

Answer to comments from the editor (P. van der Zaag)

Editor general comments:

The paper by Fabre et al. 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” has raised quite some interest, as can be gauged from the four comments received, which all provide valuable and detailed suggestions.

I invite the authors to respond in detail to these comments, indicating how they intend to address these in a significantly improved manuscript.

Authors’ response:

We would like to thank the Editor P. van der Zaag for his interest in our paper, as well as the two anonymous referees, and L. Menzel and C. Steele for their valuable, detailed comments on the manuscript. They have been thanked in the acknowledgements; should the two anonymous referees wish to lift their anonymity, we would gladly thank them by name.

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.

Editor comment:

The paper emphasises climate change. The argument hinges on an assumed change in climate around the year 1980 (was the climate during the 1970-1980 period average or relatively wet compared to long term climate data?). I feel that this assumption should be substantiated by referring to appropriate scientific literature.

Authors’ response:

Climate change is indeed one of the background elements for this paper. However in this paper we first test our model in the past: in order to ensure that it can be used in conditions of climate change, we tested in on a long past period which comprised significant climate variability. We applied statistical tests to the temperature and precipitation series in both basins to characterize this variability, and detected a statistically significant break in 1980. However, we did not mean to assume a change in climate around the year 1980. Although the past period does not represent climate change proper, the significant climate variability over the period 1971-2009 shows the robustness of the model in variable climatic conditions

Authors’ changes in manuscript:

P. 7 line 22 we added the following text at the beginning of the paragraph (text in italic):

“The period 1971-2009 comprises significant climate variability: statistical breaks in temperature and discharge series were detected in both basins.”

Editor comment:

The concept “water demand” needs to be specified: do we need demand proper (how measured/quantified?), actual water withdrawal (without deducting return flows), actual consumptive use? Perhaps it would be appropriate to refer to some publications where water use (not demand) is appropriately taken care of, such as Kiptala et al. (2014).

Authors’ response:

Agreed. Water demand is defined here as the amount of water that users would withdraw without restrictions, i.e. the withdrawals that would enable users to have access to optimal amounts of water considering the efficiency of supply networks and irrigation techniques.

Water withdrawals were then estimated based on the simulation of water demand and water availability: if water availability was greater than water demand, then withdrawals were considered equal to demand. If water demand was greater than water availability, then withdrawals were considered to be limited by restrictions applied first to the agricultural sector, then the industrial sector, and last to urban water use.

Finally, consumptive use was assessed as described in section 3.3.2.

Authors’ changes in manuscript:

We added a paragraph in the introduction to emphasize the need to properly account for water withdrawals and water use (p. 4 line 5):

“In this mindset, interactions between water demand and water resources should also be accounted for. Notably, few studies have actually simulated natural streamflow separately from influenced streamflow. In some cases, hydrological models are calibrated on observed (i.e. influenced) streamflow data, and the simulated streamflow is compared to water demand to assess water stress (e.g. Collet et al., 2013). Also, a proper distinction should be made between water demand, withdrawals, and consumptive use, which was done for example in Collet et al. (2013), and Kiptala et al. (2014). Finally, a complex issue is the representation of the interactions between groundwater and surface water, i.e. the use of groundwater resources and the influence that their management could have on streamflow, and the partition of return flows from network losses and inefficient irrigation techniques between evaporation, groundwater recharge and return to surface flow.”

Editor comment:

It remains unclear how precipitation on the lakes of reservoirs and evaporation from such lakes have been accounted for (see e.g. section 4.1.1).

Authors’ response:

Agreed. In order to condense the manuscript, we had not added any details on the calculation of precipitation and evaporation on and from the reservoirs. In the revised manuscript, we have added a sentence to clarify this point. P and ET_0 were computed by averaging the grid data over the surface of the lakes, available through GIS maps. Evaporation was then computed by multiplying ET_0 by a coefficient K_c recommended by Allen et al. (1998) for open water in a temperate climate. To convert the precipitation and evaporation values from mm to volumes, we then multiplied by the surface of the lake, i.e. the surface corresponding to the reservoir level according to the surface-level curve if available, otherwise the surface on the GIS map.

