

# ***Interactive comment on “Hydrological recurrence as a measure for large river basin classification and process understanding” by R. Fernandez and T. Sayama***

## **Anonymous Referee #1**

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Comment on Hydrological recurrence as a measure for large river basin classification and process understanding by R. Fernandez and T. Sayama

This is potentially an interesting paper, which uses modeled monthly time series of several water balance variables on a monthly time step to classify large river basins around the world.

The paper uses something called “recurrence” as a quantitative measure of catchment similarity. Recurrence is estimated on the basis of an autocorrelation function, fast fourier transforms, and something called the Colwell’s Index.

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Overall, it is an interesting idea and I would like the paper to be eventually published in HESS.

However, I have several concerns about the word “recurrence”:

1) Firstly, the authors use the word “recurrence”: what is recurrence? There is no clear definition of recurrence in a hydrological sense, even if they use these statistical measures.

2) In several of the papers they cite, catchments are classified on the basis of their regime behavior, which is defined as the mean seasonal (this can be daily or monthly) water balance. How is recurrence different from these? Does it measure something different or something more than mean seasonal behavior? Why is this important to become a measure of a large river basin classification system?

3) The authors may want to include a schematic diagram to illustrate river basins with high recurrence and low recurrence, so the reader is clear on what they are talking about.

4) Without such guidance, I am unable to interpret the results in Figure 3, which claims to present the recurrence of precipitation, storage, evaporation and runoff.

5) Also on a statistical issue, isn't it true that in the presence of strong seasonality, the autocorrelation function is strongly affected by the seasonality, and I am not sure that in these cases the regime curve is just as well sufficient to describe the time sequence of the hydrological variables.

6) I hope the authors are aware of a classical (in Europe especially) approach to characterizing seasonal water balances (including storage, however estimated). It is called the Wundt Diagram – this is presented in the seasonal prediction chapter of the PUB Synthesis Book (Bloeschl et al., 2013), and results from some of the large river basins this paper is also studying – they may want to refer to this if they find it appropriate.

7) Likewise there has been a recent paper in WRR on the classification of the MOPEX

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basins in the USA on the basis of the Wundt Diagram: Berghuijs et al., 2014: Patterns of similarity of seasonal water balance: A window into streamflow variability over a range of timescales. *Water Resources Research*, 50, doi:10.1002/2014WR015692.

Overall my major concern is that statistical measures of “persistence” (e.g., autocorrelation function) are being used to characterize an essentially deterministic aspect of variability (i.e., seasonality). The authors have not convinced me that these metrics measure something more than seasonality (which is already present in many published studies), and that these statistical persistence measures are important enough hydrologically to be the basis for a classification. They may be right, if so, they have to convince the reader through additional explanations and results.

The paper may have to undergo moderate (perhaps not major) revisions to address these concerns. It depends on their response.

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Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 11, 8191, 2014.

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