Review report for Yin et al, HESS-2018-601

This paper aims to investigate the potential of time-series decomposition approach for exploring variability in hydro-climatic time series and the possibility of generating independent components (i.e. a zero covariance between the three components). To achieve this, the paper analysed the potential of using a two-way ANOVA based decomposition approach in comparison to the conventional approach based on linear trend removal and using moving average based trend removal.

I think paper presented an interesting application of the time series decomposition approach in pattern recognition and the contribution made by the paper could be of interest to the wider hydrologic community. However, the paper needs significant improvements, specifically, the author should consider addressing some major issues specified below to improve the overall quality of their paper:

Literature review: A range of time-series decomposition approaches has been proposed recently and these approaches have been applied to a wide area of application. Literature review/background information presented in the paper is considerably limited. I think the author of the paper should have discussed key work done in the development of time-series decomposition approaches, potential areas of application, and specifically in the context of hydrological applications.

Novelty: This paper will also benefit if the author further contextualises the overall purpose of the paper to the state-of-the-art to clearly state/identify the novel contribution of this work.

Methodology: I think that the description provided for the methodological approaches is not detailed enough to access their overall appropriateness for the present work. For example in section 4.1 First paragraph – "On this approach … equal to the remainder", it is not clear if author subtracted mean of long time-series from monthly mean or annual mean of long time-series is used to estimate monthly anomaly. How they estimated seasonal and random components, just stating that "random component is set equal to the remainder" could be misleading, until they clarify how monthly means have been used in estimating seasonal components (are they using any differencing approach here for estimation of seasonal components or they are just using monthly mean). I think the author should provide mathematical expressions to clarify their procedure.

Similarly, methodologies presented in section 4.2 and 4.3 can be updated to improve technical representation of the paper.

Finally, I think methodologies are not critically discussed to justify their appropriateness in this paper. For example: Section 4.2 line 110-111 "In general, … other period". No explanation/justification is provided for selection of 24 month period for the proposed method and what could be the potential impacts/advantage/disadvantage of using any other larger/smaller time periods.

Section 5 starts with the sentence "On further investigation …" and made a concluding statement on two-way analysis of variance (ANOVA) model facilitating three independent components (line 129). It is not clear if the author conducted any sort of pre-investigation before reaching to these conclusions, what other approaches if they investigated, what leads them to the selection of ANOVA

model proposed here, any theoretical/technical aspect of ANOVA model that could have resulted in delivering three independent components. As said before mathematical representation of procedure should be provided to enhance the overall quality of the paper.

Results: Results and outcome of the various decomposition approaches are minimally discussed; moreover discussion provided is not technical and is mainly based on general observations only. For example: In Section 4.1 Why the individual covariances are not all zero (line 104). Similarly, in Section 4.2 line 117, the author concluded "the moving average … intended purpose". They could explain technical reasons for why they observed larger covariance for Pa and Pm than variance for Pa. Further, what could be possible impact of using moving average; what are the strengths and what are the weaknesses?

Other: Section 6 – adequate details should be provided on the various characteristic of global land precipitation database, which are used to demonstrate the application of ANOVA based decomposition model (e.g. what is the structure of the database, what is the temporal/spatial resolution, etc.). Section 7 – (as said before) Results are not critically discussed; I would like to see some critical discussion on the theoretical/technical aspect of the outcomes. It would be good if the author focuses a bit on the novelty aspect of their work and also on what is the contribution of their work.

Appendix A.1: I think this section is useful and the author presented a clear mathematical proof to demonstrate all the three components are independent. Some general points –

Eq. A13 and A14 - Why there is the same expression for each year in the right-hand side of the equation. The author can provide a brief explanation for this to support non-mathematical experts.

Eq. A19 - Why is not equal to 0.

Eq. A24 - I think author missed to put bracket for term $\overline{\left(U_a(l) - \overline{P(t)}\right)}$

Appendix A.2: I think this analysis is not relevant to this paper or please provide a strong explanation to support, in particular, what could be potential significance/application of possibility of expressing the variance of the random component as the sum of the variance calculated for individual months. How do these findings are related to the autocorrelation properties of the random component, is it 0 for all lags, which implies that the remainder is a purely stochastic process, i.e. white noise process. What could be the significance of having the random component as a white noise process?