Authors' changes in manuscript:

In section 3.3.1 we added the following text (page 13, lines 6-9);

"Precipitation and evaporation on the reservoirs were computed by averaging the P and ET_0 grid data over the surface of the reservoirs, available through GIS maps. Evaporation was then computed by multiplying ET_0 by a coefficient K_c , recommended by Allen et al. (1998) for open water in a temperate climate."

Editor comment:

We need a more critical discussion of the results in section 4.1.2. Observed dam levels are ideal as a mean to validate model results. So low levels of NSE and VEM are problematic and need to be critically discussed, including the result of for example J-Caspe (in Figure 6).

Authors' response:

Agreed. Our simulation of the storage and regulation by 11 dams in the Ebro basin is an extremely simplified view of the Ebro management system, which comprises over 230 dams. We added a paragraph in the discussion section.

Authors' changes in manuscript:

We added the following text in the discussion section (p. 25 line 1):

"As shown in section 4.1.2, the functioning of some dams was rather poorly simulated. In the case of the Caspe and Talarn dams, this may be due to the streamflow regulations by other dams located upstream and not included in this study. Furthermore the Talarn dam is mostly operated for hydropower production, which was not included in this study. In the case of the Caspe dam, the irrigated areas actually linked to the dam may have been underestimated. The Sotonera and Grado dams are operated jointly since 1981. The reconstruction of the changes in irrigated areas and their attribution to one dam or the other was a difficult task and a misrepresentation of the links between irrigated areas and each dam might have led, for example, to the Sotonera dam to be under-solicited in our simulations. The same can be said of the jointly operated Barasona and Santa Ana. Moreover our simulations considered the three dams Escales, Canelles and Santa Ana as one large dam in the location of Santa Ana (see section 3.3.1). This may be an oversimplification of the real-life management of these dams."

Editor comment:

Section 4.1.3: if the key issue of the paper is increasing water withdrawals and use, then a key indicator for model performance should be whether a model can reproduce low flows. This should in my view be a critical and central issue.

Authors' response:

A paragraph was added in the results section and another in the discussion to comment on the model's performance over low flow periods.

Authors' changes in manuscript:

Paragraph added in the results section (p. 17 line 33):

"The ability to represent observed low flows, which are highly influenced by storage and water withdrawals, is an indicator of the modeling chain's efficiency. Low flows were rather well simulated at the outlet of the Herault basin (Herault at Agde) with NSELF values of 0.66 and 0.87 over the 1981–2009

and 1971–1980 periods respectively. The low NSELF values in the Vis at Saint-Laurent and Hérault at Laroque sub-basins over the 1971–1980 period (0.28 and -0.16) respectively, are most likely due to difficulties of the hydrological model to reproduce the functioning of the karstic system in wet periods, as it was calibrated over a drier period. Low flows in some of the Ebro sub-basins basin were poorly simulated, particularly in the Cinca and Segre sub-basins, where low flows are highly influenced and depend almost exclusively on the outflow from the various storage dams.”

Paragraph added in the discussion (p. 25 line 13):

“The ability to represent observed low flows, which are highly influenced by storage and water withdrawals, is an indicator of the modeling chain’s efficiency. Low flows were rather well simulated at the outlet of the Hérault basin. As indicated in section 4.1.3, the poor simulations of low flows in the upstream sub-basins of the Vis at Saint-Laurent and the Hérault at Laroque are most likely due to difficulties of the hydrological model to reproduce the functioning of the karstic system. Indeed withdrawals are very scarce in these areas, therefore their influence on streamflow is limited and many natural streamflow data were available for the calibration of the model. The simulated low flows in the Hérault at Gignac could be biased because of the upstream natural streamflow simulations and because of a biased simulation of withdrawals from the Gignac canal and their influence on streamflow. Low flows in the Ebro basin were more problematic, particularly in the Cinca and Segre sub-basins, where low flows depend almost exclusively on the outflow from the various storage dams. Streamflow is influenced by the management of the Grado dam (operated jointly with the Sotenera dam) and of the Barasona dam (operated jointly with the Santa Ana dam) at the outlet of the Cinca sub-basin, and by the management of the Santa Ana dam and of the jointly operated Talarn and Oliana dams at the outlet of the Segre basin.”

Editor comment:

In the ‘Limitations’ section (5.2) it is stated that some simulations may not have been realistic since irrigation seasons started with few water resources available and half-filled reservoirs. My question is: why could the authors not model these situations more realistically, if the very essence of the paper is to simulate water demand and availability?

