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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Practical experience and framework for sensitivity analysis of hydrological models: six methods, three models, three criteria" *by* Anqi Wang and Dimitri P. Solomatine

W. Becker (Referee)

william.becker@jrc.ec.europa.eu

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This paper presents a comparison of six sensitivity analysis approaches on three hydrological models. The methods in question are the Sobol' method, eFAST, the method of Morris (elementary effects), LH-OAT, RSA, and PAWN. The authors calculate measures of effectiveness (a rough comparison of the results between methods), efficiency (minimum runs required to reach some criteria of "effective results"), and convergence (calculation of confidence intervals using bootstrap).

The paper is reasonably clear, but unfortunately it lacks focus and novelty. To begin with the latter, the paper does not really add anything over previous comparison stud-

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Discussion paper

ies. It compares one reasonably-new approach (PAWN, although this is very similar to more established moment-independent methods), but compares it to well-established methods that have been around for a long time, and subject to many comparisons. Even inside the hydrology domain, there have been many comparisons of sensitivity analysis techniques, as noted by the authors. To add novelty in this respect, the authors would have to look at very recent developments in sensitivity analysis, perhaps including the latest metamodeling techniques, or methods that account for correlations between input parameters, multivariate output, or other more cutting-edge topics in sensitivity analysis.

From the perspective of focus, it is not really clear precisely what the authors are trying to investigate with this paper. Their main conclusions seem to be that all the methods are useful, but one should be careful to ensure that results have converged, and that different methods anyway interpret sensitivity in different ways. This kind of advice can be found in textbooks, so it is not really publishable material. In order to improve the focus of the paper, the authors should consider focusing a bit more: for example, are these results particular to their models, and to what extent can they be generalised? How do their results compare with other comparisons, and why might theirs be different? What is it about their models that makes one method more suitable than another, for example in terms of input distributions, dimensionality, degree of nonlinearity and so on? If the paper could give some kind of more in-depth analysis of why certain methods perform better than others, that would already help. However as it is, there is really nothing that a reader can take away from the paper that cannot be found in many other places.

To summarise, to be novel, a comparison study should either study very recent methods that have not been subject to comparisons so far, or (and) go into a level of depth that uncovers new conclusions about the methods in question. While the authors made an attempt to look at different aspects performance, the conclusions they draw show that no real novelty has been produced here. Therefore I must recommend a rejection.

I would encourage the authors to think carefully about the added value of their paper to other researchers, and use that as guide for how to improve the paper, perhaps by incorporating more advanced techniques or really drilling down to the differences between the methods in considerable detail.

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