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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The influence of global climate and local hydrological variations over streamflow extremes: The tropical-mountain case" *by* Juan Contreras et al.

Anonymous Referee #2

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First, I have to say that I am attracted by the title of the manuscript before accepting to review this manuscript. However, after I carefully went through the manuscript, it is definitely not what I thought that focus on the physical influence of global and local drivers to streamflow extremes, so I think the title should be more specific on the basins or some more related to regional studies.

This manuscript uses the GAMLSS model to analyzed the nonstationarity of streamflow extremes over two stations. Frankly speaking, both the method and nonstationarity related to the large-scale climate variability are very common for many previous studies. I personally used GAMLSS model to study the nonstationarity of Canadian floods with

more than 100 stations (Tan et al., 2015). This paper focuses on only two stations and examined only statistical relations between streamflow extremes and climate indices. As I understanding, the relations detected might be only statistical but without any physical reasons, therefore, I think the authors should be more looking at some physical mechanisms. Therefore, I suggest the authors make substantial improvements on the way to be publication. The following are some commentsïijŇ

(1) The authors used too many climate indices. Since many climate indices used have strong correlations, so I think it is not necessary to use a variety of climate indices, without previous selection based on the physical relations between global climate and region hydrology. Again, some statistical relations can only be statistical, but no real meaning to promote understanding of teleconnections and predictability of regional hydrology. (2) The treatment of change points in statistical analyses. The authors detected change points for both time series of streamflow extremes over two basins. Whether the change points are due to the nonlinear relation between climate indices and streamflow extremes? The nonlinear relations are very common in teleconnections, even though I do not know this exists in South America and large scale climate variability, but it quite is evident in Australia, e.g. Cai et al., 2012 and 2013. So how do the authors consider the change points in GAMLSS analyses? (3)This study only examined two time series, which make me thought that the study should more focus on the physical teleconnection but not statistical relations, because of the limited samples. (4)To make the study more attractive, the manuscript should point out the novelty of GAMLSS analyses. Currently I do not see new points. (5)The authors implemented precipitation information to GAMLSS model. Here, I think the authors should specify the purpose of using precipitation information to predict streamflow extremes. The relations between precipitation and streamflow is quite straightforward and there is no need to use GAMLSS model to find this relation. Moreover, because the precipitation and streamflow are generally not lag-correlated but changes simultaneously on time scales larger than monthly, so preciptation do not provide any predictability to streamflow extremes, even though the relations can be found by some statistical analyses.

Minor comments Line 80: both hydrological extremes? I think should be extremely low and high streamflow? There is should be Is there? Line 265: they were not significant.

Cai, W., and P. van Rensch, 2013: Austral Summer Teleconnections of Indo-Pacific Variability: Their Nonlinearity and Impacts on Australian Climate. Journal of Climate, 26, 2796-2810. Cai, W., P. van Rensch, T. Cowan, and H. H. Hendon, 2012: An Asymmetry in the IOD and ENSO Teleconnection Pathway and Its Impact on Australian Climate. Journal of Climate, 25, 6318-6329. Tan, X., and T. Y. Gan, 2015: Nonstationary analysis of annual maximum streamflow of Canada. Journal of Climate, 28, 1788-1805.

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