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Interactive comment on "A Comparison of the Discrete Cosine and Wavelet Transforms for Hydrologic Model Input Data Reduction" *by* Ashley Wright et al.

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The authors would like to thank the reviewer for their comments and questions. The remarks made by the Reviewer are written in italics, and the replies in normal font.
OVERVIEW The study investigates the use of Discrete Cosine and Wavelet transforms for the reduction of input data dimensionality in hydrological modelling. GENERAL COMMENTS I am reviewing the paper after reading the comments raised by previous reviewers on the interactive discussion. As specific comments were already given by previous reviewers, I included here only my general comments for the paper. The paper topic seems to be relevant for the HESS readerships. However, I found some important issues that need to be addressed before the publication.

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Discussion paper

1) It is not clear to me how the DCT and DWT methods are applied. If I well understood, for each basin the authors used streamflow and precipitation data, together with a hydrological model, for applying DCT (and DWT) and thus reducing the dimensionality of precipitation data. However, no hydrological model is mentioned in the paper. What are the input and the output data? Is a hydrological model used? Can the procedure be applied in a testing period? What is the targeted application of the proposed approach (something is mentioned in the introduction, but needs clarifications)? All these questions need to be addressed. Otherwise, I have not clear why the study is relevant for the hydrological community.

We would like to clarify that the authors have only used rainfall data, there is no mention of streamflow data in the Data Set section.

The input data is rainfall and the output data is rainfall represented by a smaller number of parameters than the number of rainfall observations.

No hydrological model is used and there is no reference to one in the Experiment Design section. The definition of testing period is unclear. Is it possible to clarify this?

The targeted approach has been outlined in the Introduction and Model Input Data Reduction Theory sections. In summary, the reduction of model input data allows input data such as rainfall to be reduced to a small number of parameters. Using modern parameter estimation algorithms, the representation of rainfall as parameters allows for the uncertainty in input data to be explored.

If there are specific lines that are unclear, we would like to ask the reviewer to make these known such that modifications can be made to the manuscript.

The authors acknowledge that some ambiguity may have arisen due to a typo in which peak streamflow error was mentioned. We apologize for this and would like to clarify that it is peak rainfall error.

2) An important issue in the analysis of rainfall time series is related to the zeros, i.e., days with no rainfall. By looking at the results, good performances are obtained for POP values larger than 30-40

The analysis of no rainfall days is indeed important when analysing rainfall time series.

As a larger Percentage of Original Parameters (POP) is used, it is expected that the reduced rainfall product correctly represents more days in which no rainfall was observed. Obtaining the best possible representation of rainfall using a minimal number of parameters is a primary concern for model input data reduction. Consequently, it is the reduced rainfall products that have a POP of 40% or less that are of interest to this study.

3) Besides the performance metrics related to precipitation, also the peak discharge error is mentioned. However, it is not clear how it is computed (see also comment 1). If a hydrological model is used, it should be mentioned. I expect that results depend also on the quality and reliability of discharge time series. If yes, it should be investigated and discussed. All these information are totally missing in the current version of the paper and should be added.

As was mentioned earlier, the authors made a typo when referring to peak streamflow error. We apologise for any confusion caused. The results shown do not depend on the discharge time series.

4) Some parts of the paper seem to be written quickly without much attention. Therefore, typos and grammatical errors are present. I suggest a detailed review of the whole Discussion paper text, and of the figures (e.g., y-axis labels in Figure 2 are wrong).

We will conduct a detailed review of the entire paper. Figure 2 will now be amended.

RECCOMMENDATION On this basis, I found the topic of the paper relevant, but as I mentioned above, the analysis and the text need major revisions before the possible publication on Hydrology and Earth System Sciences.

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