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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The importance of parameterization when simulating the hydrologic response of vegetative land-use change" *by* Jeremy White et al.

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In one sense the paper is relatively simple. However it raises some important points that are not commonly realised by the greater modelling community. The authors show that: âĂć Some predictions made by a calibrated model can have a posterior uncertainty that is almost as high as its prior uncertainty, notwithstanding that the model is calibrated; âĂć Failure to include in posterior predictive uncertainty analysis, all parameters to which a prediction may be sensitive, regardless of the estimability status of those parameters, can result in serious under-estimation of uncertainty.

While obvious, these two points are in direct conflict with the "modelling culture" on which a great deal of surface and land use modelling is based. A constant refrain is

that of parameter parsimony, as only a handful of parameters can be estimated on the basis of a flow time series. While this is true, it does not follow that these parameters are uniquely estimable, for the information content of the calibration dataset is normally shared among many parameters. Before calibrating his/her model, a modeller undertakes manual regularisation such that only a few parameters are exposed to the parameter adjustment process while many others are fixed. This can result in considerable parameter and predictive bias. It can, as the authors point out, also result in serious underestimation of the uncertainties of some predictions, and an illusion that the uncertainties of these predictions have been reduced through the history matching process.

Given the importance of these points, and the urgency with which they need to be understood by the surface water and land use modelling community, I have no hesitation in recommending that the paper be published.

I also like how the authors document the use of some powerful, publicly available software and methodologies of which the modelling community needs to be more aware, e.g. PEST++ and support software, including its GSA tools and parallelisation capabilities.

I do think, however, that the text would benefit from a little more fluency, if the authors can manage it. One wonders if it was given a final re-read before submission. Some obvious errors are as follows.

Line 11 on page 2: missing "an" after "in" and before "attempt"

Line 8 on page 3: alter "verify to reliability" to "verify the reliability"

Line 20 on page 3: alter "brush management focussed on the removal" to "brush management focusses on the removal"

Line 1 on page 7: "ranges from" to "ranges form".

Etc

Also there are a couple of places where the authors say “different than”. Correct grammar is “different from”.

One more thing. I have no problem with subjective likelihood functions. Also the authors warn against over-fitting – another concept with which I have no argument. However it is possible that there is information in the calibration dataset that was not “tapped” by the calibration process. This may have reduced predictive uncertainty.

Nash Sutcliffe as a calibration metric is a very blunt instrument. In my opinion it is better to form a multi-component objective function wherein the modeller ensures that certain information-rich components of a flow time series are well matched, this ensuring that the information contained in these components is transferred to model parameters. They certainly have enough parameters to do this. However I recognize that the authors are using the same calibration metrics as many others (which doesn’t make it right – but it would be wrong to penalize them). So they can address this point if they wish (or not if they don’t wish).

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