

***Interactive comment on***  
**“Intensity-Duration-Frequency and spatial  
analysis of droughts using the Standardized  
Precipitation Index” by M. Mohseni Saravi et al.**

**Anonymous Referee #1**

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The manuscript: Intensity-duration-frequency and spatial analysis of droughts using the Standardized Precipitation Index is a failed attempt to analyse spatial and temporal variability of droughts in a region of Iran. The manuscript does not follow the common standards of a paper to be published in a top-quality journal as Hydrology and Earth System Science. The manuscript has several lacks and shortcomings both in conceptual and formal aspects. More detailed comments are listed below.

1. Abstract is too long. It must be concise and to inform about the main findings of the article. For example, the three first sentences are too generic and it would be more suitable to be included in the introduction.

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2. Several cited references are not listed in the reference list. E.g. Dalezios et al. 1991, Girux, 2001, Zenvettor 2000, etc.
3. Introduction should not include references and formulas.
4. In relation to the purposes stated by authors and the following results. Authors indicate the objective of assessing the SPI capability to monitor droughts. The capability of SPI to analyse droughts has been detailed in a number of studies. Authors should reformulate the objective in terms of analysing spatial and temporal variability of droughts in the region.
5. Authors use a very short period (1972-1999) to analyse the climate characteristics of droughts and to obtain the SPI since long-term stable parameters are required for calculations with a minimum of 30 years. Updating the data until 2006-2008 would be useful to extend the analysis and to fulfil the requirements of longest records for SPI calculation.
6. A map with the location of the study area within Iran would be welcomed for an international reader. Also a Digital Terrain Model including elevations would be useful to understand spatial differences of climate.
7. Authors indicate that missing data was very small in the series. What is really small? A percentage of the data gaps would be welcomed. Authors state that missing gaps were completed based on correlations. I suppose they refer to linear regression since pearson correlations only measure the association between variables.
8. What about the quality and homogeneity of the series?. To test the homogeneity is crucial in any climate analysis to guarantee that climatic signal is not affected by artificial noise and to allow for spatial comparisons.
9. Authors justify the use of SPI-3, -6, -12, -24 and -48 since these time scales affect the five types of usable water resources. What kind water resources authors do refer? Any empirical evidence for this statement?

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10. Authors state that to calculate the SPI is required a good fit to a Gamma distribution. Therefore, why the authors test other distributions?. Given the results shown in the Figure 3, I do not think that Gamma distribution match well to the empirical frequencies. There is a large discrepancy between observed and modelled  $F(x)$ . Thus, If this is the common adjustment for the different observatories the use of a Gamma distribution is not a correct approach. 11. Authors state the use of a software to calculate the SPI. What software was used?.

12. Authors indicate that drought duration and their magnitude series were computed. I understand that authors firstly identified drought episodes but it is not explained how they are calculated. How the drought periods were identified? According to a determined SPI threshold? A possible temporal dependence of droughts was considered?.

13. Given the low number of years used I do not think correct the probabilistic approach based on drought duration and magnitude series. For example, for a 12-months SPI in the station of Yassoj shown in Figure 4 I identified visually only 9 droughts are identified if  $SPI = 0$  is fixed as a threshold to select droughts. Based on 9 records it is not possible to obtain reliable statistical adjustments to estimate the droughts duration and magnitude risk. For examples, there are very few records to estimate 100-years return periods but authors do it. Authors should provide more details about the method of obtaining drought duration and magnitude series but also more information about the probabilistic approach to estimate the risk. For example, a L-moments diagram (see references of Dr. Hosking) or a Chi-square test or a Kolmogorof-Smirnof test are very suitable approaches. Thus, table 3 does not provide any information about the goodness of the excessively short series used to any frequency distribution.

14. A comment about interpolation of SPI. Authors indicate the calculation of the RMSE, where the values are?. Given that IDW is an exact interpolation procedure, how the RMSE were calculated: using a jackknife routine or with an independent sample for validation?. IDW produces high bulk-eye effects and for a robust interpolation of climate data other methods are preferred (e.g. Kriging, thin plate splines, etc.)

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15. The results and methods are mixed. Thus Figures are cited two times (in methodology and results sections).

16. Table 2 is usually mixed up with Table 2. Table 3 does not show any relative frequency.

17. An important problem is that authors analyse iso-frequency and iso-duration drought maps. This is a very important error when using SPI. According to the SPI definition, the probability of occurrence of a determined SPI value is the same for the different areas represented by components, i.e. the index cannot be used for estimating the spatial differences in drought risk since if long precipitation series are used the same frequencies will be obtained to any analysed place. Therefore, the approach followed by authors to analyse drought risk is erroneous and the maps for iso-frequency and iso-duration are not valid for a drought analysis.

18. In section III authors express drought duration in terms of consecutive days with no rain but previously they stated the use of monthly data. I understand this as an error.

19. There is not explanation about how return periods were calculated.

20. The majority of the results section is unnecessary. For example, authors detail the drought variability in one observatory. Maybe if the analysis and description correspond to a regional series, or a more extensive spatial analysis, could have some interest. Also the explanation of the drought patterns in May 1999: Two pages to explain a particular episode.

21. Discussion repeat mostly the results of the previous section.

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