Authors’ response:

This paragraph may have been confusing in the submitted version. We believe we correctly reproduced demand and availability at the beginning of each irrigation season. However, during irrigation season, we assumed that irrigators used all the water available in the dam without anticipating eventual shortages that could appear during the season. Consequently, some severe limitations on withdrawals from the reservoirs can be seen in our results. In the Ebro basin, as explained in the discussion section, local ‘water parliaments’ (*Juntas de explotación*) can re-allocate water according to the level of reservoirs and the snow known to be stored upstream from the dams. Consequently, sometimes withdrawals can be limited, say by 80%, from the start of the season, in order to keep some water for the end of the season, instead of limiting 100% of withdrawals once the reservoir comes to its minimum level. This management strategy was not accounted for in our model.

We hope this answers the Editor’s question.

Detailed editor comments:

-p. 12332 line 23-24: *"Maximum shortage (MS) was defined as the maximum simulated annual irrigation water shortage rate". Not clear what it means. What is "shortage rate"? How has it been defined?*

Authors' changes in manuscript:

In section 3.1.1 a sentence defining water shortage was added: "Water shortage is calculated through the difference between water demand and effective water withdrawal." (p. 9 lines 27-28)

-p. 12332 line 24-26: *"...reliability (Rel) and [sic!] water considered to be the rate of occurrence of satisfactory years, i.e. years with an annual irrigation water shortage below 50%." What is the basis of this definition?*

Authors' response:

This definition was adapted from the reliability indicator used in Asefa et al. (2014) and other previous studies (Hashimoto *et al.*, 1982, Fowler *et al.*, 2003, cited in the submitted manuscript). Reliability is defined as the rate of occurrence of satisfactory years for the water supply system. In the Ebro basin, the *Confederación Hidrográfica del Ebro* considers that water supply to irrigated areas is satisfactory if it is greater than 50% of the water allocated. We applied the same definition to the Herault basin to facilitate the comparison between the two basins.

Authors' changes in manuscript:

We added a reference to Asefa et al. (2014): "(Rel) and was considered to be the rate of occurrence of satisfactory years (*adapted from Asefa et al., 2014*), i.e. years with an annual irrigation water shortage below 50%" (p. 15 line 28)

-p. 12333 line 4: *"The final indicator was the frequency of occurrence of water sharing conflicts (C)." How are such conflicts defined and measured?*

Authors' response:

As defined in the previous paragraph (p. 15 lines 13-16) water sharing conflicts are considered to appear when the model simulated urban shortage, which means that all other less priority uses are also experiencing restrictions: "Years with urban water shortage exceeding 5% of the demand during at least one 10-day time step were also identified. These years corresponded to critical situations in which withdrawal restrictions also applied to non-agricultural users, pointing to potential water sharing conflicts."

Authors' changes in manuscript:

We added a sentence (p. 15 line 37): "The final indicator was the frequency of occurrence of water sharing conflicts (C). As stated above, the years with an occurrence of water sharing conflicts were considered to be the years with urban water shortage exceeding 5% of the demand during at least one 10-day time step."

-Some words may be used inappropriately: p. 12318 line 6: *"tackled"*; p. 12320 line 9 and 16: *"boarded"*; p. 12320 line 19: *"anthropic"*; p. 12326 line 14: *"isolated"*.

Authors' changes in manuscript:

"tackled" was replaced with "dealt with"

"boarded" was replaced with "bordered"

“anthropic” was replaced with “anthropological”

“isolated” : the original sentence “The Gignac canal and its irrigated areas were isolated in the Herault at Gignac sub-basin” was replaced with: “The sub-basin of the Herault at Gignac was delimited to isolate the Gignac canal and its irrigated areas”.

-Figures 2 and 4: “Agricultural water demand” is in my view a misnomer. More appropriate would be “Irrigation water demand”, since it excludes rainfall.

Authors’ response:

Although we agree the term “agricultural water demand” is debatable, we would prefer keeping it as is in the manuscript, mainly to be consistent with other published papers referring to the same project, e.g. Grouillet et al. (2015). Moreover, since in our study water demand refers to blue water, we believe the term “agricultural water demand” to be equivalent to “irrigation water demand”, in our case.