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Hydrology and Earth System Sciences Discussions

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

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The authors would like to thank the anonymous referee for his/her constructive comments which will help us to improve our discussion paper (DP) further and make it clearer for the readers.

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.

We will improve the English language in the final version.

2- Page 1888 line 8: please explain the acronym DYNIA.

The acronym of DYNIA was clarified earlier in the DP; page 1886 line 25 and page 1887 line 1.

3- Top of page 1889: there are actually papers that use entire soil moisture profiles 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 a fact, they probably are not, which makes this methodology all the more relevant to them).

We are not sure which papers the reviewer is exactly referring to. However we have incorporated some references to strengthen the link between proposed method by the DP and previous studies. In principle, the suggested calibration strategy is independent of the number of calibration objectives and/or criteria, which can be chosen at the discretion of the modeler. The same is true for the choice of the actual optimization algorithm. Rather, the method allows extracting and incorporating information from what is traditionally called the "validation period" and using this very information to strengthen the model parameterization. The advantage of the calibration method proposed by the DP is that all the available time series will be used while the different dynamics of distinct sub-periods are preserved.

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.

We agree with the reviewer that the terms multi-criteria and multi-objective should be better defined upfront. What the reviewer refers to as "multivariate" is referred to in the DP as "multi-criteria". In other words, multi-criteria calibration is defined here as calibration with respect to different criteria, such as stream flow, groundwater fluctuations or tracer response. On the other hand, "multi-objective" calibration is defined here as calibration with respect to multiple objective functions of a given criterion, such as RMSE and NSE of stream flow.

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

The time step of modeling is 12h which was stated clearly in section 3.1, however we also emphasize this in section 3.2 to clarify the units of parameters and fluxes.

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.

We do agree with this comment. We will incorporate a longer validation part in the Result section to evaluate the parameter sets defined by SuPer calibration and traditional calibration for completely different time series with different length to make the difference clearer for readers.

In the original DP, the predictability of the selected parameter sets by traditional calibration and SuPer calibration were compared for a separate time period of one year. Thus, the performance of the parameters sets selected by SuPer calibration and traditional calibration are independently evaluated in year 2004, which acts as a kind of "validation period". Note, that such a period is used here merely as a proof of concept and is not necessary for operational applications. This is what is shown in figure 4 of the DP.

Once again the authors would like to thank anonymous referee 1 for his/her constructive comment on our work. We hope that having addressed these comments in our work meets the demands of the referee and improves the transparency of the paper.

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