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Interactive comment

Interactive comment on "A new fractal-theory-based criterion for hydrological model calibration" *by* Zhixu Bai et al.

Anonymous Referee #1

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GENERAL

The authors proposed a calibration strategy in terms of Hausdorff dimension and Nash-Sutcliffe Efficiency (E) (Nash and Sutcliffe, 1970). The concept that the fractal dimension of observed streamflow should be comparable with that of the model output is an important consideration for hydrological modelling. In Equation (2) of the discussion paper, the authors suggested the metric RD as the ratio of Ds to Do (where Do and Ds denote the Hausdorff dimension of observed and simulated flow, respectively). The calibration strategy the authors are proposing is in terms of E-RD. The authors applied the E-RD strategy to calibrate a selected rainfall-runoff model in applied to a number of catchments in China. In its present form, the manuscript has a number of major areas of concern especially with the use of the proposed E-RD strategy.

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Discussion paper

COMMENT 1

The metric E which the authors are using in their strategy is known to have a number of issues in its application for assessing "goodness-of-fits". Eventually, the need to modify E has been on the radar of hydrologist for decades. In other words, several variants of E exist to address the issues related to the use and interpretation of the original version from Nash and Sutcliffe (1970) which is still widely applied in hydrology. The question to answer is: why did the authors adopt the original version of E but not any of the existing variants?

COMMENT 2

RD varies from zero to positive infinity (see line 155 of the discussion paper). However, E varies from negative infinity to zero. The point is that both E and RD are relative error measures. For relative error measure, we focus on the "standard" range in which values vary from zero and one with association to imperfect and perfect model, respectively. Therefore, how can a modeler interpret E and RD in a combined way yet the range of the values from each of these metrics is wider than the "standard" one?

COMMENT 3

There is a possibility in modelling that the larger the number of calibration runs, the better the value of the objective function (especially if the parameter spaces are not small). However, the modeler needs to compute both E and RD in each calibration run as a requirement for the strategy being introduced. Thus, application of the introduced strategy brings about the problem of computation time required to reach optimum during calibration of a hydrological model. How can this problem be addressed to ensure application of the introduced strategy is not at the expense of calibration time (especially if the modeler is making use of long-term hydrological series)?

COMMENT 4

The best RD does not guarantee that E will be at its highest value. Furthermore, E reduces as the modeler searches for the best RD (see lines 330-331 of the discussion paper). This brings about (i) the issue of subjectivity in determining which values of E and RD should be used to select the set of optimal model parameters, (ii) the complication in dealing with the trade-off regarding the decision on which study objective should be preferred to others. To explain (ii), the authors need to note that a modeler may be aiming at reproducing extreme hydrological extremes especially peak high flows, and low flows. Applying the E-RD strategy means, the modeler should also aim at ensuring Ds and Do are the same or very close to one another. The question that the authors need to answer is: How can a modeler deal with the issues (i) and (ii) in application of the calibration strategy being introduced?

COMMENT 5

Sub-flows' separation procedure adopted for this study (incorporated in the tool named WESTPRO) makes use of a number of parameters. The authors never mentioned any values of such parameter in their discussion paper. Examples of such parameters (among others) include recession constants, and the filter parameter. At least two parameter values are required to extract base flow. Again, not less than two parameters are required to filter interflow. Thus, for each river flow time series one requires not less than four parameters to obtain the various sub-flows. The problem is that the choice of this parameters can be largely subjective (even if one takes into account his or her expert judgment in deciding on the parameter values to use for sub-flow filtering of a given streamflow). Moreover, sets of parameters required to separate sub-flows vary from one catchment to another. Finally, there are several methods available for separation of flows (what we also call the baseflow separation techniques). All these problems compound the challenge of using E-RD to judge model performance (or select which calibration run is the best). Furthermore, the overall problems that the authors need to take into account, here, are with respect to the uncertainty (i) due to the choice of the baseflow separation technique (whether manual approach as the authors adopted or automated technique), (ii) the subjectivity of selecting which

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parameter values to use in filtering the observed and modeled streamflow. Here, the fact that the same set of parameter values are required to be applied to both observed and modeled streamflows should be considered basic and they need to go beyond it in addressing this comment. Finally, given the above background on sub-flow separation and unanswered question, statements made by the authors in the manuscript citing that the use of RD improves simulations of sub-flows remain claims (or are vague) unless they prove otherwise.

COMMENT 6

The authors attempted to relate optimal values of the model parameters to obtained RD's. In a number of cases (see, for instance, lines 436 and 505) the authors pointed that the selected model lacked capacity to simulate certain hydrological processes. The question to answer is: Why did the authors not take into account the uncertainty in their results due model selection? Models differ with respect to their structures (or underlying assumption and equations). It becomes imperative that the authors need to select at least two models and apply them to various catchments. In doing so, I suggest the authors focus on clear objectives of modeling so as to allow them comprehensively judge the influence of application of RD on the model results. Such objectives may include reproducing (i) extreme peak high flows, (ii) low flows, (iii) fractality in the observed streamflow. Furthermore, results on comparison of RD with model parameter should be put as supplementary material (if they cannot be discarded from the manuscript).

COMMENT 7

Instead of only selecting catchments from China, the authors need to take into account the influence from the differences in climatic conditions on the use of the E-RD strategy. This is because the difficulty in reproducing fractality in observed streamflow from catchments selected across various climatic regions may not be comparable. Furthermore, to guard against manipulations of model inputs, the catchments should be selected in such a way that their datasets for modeling should be from sources which readers can easily access. There are a number of catchments with complete information such as, hydro-meteorological data, which can be used for rainfall-runoff modelling. To mention, but one example, is the Rainfall-Runoff Library data which can be obtained via https://toolkit.ewater.org.au/Tools/RRL (accessed: 8th December, 2020).

MINOR COMMENTS

Line 9: Change “aims to investigate” to “aims at investigating”

Short forms or words should be removed from the manuscript. A few examples of such words written in short forms include "doesn’t" (lines 90,301), "won’t" (line 260), and "don’t" (line 495).

Line 285: Change “William” to “Willems”

REFERENCE

Nash, J. E., and Sutcliffe, J. V. River flow forecasting through conceptual models part I - a discussion of principles, J. Hydrol., 10, 282–290, doi:10.1016/0022-1694(70)90255-6, 1970.

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