

Interactive comment on “A nonlinear modelling-based high-order response surface method for predicting monthly pan evaporations” by Behrooz Keshtegar and Ozgur Kisi

Anonymous Referee #2

Received and published: 26 July 2016

This manuscript is just application-based paper, the proposed methodology is more or less known and I do not see any novelty on that. The authors compare different methods (the proposed one with the previous methods, in fact, all are known methods) of very different nature. I expect to see deep discussions on why there is a difference on the results achieved for each proposed methods to reflect the nature of each method mathematical procedure and the nature of the hydrological parameter under the study (evaporation), unfortunately, the paper is lacking of that and did not present the link between the methods and the hydrological science which is the core of the journal (the paper is lacking for the hydrological sound). Even the proposed performance indicators to evaluate the models are very well known and actually does not really add or show

[Printer-friendly version](#)

[Discussion paper](#)



the advantages of the proposed method as the results show nearly values of those indicators. I should reject the manuscript in its current shape to be published in HESS as basic hydrological journal and the manuscript is more suitable to be submitted to journals with focus on soft computing or computational research. However, I can give few comments to improve the position of the manuscript. 1- To provide the reasons for proposing this method and its suitability on the hydrological application (in this study is the prediction of the evaporation). My point of view is this is the main point that the authors has to focus on, and has to demonstrate that in all sections of the manuscript. 2- Details on how the proposed method has been adjusted with the hydrological application (prediction of evaporation) have to be provided, 3- One of the major target of this study is to introduce a comparison with existing prediction models applied for the same case study, developed by the authors, but they did not provide any information how those models were developed in terms of their structure, to compare the fashion of these models as well. 4- Deep discussions on the differences of achieving results due to the nature of each method fundamental with illustration its sound on the hydrological science which is the major interest of HESS readers 5- The authors should show how significant is the improvements achieved using the proposed model over the existing one. Specific Comments; 1- Page 2, I expect to see why the author proposed their model over the existing models for the study area 2- Page 2 line 19 and 20; What you mean by SLIGHTLY, How far it is?, I expect to see some results indicator in the abstract. 3- Page 3 line 56 and 57, the existing FG model offers superior results for the same study area, then what is the need for offering new model 4- Sudden jump to report the objective of the study without showing any deep discussion on the existing model and the reason for a need to have new model. . . the weakness of the existing model and the ability of the new one, especially and one of the objective is to compare the proposed model with the existing one 5- Page 6 line 124 to 129, this paragraph is very shallow and does not mean scientific information for the reader, and the claim that the HOR produces better results is not really understandable here. 6- I expect to see the real structure of the proposed model and how it adjusted to be suitable for the

application, but it is missing. Such information will help the readers to see the applicability of the proposed model in other hydrological applications. 7- A brief on how the existing model for the same study area should be presented here to be able to view the difference. 8- Reporting of the case study is very poor, no figures to show the raw data used in the model,,, to give a chance for readers to see how the pattern of the data 9- Page 7 line 154 to Page 8 line 159, based on what the splitting of the data in this way, is there any justifications for that. What is the difference between the testing and validation parts, why we should do that. . . which one should come first, testing before validation or validation before the testing. . . 10- This section is really very poor, does not show any hydrological discussion and or analytical view for the numbers showed whether in the tables or figures. Generally, for such research for developing prediction model, there are few type of figures should be presented such as, observed-actual and relative error figures but they are not demonstrated, therefore, this reviewer consider that the analysis of the results are not completed yet. In addition, there are plenty of grammar errors and misspelling everywhere which make the paper difficult to follow. In addition, there are a lot of plagiarism spots with previous published manuscripts for the authors. Due to the above reasons, I recommend to reject the manuscript and not suitable for publication in HESS. I hope the authors find the following specific comments useful for improving the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-191, 2016.

HESSD

Interactive
comment

Printer-friendly version

Discussion paper

