

Interactive comment on “Moving beyond traditional model calibration or how to better identify realistic model parameters: sub-period calibration” by S. Gharari et al.

Anonymous Referee #1

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In this paper, the authors try to identify model parameters by splitting a calibration period into several parts, and searching for model parameters that work well for each of them. The idea is interesting, and the paper deserves to be published. I have a number of remarks.

- 1) The quality of the English needs improvement. I am not going to list the linguistic mistakes, because there are so many. Having a native English speaker proofread the paper would be a great help.
- 2) Page 1888 line 8: please explain the acronym DYNIA.
- 3) Top of page 1889: there are actually papers that use entire soil moisture profiles
C91

and energy balance data (from Bowen ratio or eddy covariance) to estimate model parameters (mostly published in AGU journals such as JGR or WRR, some of these papers go back 10 years or so). This seems to me very relevant to discuss here also. The methods that the authors develop could and should be applied to multivariate calibration as well. It may be a different type of models that is used in these studies ("physically-based" models), but that does not mean that they will be better calibrated than rainfall-runoff models (as a matter of fact, they probably are not, which makes this methodology all the more relevant to them).

4) I think that the term multivariate is better suited than multicriteria in this respect. A couple of RMSE values could be minimized, which is all the same criterium, but for multiple variables. This is just a suggestion, I leave it to the authors to change the terminology or not.

5) Section 3.2: please state explicitly that the model time step is 12 h.

6) This is a major comment about the method implementation: why did the authors only use 3 years of data? As the site that they are working is extremely well instrumented, it seems to me hard to believe that only 3 years of data are available. A much longer model validation would be possible then. I don't think that the model takes very much CPU time. So an extra model validation using the obtained parameters should be possible. What I would suggest is to calibrate the model in the traditional way (calibration-validation period), and compare the performance of the model using these parameters during the validation period to the performance using the new method. I would keep the calibration period the way it is now in the paper (thus the authors do not need to redo their study), but use the obtained parameters for an extra run with a validation period of a couple of extra years. That way the benefit of the new method would really become clear. This would make the paper much stronger. Doing an extra calibration in the traditional way should be easy for the authors, considering that the new methodology is much more complicated.

Overall, I think that it is a good paper that should be published. I urge the authors to take into account my comments, and especially my last comment (run the model for an extra couple of years and compare the results to the results from a traditional calibration). This way the benefit of the new method would be crystal clear. If this extra validation is not done, it is very well possible that the argument that the new method has not been validated enough will keep on coming back.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1885, 2012.