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## Comment on nhess-2020-416

Anonymous Referee #2

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Referee comment on "Evaluating Spatiotemporal Patterns and Trends of Drought in Japan Associated with Global Climatic Drivers" by Ke Shi et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2020-416-RC2>, 2021

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Review of: Evaluating Spatiotemporal Patterns and Trends of Drought in Japan Associated with Global Climatic Drivers.

This article analyses drought variability in Japan by means of global datasets and the PDSI. I think the manuscript needs to reinterpret and rework several issues and to change several issues affecting both formal and substance aspects.

35-36. This should be qualified in some way. In which regions? You note e.g. the different view by Sheffield et al. (2012) Nature, also based on the PDSI.

43-44. which events? Why these continents and not others?

44-46: Under rainfall deficits is not expected an increase of ET (as the soil moisture is constrained), then what has an effect is the increase of the atmospheric evaporative demand (this term is absolutely better to use than potential evapotranspiration, see e.g. <https://onlinelibrary.wiley.com/doi/full/10.1002/wcc.632>), which increases plant stress, leaf temperature (given changes in the partition of latent and sensible heat).

60-61: also thermodynamic forcing (e.g. radiative CO<sub>2</sub> forcing increases VPD, but also land-atmosphere feedbacks: see

<https://nyaspubs.onlinelibrary.wiley.com/doi/10.1111/nyas.13912>)

95-103: This is better to be moved to the methods.

116-117: I think the capability of this dataset must be tested for the use of the assessment of droughts at the regional scale. Some comparison with local stations/datasets would be desired.

122-144: This is not needed. There are several references reviewing PDSI. Authors should cite the pioneer study by W. Palmer of 1965. It is not necessary to show the formulation of the FAO-56 Penman-Monteith reference evapotranspiration.

145-157: This is also not needed. Authors are using directly the PDSI data generated by Gerard Van der Schrier, so they may simply refer to this dataset and do not explain how this author generate it.

159-166: Why do the authors select these circulation indices and not others. Note that the effect of ENSO can be very different as a function of the selected index given the different physical mechanisms related

(<https://link.springer.com/article/10.1007/s00382-016-3082-y>).

180-199: Not needed to describe the work of Yue et al. 2002.

What is the purpose of using the methods described in 2.3 and 2.4? The authors should justify its use in the context of the objectives of the study.

238: In figure 1 it is not necessary to include the trend in temperature. It is not a metric included in the analysis of drought by means of the PDSI. Please, include precipitation and atmospheric demand in absolute units using the same scale.

259-260: This is confuse. Gridded data from CRU is based on interpolation of station data. With this approach the authors suggest a re-interpolation based on mann-kendall results, but this should not be necessary as the input data is already a gridded dataset.

278: Averaging PDSI is not a suitable approach given spatial differences of autocorrelation characteristics in the PDSI

([https://journals.ametsoc.org/view/journals/hydr/11/4/2010jhm1224\\_1.xml](https://journals.ametsoc.org/view/journals/hydr/11/4/2010jhm1224_1.xml)) so spatial comparability is poor. If authors are interested to show a time series over Japan, figure 4 showing surface area affected by drought (in %) is much better.

296-308. This figure is redundant. Given strong autocorrelation of the PDSI (<https://www.sciencedirect.com/science/article/pii/S0022169414009305>), it is not expected a difference in drought conditions at the seasonal level considering the PDSI. Figure 7. I do not find this figure very logical as precipitation is declining more than PDSI. PDSI is low sensitive to the atmospheric demand as there is an alpha parameter used to obtain the cafec PET that limit the role of PET, but this role is always negative so I do not find logical that under increased PET, the precipitation may show a more declining trend that the PDSI.

353-358: Same comments related to the relevance of seasonal differences. If the authors want to analyse droughts at the seasonal scale, the PDSI is not the best choice. More useful alternatives are the SPEI or the SPDI (Ma et al., 2014, Hyd Procc). These indices provide exact seasonal information as the time scales are defined a priori and they are known.

3.4. This section seems to be very disconnected to the rest of the study. The authors provide a very simple analysis to connect drought index and area affected by fire. In addition, there is not any critical discussion of the obtained results.

3.5. This section would gain clarity if in addition to the power spectrum analysis the authors would include further information in order to determine possible anomalies in the selected atmospheric circulation indices during drought conditions (in average for the whole Japan and for the two regions). A simple box-plot or some maps of PDSI anomalies corresponding to high/low phases of these circulation indices would be enough. With the current information it is really difficult to distill if drought events may be affected by these circulation drivers.

Section 3 although it is named as "results and discussion" shows very limited critical discussion of the obtained results, but this is necessary in any serious scientific research.