

## ***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.***

### **Anonymous Referee #2**

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#### General comments

Scientific Significance: Fair (3) In the present form no conclusion of scientific significance has been clearly described. Would the authors clarify the focus of the study, improve the embedding in scientific literature and perform additional model evaluation this could probably be overcome.

Scientific Quality: Fair (3) Discussion of the approach on calibration, validation, uncertainties is largely lacking. I find this to be a key requirement for a publication on a

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modelling study of this kind.

Presentation Quality: Fair (3) Figures are well prepared. There is a need for more precise definitions especially in the introduction of the study. The presentation of results, discussion and conclusion should be improved.

#### Specific comments

Title: I do not see why the assessment of drivers should be at the river basin management scale. Rather they are assessed at the river basin scale.

Abstract: The aim described in the abstract does not correspond to the aims described at the end of the Introduction section. No clear description of scientific relevance, outcomes, discussion or conclusion.

1 Introduction The terms vulnerability, water stress and hydrosystem are not clearly defined. This makes this section quite hard to understand. What is meant by ‘the vulnerability of a hydrosystem...’ and ‘vulnerability to water stress’? Is water stress not the effect of a sensitivity to variations, a perturbation or a pressure? Also the term ‘resource’ is used where availability of water seems to be meant. Effort should be made to use terminology in a more consistent way. If the same is meant why would you use both ‘human’ and ‘anthropogenic’? Why use ‘physical factors’ (p 12318 line 28) when you only seem to refer to climatic variations. The use of the verb ‘tackle’ (p 12317 line 19 and p 12318 line 6) seems wrong in this context. Not clear what is meant by ‘various extents’ (p12318 line 4). The text on ‘different perspectives’(p 12319 lines 1-12) seems to refer to spatial and temporal scale issues. This should be written more clearly. In this way I don’t see how it ‘...underlined the need to better understand the drivers and dynamics...’.

2 Study area At the final sentence of section 2.2 a reference would be appropriate.

3 Method 3.1 It seems that a modeling framework (p 12325 line 1) was designed. This framework is then referred to ‘integrative’ (line 24) and applied. It remains unclear

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whether this integrative framework was also calibrated/validated or tested/evaluated in any way? No description of the interactions between water use (or demand) and the resource system (or hydrosystem or water availability) is presented. Figure 2 does not clarify this. The caption could be amended to become more explanatory in this respect. No clarity on water demand/use is given. The reader is not informed on the nature of the use meant (withdrawal/consumptive). Moreover the ground water recharge-surface water interactions seem to be taken into account. In this respect it is also not clear what is meant with 'directly downstream' (p 12325 lines 16 and 21). What does 'resource vs. demand' mean? From Figure 3 it is clear that it is not water stress or water scarcity. 3.2 It seems to me that the words dynamics, variations and changes are mixed-up here and there. It is unclear how the 'natural' and 'non-natural' 10-day periods can this easily be separated. The hydrological processes involved take effect in much longer time-frames than seems to be accounted for. From the text I am also not convinced that the upstream-downstream linkages have been properly accounted for in this respect. Even more worrying is the fact that calibration seems to be based on high flows, whereas the effect of withdrawals/demand/consumptive use/reservoir operation is expected to be most relevant under low flow (dry season) conditions. 3.3 It is unclear how the simulated rules compare to actual practice. p 12331 line 'return to outlet', effectively into the sea. If so, it is not really return flow is it? p 12332 line 4: shouldn't it be 5% rather than 95%? p 12333 line 1 Would be good to include those definitions.

4 Results 4.1 I am unable to judge whether the NSE values presented are 'well reproduced' as claimed on p 12334 lines 19,20. 4.2 p 12336 line 19: So, no impact in 1990s? Figure 9 does not show a clear trend, what does it mean? Could you maybe highlight what the reader should observe? p 12337 line 15 'at a 10 day time step': at one of the time-steps? P12337 lines 26-28: My interpretation: The impact of water use has become relatively larger because natural runoff decreased that much. If my interpretation is correct, please rephrase your text. Now it seems that the impact of climatic variations is larger.

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5 Discussion and conclusion I am not convinced on how your study distinguished between hydro-climatic and human-induced dynamics (p12341 line 11). p12341 line 14: '...satisfactory results.' To who and why? p12341 lines 26-29: So what does this study exactly teaches us? What is the added value of using your model? p12342 line 16: I am not sure how you 'reproduced' them. Rather you included them. p12342 line 28: '...appeared...'? or was it real? Discussion of the approach on calibration, validation, uncertainties is largely lacking. I find this to be a key requirement for a publication on a modelling study of this kind. No conclusion of scientific significance is clearly described.

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

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