoscale. So far, respective studies which could fill this gap were scarce, but the study presented in this paper is a very valuable contribution of such a mesoscale investigation. The two investigated basins are representative examples for Mediterranean conditions which are increasingly affected by a combination of rising water demands (e.g., through increasing agricultural irrigation or water demand from tourist centers) and the effects of strong climate variability and climate change which adversely modify the availability of natural water resources. The authors developed a very detailed approach to include the most important drivers and parameters which control human water demand (including urban, agricultural and other water demands) for the two selected catchments. They interviewed relevant stakeholders regarding the driving forces of water demand, the decisions to allocate available water resources and even tried their best to represent dam management as realistic as possible. Although the study still includes a variety of assumptions and shows several shortcomings (for example with respect to the spatial resolution of the meteorological input parameters), one should however regard the (successful) attempt to develop an integrative modelling framework which is able to represent water stress on the mesoscale and to distinguish anthropogenic impacts on streamflow variability from the impact of climate variability. Moreover, the authors have their own critical look on their study (see Discussion and Limitations sections of the paper), i.e., they always keep a realistic view, and they anticipate any critical questions from the reviewer. To conclude, the paper is a valuable contribution to the ongoing discussion how climate variability and change will – in combination with socio-economic changes – modify water stress on the mesoscale. I am sure that the presented approach could be (in a modified and further developed way) applied in different environmental and anthropogenic settings. Eventually, one should also highlight the excellent figures and graphical representations in the manuscript.*

**Authors' response:**

We would like to thank Dr. Menzel for his interest in our paper and for underlining our efforts to represent water availability and demand on the hydrological mesoscale.

**<u>Specific comments:</u>**

**Referee comment:**

*The length of the paper's title appears inadequate and reads complicated; therefore it is recommended to shorten the title.*

**Authors' response:**

Agreed. We have slightly modified the title to make it shorter and answer to the comments of Dr. Menzel and of anonymous Referee #2.

**Authors' changes in manuscript:**

Title changed to: "Simulating past changes in the balance between water demand and availability and assessing their main drivers at the river basin scale "

**Referee comment:**

*The frequently used term "hydrosystems" sounds a bit technical and not well defined, it could therefore be confused with another meaning.*

**Authors' changes in manuscript:**

The term "hydrosystem" was replaced with "river basin" until the end of the introduction, where it was defined as "systems made of water and the associated aquatic environments within a delimited geographical entity" (p.4 lines 33-34). We wished to refer to the study areas as "hydrosystems" fully incorporating the different water uses and the influence that these uses may have on water resources (including storage and supply facilities), to emphasize the connections between streamflow and human water uses.

**Referee comment:**

*The Introduction (…). Maybe it is a subjective impression of the reviewer, but this section reads not very well, it appears to be overloaded with a variety of specific expressions which hinder a smooth flow in reading. Maybe it could be smoothed through the reduction of some cumbersome expressions?*

**Authors' response:**

We tried to be more specific in the definition of some of the terms and clarified a few sentences in the introduction.

**Authors' changes in manuscript:**

Please see changes in change track mode in the introduction.

**Referee comment:**

*Could you please briefly explain (in the introduction) why you selected the Ebro and Herault basins for your study? Regarding the selection of the Ebro it is maybe more understandable to the reader, but the Herault basin is quite unknown among international readers, and its size is very different from that of the Ebro.*

**Authors' response:**

Agreed. We found it interesting to study two catchments of different sizes, both with a management agency, but at different scales. Also, while water management is mainly concentrated on supplying irrigation water in the Ebro basin, water uses are quite diverse in the Herault, with different and competing water demands. Finally, the comparison of two very different basins also anticipates on the next step of our work, i.e. the use of the model in prospective studies. To state an example, comparing a highly regulated basin such as the Ebro with a basin with low storage capacity such as the Herault will bring interesting information on vulnerability of river basins to climate change.

**Authors' changes in manuscript:**

We amended the last two sentences of the introduction (p.5, lines 15-20) to briefly explain why the two basins were chosen (the added text is in italic):

"The integrative modeling framework was developed and applied in two contrasting Mediterranean catchments facing increasing climatic and anthropogenic pressures: 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)*.

