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Reply on RC2

Ke Shi et al.

Author 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-AC3>, 2021

We greatly appreciate you for your constructive comments and suggestions.

First of all, please allow us to make a brief reply. When the open preprint is over (until 17 Feb 2021), we will make a detailed reply and upload the corresponding revised manuscript.

Point 1: 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.

Response 1: Thank you so much for your comment. We will add a more specific description and refer to Sheffield et al. (2012) in the revised manuscript.

Point 2: 43-44. which events? Why these continents and not others?

Response 2: Thank you so much for your comment. I'm sorry that we didn't give a detailed description and made this sentence weak. We will specify different drought events in different regions to explain clearly in the revised manuscript.

Point 3: 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).

Response 3: Thank you so much for your comment. We agree with your point. It is really important to use the correct terminology. So, we will rewrite this sentence and discuss in detail the type of drought targeted in this paper and the impact factors of drought.

Point 4: 60-61: also thermodynamic forcing (e.g. radiative CO₂ forcing increases VPD, but also land-atmosphere feedbacks: see <https://nyaspubs.onlinelibrary.wiley.com/doi/10.1111/nyas.13912>)

Response 4: Thank you so much for your comment. We are grateful for your recommended paper, and we will check Diego G. Miralles(2018) carefully. We will rewrite this sentence to include more accurate physical mechanisms.

Point 5: 95-103: This is better to be moved to the methods.

Response 5: Thank you so much for your comment. We will move this part to the method in the revised manuscript.

Point 6: 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.

Response 6: Thank you so much for your comment. We are also cautious about the capability of datasets. Therefore, in the appendix, we compare the results of the CRU dataset with the data from Japanese meteorological stations and Dynamical Regional Downscaling Using the JRA-55 Reanalysis (DSJAR-55). See Appendix and Figure A4 for details.

Point 7: 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.

Response 7: Thank you so much for your comment. The purpose of showing the key formula of scPDSI is to facilitate the understanding of the impact of meteorological variables such as precipitation and potential evapotranspiration on scPDSI. We agree with you that the formula for the potential evapotranspiration is really unnecessary, and we will remove this part.

Point 8: 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.

Response 8: Thank you so much for your comment. The purpose of showing this part is mainly to reflect the advantages of scPDSI; that is, it can consider snow. We are sorry that we did not explain clearly. We will first improve the introduction, explain the advantages of scPDSI and correspond to the content of the introduction here.

Point 9: 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>).

Response 9: Thank you so much for your comment. The reason for choosing these climatic drivers is that there have been some papers discussing these climatic drivers on Japan's climate. We will further explain the reasons for the selection and add references in the revised manuscript.

Point 10: 180-199: Not needed to describe the work of Yue et al. 2002.

Response 10: Thank you so much for your comment. We will remove this part in the revised manuscript.

Point 11: 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.

Response 11: Thank you so much for your comment. First, we will strengthen the description of the purpose of this article in the introduction. Distinct empirical orthogonal function (DEOF) is mainly to identify drought homogeneous regions in Japan. In this way, we can find specific regions from the long-term Japan drought index time series. And

TFPW-MK can identify drought trend changes in homogeneous regions. The drought homogeneous regions will be helpful for drought management and prevention, and it will strengthen our understanding of the characteristics of drought in Japan.

Point 12: 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.

Response 12: Thank you so much for your comment. The purpose of showing the temperature is that although the temperature is not used directly in the calculation of scPDSI, the temperature is used in the calculation of PET. After all, the temperature has a great influence on PET. We will modify the figure 1 using the same scale in the revised manuscript.

Point 13: 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.

Response 13: Thank you so much for your comment. Our study area is small. The 0.5° resolution is challenging to reflect the trends of meteorological variables and drought in different regions of Japan. Interpolation is used to try to distinguish the trend differences between different regions in Japan.

Point 14: 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) 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.

Response 14: Thank you so much for your comment. We will remove Figure 3 in the revised manuscript.

Point 15, 17: 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.

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.

Response 15, 17: Thank you so much for your comment. Analysing the seasonality of scPDSI is indeed challenging. But our purpose in these parts is to find the drought changes in different seasons. Different droughts often correspond to different events. For example, the spring drought in Japan often corresponds to wildfires. Summer drought in Japan corresponds to water shortage. We will add some explanations in these parts, explaining that these parts of the result needs to be treated with caution due to the autocorrelation of the scPDSI.

Point 16: 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.

Response 16: Thank you so much for your comment. We are sorry that Figure 7 (b) and (c) did not use a unified unit. In fact, precipitation has not decreased more significantly than drought. We will modify Figure 7 in the revised manuscript.

Point 18: 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.

Response 18: Thank you so much for your comment. We will try to strengthen this part and add critical discussions in the revised manuscript.

Point 19: 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.

Response 19: Thank you so much for your comment. The main purpose of our selection of wavelet analysis is that the time-lag effect of climatic drivers on drought. And our results also showed that the impact of climatic drivers on drought is complex, and it often takes several months to have an impact on drought. We will rewrite the introduction and explain the purpose of each method in the introduction to make the subsequent results more logical.

Point 20: 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.

Response 20: Thank you so much for your comment. We will add some critical discussions in the revised manuscript.