Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2017-100-RC3, 2018

Earth System Dynamics
Discussions

Interactive comment on "Population exposure to droughts in China under 1.5 °C global warming target" *by* Jie Chen et al.

Anonymous Referee #3

Received and published: 1 February 2018

Unfortunately, I do not share the positive view of the other two reviewers about this manuscript. I find the idea of the study interesting and worth exploring. But the analysis suffers from a number of severe flaws and therefore fails to provide meaningful quantifications and insights. Both the analysis and the text would require major and fundamental revisions to come to a publishable manuscript. My points of concern are as follows:

1. The standardized precipitation evapotranspiration index (SPEI) used in this study is an index of meteorological drought. Meteorological droughts do not necessarily coincide with agricultural, hydrological, or even socio-economic drought (see Wilhite, D. A. and Glantz, M. H. (1985) 'Understanding the Drought Phenomenon: The Role of Definitions', Water International, 10(3), pp. 111-120. doi: 10.1080/02508068508686328).

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Thus, meteorological droughts have only limited direct relevance to people. In addition, the SPEI defines meteorological drought as departure from the mean climatic water balance (precipitation minus potential evapotranspiration) in multiples of standard deviations. For example, a value of -1 marks an event that deviates by one standard deviation from mean conditions. By definition, 15.9% of all time steps will be classified as -1 or less. It is obvious that such an indicator does not provide a measure of dryness in an absolute sense. Under wet conditions with low temporal variability, most SPEI droughts are still wet in an absolute sense; under dry conditions, many very dry events may not be classified as drought by the SPEI. Despite these shortcomings, I do believe that assessing population exposure to changes in meteorological droughts under climate change is a valid research question. But the limitations of the employed indicator (and drought type) must be highlighted and discussed to avoid misinterpretation of the results. This is clearly lacking in the paper, which instead tends to overstate the meaning of population exposure to meteorological droughts (e.g., page 2, lines 8-11).

2. The basic concept of the SPEI is to transform a time series of the climatic water balance into a time series of normally distributed index values with a mean of 0 and a standard deviation of 1. For this transformation, a probability distribution function is fitted to the empirical distribution of climatic water balance values. The fitted distribution function is then used to map the climatic water balance values to SPEI values corresponding to the same quantile. Performing the transformation for present day and future time periods with independently fitted distribution functions, will yield two SPEI time series with the same statistical properties. Any attempt to identify a climate change signal will fail with this approach as the signal is lost in the transformation. Therefore, a single distribution function (preferably estimated from the reference period) must be used for the transformation of both the reference and future time series to be able to detect changes in the frequency of drought events. It is not clear whether this has been done correctly in this analysis as the method sections only provides a very vague description of the SPEI calculation. However, the results and how they are

presented indicate that separate distribution functions have been fitted to the reference and the future time period.

3. On page 2 line 32 the authors explain that the climate data from the five available GCMs had been averaged prior to the analysis. Averaging time series is never a good idea. But in the case of GCM time series and with the aim to calculate SPEI it is simply wrong. The argument that "combining multiple models has been to shown superior to a single model" only holds true for long term averages and only for the comparison to observations. The SPEI analysis must be performed for each GCM individually. The results can then be averaged while properly accounting for GCM uncertainty.

4. The paper defines population exposure to drought as "the frequency of mild, moderate, and extreme droughts multiplied by the number of people exposed to them" and reports it as number of people. I don't think this is appropriate. Let's assume a moderate drought is found to occur over 10 % of the time in a given grid cell. Then, according to the above definition, 10 % of the total population in that grid cell would be counted as exposed to moderate drought. This is strange because intuitively one would expect that all people in that cell will experience moderate drought conditions over 10 % of the time. It is possible that it is only the unit (population numbers) that is puzzling here and that it could be fixed by including the temporal dimension. However, under no circumstance should the population exposure obtained for different drought severity classes be added (as done on multiple occasions in the paper).

5. The methods description is very short and lacks explanation of important aspects, which are crucial for the understanding of the analysis. It is by no means clear how ETo was calculated (e.g., climate variables used, temporal resolution) and which procedure was used to derive the SPEI (e.g., temporal resolution or number of time steps of SPEI, probability distribution type assumed for climatic water balance, fitting methods for estimating parameters probability distribution function, same or different parameters for reference period and scenario). In order to assure transparency and reproducibility of the analysis this information must be provided.

6. It is not clear to me how the section 3.4 can contribute to a quantification of uncertainties.

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