

Interactive comment on “Annual Characterization of Regional Hydrological Drought using Auxiliary Information under Global Warming Scenario” by Zulfiqar Ali et al.

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GENERAL MAJOR COMMENTS The paper claims to present a hydrological drought index, which uses improved monthly precipitation estimates under global warming scenario. The monthly station precipitation timeseries are improved using local weights utilizing regression equations between precipitation and temperature. Temperature is treated as an auxiliary variable. The paper proposes a new drought index, the Locally Weighted Standardized Precipitation Index (LWSDI) for drought assessment. LWSDI is applied in ten stations across Pakistan for the period 1970-2017 and LWSDI timeseries are compared at 12-month time scale with the commonly used and well known drought

C1

indices SPI and SPEI. Major Comments There are many points that should be clarified before considering the paper for publication. 1. The title, abstract and the core of the paper claim that the LWSDI is a hydrological drought index, which is not. However, the time scale of 12-month may suggest that this is a hydrological/water resources drought index. There are many papers that have identify the importance of drought indices time scale in characterizing meteorological and/or hydrological droughts, and the authors should refer to them. Authors Response: We agree with the reviewer. In literature, there is too much confusion between the time scales and the definitions of drought such as meteorological, agricultural and hydrological (Bazrafshan et al., 2014). However, many authors have considered that twelve-month time scale is suitable for defining hydrological drought (Svoboda et al., 2012). In the present work, a statistical methodology is adopted to improve time series data of precipitation. On the same manners of SPI, the proposed data is further used to compute drought indices. The output of the proposed method of data is compatible to compare SPI-12 and SPEI-12. Therefore, we called LWSDI as a hydrological Index, because it gives monthly numerical standardized values at 12-month time scale. References: Bazrafshan, J., Hejabi, S., & Rahimi, J. (2014). Drought monitoring using the multivariate standardized precipitation index (MSPI). *Water resources management*, 28(4), 1045-1060. Svoboda, M., Hayes, M., & Wood, D. (2012). *Standardized precipitation index user guide*. World Meteorological Organization Geneva, Switzerland. 2. The title of the paper (and a few sentences in the Abstract and Introduction) is not appropriate and it does not reflect the research presented. The LWSDI is not a hydrological drought index and the paper does not use a Global Warming Scenario. Authors Response: The revised version of the title will be more appropriate and representative. The prospective title is as follows: “Characterization of Regional Hydrological Drought using Improved Precipitation Records under Auxiliary Information”

3. The various climatic regions of Pakistan should be presented in the “Study Area” section of the paper, since it is claimed that the 10 stations used are representative of these climate regions. Authors Response: In the revised version of the manuscript,

C2

we will cite the article of Qasim et al., (2014). This article assesses and provides statistical evidence regarding variation in all parts of the country. In addition, it provides zonal classified map and statistics which are indicating variation among each zone. References: Qasim, M., Khilaid, S., & Shams, D. F. (2014). Spatiotemporal variations and trends in minimum and maximum temperatures of Pakistan. *J Appl Environ Biol Sci*, 4(8S), 85-93. 4. It seems that the proposed index is essentially the SPI using locally weighted precipitation. Thus, it is expected that the proposed index LWSDI to compare well with SPI. I suggest that the authors try to analyze specific common and extreme drought events using LWSDI, SPI, SPEI, derive the drought parameters (i.e. drought duration, severity, intensity, etc) for each drought index and compare the results. Authors Response: We are thankful to the reviewer for his valuable suggestions. However, inclusion of the suggested analysis and procedures (i.e. drought duration, severity, intensity, etc) are beyond the scope of the objective. 5. The authors claim that if a positive linear relationship exists between precipitation and temperature then the proposed method could be applied. However, the relationships for the 10 stations are not presented. These relationships should be presented and thoroughly been discussed, since the methodology is based on these relationships. The discussion of the relationships should be linked with the climatic features of the 10 station locations. Authors Response: In casestudy experimentation, the correlation between precipitation and temperature is positive. For the revised paper, we have now prepared a table of correlation between precipitation and temperature. In addition, the significance of the correlation is tested by T-test. 6. The empirical probability distributions of precipitation in the 10 stations should be discussed and the discussion should be linked with the climatology of the regions of Pakistan. Authors Response: We have a complete set of analysis data and graphs. We will make a panel graph of the probability distribution. In light of these graphs, the discussion section will be improving accordingly.

Minor comments 1. There many sentences that need rephrasing. For example: a. Page 2. Line 4. “. . .(SDI) are one of the most . . .” Please revise – check English Authors Response: In revised version of the manuscript, we have now revised the

C3

sentence. b. Page 2. Line 30. “Some of them are. . .” Please revise Authors Response: Thank you reviewer. We have now revised it. c. Page 3. Line 18. Please correct 20017 to 2017. Authors Response: We have now corrected the typo. d. Page 4. Line 10. What is “unbiasedness”??? Please rephrase-correct Authors Response: In statistics and sampling theory, unbiasedness is the property of an estimator. For the best understanding, we will try to rephrase it accordingly. e. Page 4. Line 28. “. . .by both mathematically and numerically. . .” Please correct. Authors Response: Thank you. The grammatical mistakes and typo will be corrected from native English speaker.

And others. 2. The first two sentences of Abstract and Introduction are the same. Please revise having in mind the major comment #2. Authors Response: In revised version of the manuscript, we now have revised the sentence accordingly.

3. Figure 1. Should present the elevation (DEM) of Pakistan. The figure should be a proper map of Pakistan having scale, legend and North symbol. Authors Response: Here, the map of study area is flexible for its improvement. In a revised map, we will follow the reviewer instructions and suggestions. 4. Equation 3. Define $F(x)$ and $G(x)$. Authors Response: $F(x)$ is the notation of the cumulative distribution function of any probability distribution. While $G(x)$ is the modified cumulative distribution function. In the revised manuscript, we will define it accordingly.

The presented study falls within the scope of NHESS. However, the paper is not ready for publication and needs major revisions before it would be acceptable for publication in the journal of NHESS.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-373/nhess-2018-373-AC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-373>, 2019.

C4