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Interactive Comment

Interactive comment on "Using expert knowledge to increase realism in environmental system models can dramatically reduce the need for calibration" *by* S. Gharari et al.

Anonymous Referee #2

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The article presents an approach to constrain the identification of hydrological model parameters, using physical reasoning and expert knowledge on the catchment behaviour. Using models of increasing complexity, the authors provide a method to constrain internal model variables and/or model parameter values. The approach is illustrated on one catchment in Luxemburg.

The article is clear and well-written. The approach proposed by the authors makes sense and may be a way to overcome the problem of model overparameterization generally faced by complex models. Therefore, it is potentially useful for modellers and should be interest to a broad audience.

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However, I found that the article fails to demonstrate the actual value of the proposed approach. Indeed, the approach is tested on a single catchment, with a single calibration/validation test and on a very short series. Therefore, nothing shows whether the results are general and whether they could be transposed to other conditions. Hence my main recommendation to the authors is to provide a more substantial evaluation of their approach, so one can be sure that their results were not obtained by chance. This means introducing more basins, using longer time series and making more systematic calibration/validation tests. Several aspects of the testing methodology could also be improved.

I also found that the article is somewhat redundant with the other article simultaneously submitted by the authors to HESS presenting the approach. It would probably make sense to merge the two articles or to publish them as companion papers.

Below I give a number of suggestions that could be considered by the authors when revising their manuscript. I advise publication after major revision.

Detailed comments

1. Introduction: Maybe "complexity" as it is understood in the paper should be shortly defined in the introduction, to clarify its meaning here.

2. P. 14803, L. 2-4: Lumped conceptual models may not be one endpoint of the modelling spectrum. Black-box models like artificial neural networks are probably further at that endpoint of the spectrum.

3. Section 2: The approach should be tested on a larger number of catchments, showing contrasted characteristics (especially various proportions of wetland, hillslope and plateau and various climate conditions). This would help demonstrating whether the approach can be also successful outside the test basin used here. Does it successfully adapt to other physical and hydroclimatic conditions?

4. P. 14817, L. 20-22: I did not understand this sentence: when calibrating the model,

the model must still be run to evaluate model efficiency.

5. P. 14819, L. 1-5: This paragraph is unclear.

6. P. 14819: The use of the runoff coefficient is a bit strange, since it is based on flows. It directly provides information about water balance. Hence it is not a typical physical characteristic available on ungauged basins. The use of simple water balance models may introduce strong errors in water balance. Their potential impact on model results is not analysed. Why runoff coefficient was used? What would be the results without this characteristic?

7. P. 14820: Equations could be presented in a more general way based on mean and standard deviation, instead of giving the values for the case study.

8. P. 14822, L. 14-16: It is unclear why the authors chose to use such a short period for model testing, which potentially increases the dependency of modelling results to the characteristics of the period. Longer time series should be used. If the authors wish to test the robustness of their approach when data availability is decreasing (which is an interesting question), they should do the corresponding tests and show results.

9. Section 3.4: The testing methodology could be improved. First, instead of only doing a single calibration/validation, the authors should do cross validation on the two periods. This would give a more complete assessment and provide a way to analyse the consistency of parameter sets identified on two different time periods (potentially with differing characteristics and therefore different impacts of constraints on parameter selection). Second, the authors could also introduce as a benchmark the models calibrated without any constraint (neither on parameters nor on states). The comparison with the author cases would be interesting to discuss.

10. P. 14822, L. 10-13: Why KGE measures were not used instead of NSE efficiencies, which are known to be biased (see the work of Kling and Gupta)?

11. P. 14822, L. 26: It cannot be said that flow observation was not use, since it is

indirectly used in the runoff coefficient.

12. P. 14823, L. 4-5: The number of constraints accounted for in each model could be indicated.

13. P. 14823, L. 10-11: Uncertainty was estimated based on the width of the confidence interval. This is not a sufficient measure of the quality of the uncertainty estimates. A confidence interval may be sharp but not reliable. Hence the authors should use a more comprehensive evaluation measure that accounts for the reliability of uncertainty estimates.

14. P. 14824, L. 23 – P. 14825, L. 2: The reason for the difference between the two models is unclear. Is it due to the difference in complexity or the difference in the number of constraints? This is not clearly shown.

15. P. 14825, L. 5-6: The interval is narrower, which is probably the direct consequence of a larger number of constraints. However, is it still reliable? Sharper confidence intervals are generally less reliable and therefore of more limited use to qualify the actual model predictive uncertainty.

16. P. 14825, L. 16-17: Same as previous comment.

17. Section 4: The consistency of parameters identified on two test periods should be analysed since it would also be an indication of the robustness of the proposed approach. It would also provide good arguments in favour of the application in the ungauged case in the context of regionalization.

18. Section 4.3: I found that this section largely repeats what was already said in sections 4.1 and 4.2. Probably a more synthetic presentation and comparison of results could be sought in sections 4.1 to 4.3, to avoid repetitions.

19. P. 14827, L. 25-26: To which extent conclusions are weaker in this case? Why is it so?

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20. P. 14829, L. 4-7: Which respective impacts have these two aspects on model results?

21. Table 2: In the equation of the slow reservoir, I did not understand why the Rp component does not appear, whereas it seems to act on the reservoir in Fig. 2.

22. Figures 3 and 4: Differences between models are difficult to see. Therefore these figures are not very useful.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14801, 2013.

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