Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-691-RC3, 2018

Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Incremental model breakdown to assess the multi-hypotheses problem" *by* Florian U. Jehn et al.

Anonymous Referee #3

Received and published: 12 January 2018

The article presents an incremental model breakdown approach to determine an optimal hydrological model structure for rainfall-runoff modeling. The hypothesis of the authors is that one should start from a model structure that includes all possible processes and that this structure should then be incrementally simplified by successively removing the unimportant processes, i.e. those for which the model performance is not degraded or even improved when they are removed from the structure. The approach is demonstrated on a catchment in Germany.

Though the approach is interesting, I have several concerns about the way it is applied and demonstrated:

- I think that the "one-at-a-time sensitivity analysis" approach that is applied makes the hypothesis that all processes are independent from each other in the model structure.

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However, this is probably not the case and it is most likely that there are interactions and compensations between model components. Therefore, I find it is difficult to conclude on the individual value of each component based on these tests only. There is no guarantee that the model structure selected at the end is optimal, since only a very limited number of structures among all the possible ones have been tested. It is likely that there are many options which are close to each other in terms of performance.

- The parameter sampling approach, drawing 300,000 parameter sets for each structure, makes that the parameter space will be much more densely scrutinized in the case of a model with 10 parameters than in the case of a model with 19 parameters. This means that the chance of getting behavioral parameter sets is much more limited in the second case than in the first case. This may induce a bias in the way the models are compared when using the GLUE approach. This should at least be discussed or ideally further tested.

- The way the structures versions are selected is unclear. Is this based on results in calibration or in validation? Actually these two options should be tested and discussed. Furthermore, how a model structure is judged to be significantly better than another? Is there any threshold in model improvement or statistical test associated?

- The robustness of the structure selection should be discussed. The model structure is selected based on the use of the first period as calibration and the second as validation. I think the authors should at least test the procedure by inverting the role of the two periods. It is likely that the structure selection may end up (maybe not on this catchment but there are probably cases where it may happen) with different model structures in the two cases. This raises the problem of equifinality in the choice of model structures, and may be a limit of the proposed approach. The selected structure may be over-specialized for the selection period and not really transposable on periods with other conditions. This is what can be observed in the case of model parameters and it is probably also the case in terms of structures. This is probably even a larger problem for periods with much contrasted characteristics.

- The authors did not really discuss the respective roles of structural and parametric complexity in the results. At the end, they have a much more simple structure than at the beginning... but which still has ten parameters, which may appear as overparameterized at the daily time step. It may be interesting to have even more simple model structures, to see how the further simplification possibly leads to degradation in the modeling.

- The authors criticize the usual approach which takes existing models, with interesting arguments. To further demonstrate the value of their approach compared to the classical one, they could test an existing model (e.g. HBV or another model of this type) as a benchmark, to explain the added value of their approach compared to the case when one simply take an existing model.

- Last, I find that making the test on at least a second catchment with contrasted characteristics may strengthen the conclusions. Here the results may be obtained only by chance. There is no guarantee that the results are general outside this case study.

I also have other comments detailed below. In summary, I think there is valuable material in the article, but that the methodology should be further tested and more thoroughly evaluated to provide a more convincing demonstration of its usefulness. I suggest major revision.

Detailed comments

1. P2,L28: This is probably true for all modeling approaches!

2. Section 2.1: Say in which country the basin is located. Maybe a location map could be added. Is catchment size actually 2.977 or 2,977 km^2?

3. P4,L10: I find that the definition of a process in the structure should be given. When a process is removed, what happens in the connections in the structure, especially when there are several branches coming to/departing from this process?

4. P6,L18-19: As mentioned in the major comments above, I think that it should be

explained how a version is considered to be significantly better or worse than another.

5. P9,L3-8: Why there is not a pure time-delay parameter (possibly non integer) in the model that would be added in the model structure to account for this time shift and to make it more generally applicable?

6. P9,L12-14: Please remind in brackets for each criterion the optimal value and range of variations, to avoid misunderstanding in the interpretation of results for readers not fully familiar with these criteria.

7. Table 3: Please add a column for units. Maybe also add a column to remind in which structural element (as defined in Table 1) each parameter is included. In the caption: "all model parameters"

8. P11,L11-12: Is not that expected by construction that all model structures have less parameters than the original one?

9. P16,L8-15: This seems to repeat the last paragraph of the previous page.

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