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Interactive comment on "Copula and ARMA based study of controlled outflow at Farakka barrage" *by* Uttam Singh et al.

Anonymous Referee #1

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The paper describes the application of Copula and ARIMA models to the estimation of outflow discharges from a reservoir. They choose from different Copula models. Alternative models are compared through standard performance indices, such as Mean Square Error (MSE), Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC). They found that Frank Copula performed better than the rest. Estimation of outflow discharges from a reservoir is a relevant topic for the audience of HESS. However, I was not able to identify any relevant contribution in this work. I am afraid the manuscript should be much improved before it seeks publication in HESS.

One obvious drawback of the manuscript is its poor English and lack of attention to details that convey the impression of unfinished work. Although I am not a native English speaker I noted a significant number of grammar errors and incomplete and/or

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Discussion paper

meaningless sentences which are totally unacceptable for a scientific publication in an international journal. The authors should have been more careful while drafting the manuscript and they should have requested the help of a fluent English speaker to correct all these errors before submission. Furthermore, there are also aspects of the paper presentation and organization that suggest that the authors submitted an unfinished draft. Tables and illustrations have not been properly prepared. They lack suitable captions and the reader is unable to figure out what is being presented.

In addition to those formal issues, I have two major objections to the way the work is presented.

My first objection is related to how the research problem is formulated. Apparently, the Copula and ARIMA models have been applied to an existing dataset with the only purpose of fitting a model to the data. In my opinion, this is not a correct focus. The reason to apply any model is to solve a specific problem through its predictive capabilities. In the manuscript, the models are calibrated (the parameters are estimated) for a certain set of available data (several years of outflow from the Farakka reservoir) and then they are tested for a different set of available data. This is standard practice in data fitting. However, the authors do not provide any indication of why they are fitting the data and which problem they are intending to solve. We only know that the reservoir is located in the Ganga river (this information is only provided on the abstract). We do not know reservoir or basin size, mean annual inflow, the purpose of reservoir operation. . .We do not know why modelling is attempted. We do not even know their modelling scheme. Are outflows estimated from inflows or from outflows the previous months? Without this information we cannot possibly decide whether the modelling exercise was successful or not. The fact that the data set covers the period 1949-1973 is really surprising. Why is the dataset so old? Why was this particular reservoir chosen for analysis if it has no new data since 1973? I am afraid the analysis performed, as presented in the paper, is of little interest to the hydrologist.

My second objection regards the way the work is presented. The authors do not pro-

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vide enough information to understand the logic of the successive steps. Figures and tables convey the idea of a brute-force data fitting exercise where all alternatives are analysed with the help of a statistical package and the final decision is made on the set of selected performance indices. There is no discussion whatsoever on modelling strategy or on results. For instance, on table 2 the authors apparently select the Generalized Extreme Value for the marginal distribution of pre-monsoon and post-monsoon discharges. However, they are dealing with monthly values and it is surprising that the best fit is obtained for an extreme value distribution. There is no discussion of this surprising result. The Copula modelling is even more difficult to follow, since they are applying a bivariate modelling scheme with only one variable: outflow discharge. How is this done? Which two variables are included in the joint distribution? From lines 155-156 and the axis legends of Figure 9, I gather that they might be discharges in the pre-monsoon (December to May) and post monsoon (June to November) seasons, but how is this done? These two series are alternated, not simultaneous. Is there a rationale to assume a dependence structure? How are the two values of different months coupled? What sense does it make? What would be the use of modelling such distribution? The ARIMA modelling is also extremely difficult to follow, with very long tables with a lot of data and little discussion. The same applies to the Results and Discussion section. There are several figures with and extremely short caption and the text is not informative on what is being shown and what can be inferred from it. Finally, I find the conclusions of the paper unclear and not very useful.

As it is presented in the paper, the analysis carried out is only a data fitting exercise with little value added to the hydrologist. Sadly, I recommend declining this manuscript. My final suggestion to the authors is to rewrite and resubmit the paper from the hydrological perspective focusing on the issues mentioned above or to seek publication in an appropriate statistics journal.

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