**Referee comment:**

*(…) the meteorological input appears to be quite coarse information (8 x 8km) grid, especially with respect of the total area of the Herault basin. Don't you think that a finer spatial grid would better represent the spatial meteorological characteristics of the catchments and thus lead to more reliable modelling results?*

**Authors' response:**

This meteorological grid used over the Herault basin (the SAFRAN grid) is the grid available from Meteo France, provider of meteorological data in France. For a finer representation, we could have interpolated data from stations, which may not have led to finer information. As can be seen in Figure 3a, the 8x8km grid allowed us to show climatic contrasts in the Herault basin. Moreover climatic data was averaged over each sub-basin for input into the GR4j model, and over the irrigated areas corresponding to each demand node for simulation of agricultural water demand. Note that most of these areas cover several grid cells.

**Referee comment:**

*Why did you calculate potential evapotranspiration instead of actual evapotranspiration? Do you think it realistic to apply two quite different formulae to calculate potential evapotranspiration in the two basins? Did you apply both formulae for one basin in order to assess the bias? I think that the Hargreaves formula as presented in equation (1) is an extreme simplification of the evaporation processes (e.g. factor 0.0023 in "windy areas").*

**Authors' response:**

Potential evapotranspiration rather than actual evapotranspiration is the input variable for the GR4j hydrological model. Regarding the calculation of water demand for irrigated crops, we used the classic crop coefficient method to calculate maximum evapotranspiration of each crop, ETc, based on potential evapotranspiration as shown in figure 4.

We agree that the fact that we applied two different formulae to calculate potential evapotranspiration is a limitation in this study. 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 $ET_0$ 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."

**Referee comment:**

*Equation (1): what is MOY?*

**Author's response:**

Tmoy is the mean temperature. It was replaced by Tmean for more clarity in the manuscript.

**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.

**Referee comment:**

Section 5.3: please add a web link to the GICC-REMedHE project.

**Authors' changes in manuscript:**

Done at line page 27 line 15.

**Referee comment:**

*Could you please state (in your response to the reviewers comments) what is different in this paper with regard to the previous papers you and your colleagues published earlier, especially the articles of Collet et* *al. (2013), Collet et al. (2014), as well as the papers of Milano et al. cited in your manuscript? What is substantially new in this manuscript?*

**Authors' response:**

The previous work by Milano et al. and Collet et al. are part of the effort of our research team to study the impacts of climate change and possible adaptation for water management by integrating human and climatic aspects at the scale of managed river basins. Although this work was evidently developed on the basis of these previous studies, a number of improvements and additions can be put forward.

- First, an effort was made to use the same method in both basins, in order to test its robustness and applicability to different hydrological and water use conditions. The framework is quite similar to Collet et al. (2013) but here compromises had to be made to apply it in a bigger, more complex system.

-As part of the efforts to use the same methodology in both basins, the same dam management model was applied to the Herault and the Ebro basins. In Collet et al. (2013) a detailed model was developed especially for the simulation of the Salagou dam, whereas in this study a more generic, demand-driver model was applied to all dams.

-In the Ebro basin, compared to Milano et al. (2013) a new conceptualization of the basin was proposed, closer to management realities. Data with finer time steps and space scales were used for simulation and analysis, and hydrological regime of the sub-basins was better accounted for, notably in the case of snowmelt regimes. Also, one of the main improvements compared to Milano et al. (2013) is the historical reconstruction of water demand in the Ebro.

-Even with the strong assumption made here to simulate natural streamflow, we believe it is a step forward compared to previous studies.

-This simulation of the impact of consumptive use on streamflow enabled us to compare the changes in natural and influenced streamflow in the past. We were able, for example, to indicate that the streamflow variations in the Ebro were also caused by anthropogenic pressures whereas natural variability was the primary cause of streamflow variations in the Herault.

-Finally the comparison of water shortage in both basins brought some interesting information, a point that will be further enhanced in the comparison of water stress under climate and water use scenarios.

**Authors' changes in manuscript:**

Although it was not formally asked, we added a paragraph in the discussion section on the comparison of this study with other similar studies, including the previous studies of our team, since we believe it helped show this work's originality (p.22 line 28 to p.23 line 6).

"To our knowledge other studies did not simulate natural and influenced streamflow at a 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."