Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-184-RC1, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Changing patterns of extreme water levels in urbanizing plain river network region of Taihu Basin, China: characteristics and causes" *by* Y. Wang et al.

Anonymous Referee #1

Received and published: 27 May 2016

The manuscript studied the temporal trend and change point of the annual maximum and minimum water level in Taihu Basin, China. The studies used an interesting high-quality data sets which is the daily water level datasets from 1960 to 2012 at 8 stations in the basin. Despite this interesting data set, the case study considered in the manuscript is very basic, and scientific contribution looks limited. The paper also suffers from serious flaws on the methodology and some conclusions are not supported by evidence. In my opinion, the current manuscript does not meet the standard of HESS, which still requires substantial improvement.

- Page 3, line 30. The thiessen polygons method (Jones and Hulme, 1996) was used to calculate regional extreme water level series. However, these kinds of method are

usually not theoretically justified for extreme data sets (e.g. annual maximum or annual minimum), because extreme data are usually highly skewed. Thus an acceptable way to regionalizing the extreme data set is to firstly regionalizing the daily water level time series at different locations, then calculate the maximum or minimum. The reverse of this procedure is not correct.

However, owning such a high-quality data set, I suggest to consider some regional models to study at-site extreme data, rather than regionalizing to one time series, in which huge amount of information can be lost during the process of regionalization. Focusing directly the at-site data could provide a better understand on how the extreme water level changes at each site due to climate and human activities. I think it could be interesting to consider some extreme values distribution to quantify such risks. However, just reporting the existence of trend is not enough. Authors could consider some of the following references for the regional approaches on extreme events.

Leclerc, M. and T. B. M. J. Ouarda (2007). "Non-stationary regional flood frequency analysis at ungauged sites." Journal of Hydrology 343(3-4): 254-265.

Maraun, D., et al. (2011). "The influence of synoptic airflow on UK daily precipitation extremes. Part I: Observed spatio-temporal relationships." Climate Dynamics 36(1-2): 261-275.

Sun, X., et al. (2014). "A general regional frequency analysis framework for quantifying local-scale climate effects: A case study of ENSO effects on Southeast Queensland rainfall." Journal of Hydrology 512(0): 53-68.

Chen, X., et al. (2014). "Climate information based streamflow and rainfall forecasts for Huai River basin using hierarchical Bayesian modeling." Hydrology And Earth System Sciences 18(4): 1539-1548.

Steinschneider, S. and U. Lall (2015). "A hierarchical Bayesian regional model for non-stationary precipitation extremes in Northern California conditioned on tropical moisture exports." Water Resources Research 51(3): 1472-1492.

- A follow up question is that in page 9 line 12 authors mentioned heterogeneity of rainfall in Taihu Basin (Deng et al., 2014). It is not clear why regionalizing the extreme water level data in this heterogeneous region is appropriate, because regionalizing data usually requires to be in the homogeneous region.

- Page 8, line 17, the precipitation is not well defined (Monthly? Daily?) Beta and z are not defined in Table 3. Authors attempt to use rainfall as a main predictor to estimate the contribution of climate on the maximum and minimum water level. However, selecting the rainfall at different periods (e.g. summer rainfall, annual rainfall) or different events (e.g. average rainfall, n-days cumulative rainfall and daily maximum rainfall) could lead to a very different result. Thus it is not rigorous to select the predictors arbitrarily.

- Page 8, line 21-30. I disagree that the authors account for all those variation that could not be explained by rainfall as human activities. To account for the impact of human activities, authors need to conduct a quantitative investigation on each human activity, such as land use, and assess their impacts. Besides the climate and human activities, there is also a portion of variation (an error term or residual) which could not be captured by either of these aspects. Thus the methods of quantifying the impact of human activities is problematic.

Furthermore, just using rainfall to account for climate factors is also problematic. A linear regression based on rainfall (it is not clear which kinds of rainfall is used here) was used to estimate the annual minimum and maximum water level. However, Fig 7 showed that the correlation between the rainfall and water level is quite weak after 1989. Thus “rainfall” used here is certainly not a good predictor. As a consequence, just using rainfall to calculate H_HP is not appropriate. Calculating H_hp should accounts for all climate factors besides rainfall, such as evaporation.

Additional comments:

- Page 2, Line 23, what is km/km2?

- Page 6, lines 20. Curve of statistics UF (and also UB) have never been defined, whereas UF is sometimes used as the normalized variables (removing the mean and divide by the standard deviation). UB is similar to UF, but calculated using the reversed data. This definition needs to be clearly stated in the manuscript.

- Page 6, line 29, why obvious?

- Page 7, line 5. How to support this judgement? References are required.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-184, 2016.