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Hydrology and Earth System Sciences
Discussions

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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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.

I would like to add a few comments myself, which I hope the authors will take into consideration as well.

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.

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 water 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).

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).

We need a more critical discussion of the results in section 4.1.2. Observed dam levels are ideal as a means 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).

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.

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 water availability?

Some detailed comments

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11, C5784–C5786, 2014

Certain words and phrases are in my view problematic:

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

p. 12332, line 24-26: “... reliability (Rel) and [sic!] was 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?

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

Some words may be used inappropriately:

p. 12318 line 6: “tackled”

p. 12320 lines 9 and 16: “boarded”

p. 12320 line 19: “anthropic”

p. 12326 line 14: “isolated”

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

Reference

Kiptala, J.K., M.L. Mul, Y. Mohamed, and P. van der Zaag, 2014. Modelling stream flow and quantifying blue water using modified STREAM model for a heterogeneous, highly utilized and data-scarce river basin in Africa. Hydrol. Earth Syst. Sci. 18: 2287–2303.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12315, 2014.