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Interactive comment on "Global meteorological drought and severe drought affected population in 1.5 °C and 2 °C warmer worlds" *by* Wenbin Liu et al.

Wenbin Liu et al.

liuwb@igsnrr.ac.cn

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This paper assesses changes in drought risk (and human population exposure to these risks) at 1.5 and 2 degree thresholds drawn from 11 CMIP5 models and the RCP 4.5 and 8.5 scenarios. Unsurprisingly, they find less risk/exposure at 1.5, though the abstract is missing important details of their results (where is mitigatiodn most important for reducing risks?). I think this study has some potential merit, but I have some significant concerns and critiques that I would like to see addressed before I recommend publication.

Thank you. In the revised manuscript, each of comments/suggestions has been carefully addressed (please find the point to point response below). In the acknowledgement section, we added: "We thank the Editor (Michel Crucifix), Dimitri Defrance and anonymous reviewer for their helpful comments". (Line 462-463 in the latest clean version).

1) The authors evaluate drought using one specific drought index: PDSI. This is generally fine, but there are some issues regarding how this index is used by the authors. First, despite the title of the original Palmer paper, PDSI is an indicator of agricultural drought risk because it emulates soil moisture availability. Meteorological drought refers specifically to deficits in precipitation. The language in the paper, and the title, should be adjusted accordingly.

We accept the criticism.

In response, we replaced "meteorological drought" with "drought" in the title and the main text and adjusted the language (Line 179-180) to reflect the meaning and implication of PDSI as an indicator of drought risk throughout the manuscript. Thank you.

Second, it is unclear what time period the authors used for the PDSI calibration (i.e., the CAFEC). Typically, one would use some common historical baseline across models so that future changes can be interpreted relative to historical variability. For this particular study, I would recommend using 1850-2000. Doing it this way would thus not require any differencing between future and historical periods, since the PDSI for the future would implicitly reflect drought changes relative to the historical period.

We could have explained the calculation of PDSI more clearly in the original version of this manuscript. The parameters (i.e., duration factor) in PDSI model were actually calibrated during the period of 1850-2000 in this study.

In response, we clarified this PDSI calibration and calculation in Section 2.3 (Line 197-198). Thank you.

Finally, because of the inherent memory and persistence embedded within the PDSI calculation, this index is much better for picking up long-term and seasonal-scale droughts, and is less appropriate for shorter term (e.g., 1-month) events. For example, the severe and by some indicators record breaking 2012 drought in the Central Plains of the United States only shows up modestly in PDSI, primarily because this drought intensified quite quickly. For this study, where the authors are interested in month to month changes in drought intensity/persistence/etc, it would be better for the authors to use the Z-index that comes out of the PDSI calculation.

We agree that the PDSI was criticized for its inability to depict droughts on time scales shorter than 12 month when monthly PDSI values were used (by A.G. Dai, link: https://climatedataguide.ucar.edu/climate-data/palmer-drought-severity-index-pdsi). However, the purpose of this study is to examine the changes in PDSI-detected drought characteristics (i.e., monthly-averaged PDSI, mean drought duration, intensity and severity) between the 1.5/2 degree warming scenario and the baseline period. This time-scale related issue is not the focus of this study. Whilst Z-index could assist short-term drought identification, we are not aware of standardized method(s) from existing literature which could help defining drought duration, intensity and severity. By contrast, the PDSI method appears to be a more compelling method in current study.

2) Given the relative coarseness of the CMIP5 models, I think interpolation of the results to 0.5 degree spatial resolution is not appropriate. A 2 degree common grid would be better, and would avoid effectively making up data at the much finer resolution.

In this study, we first calculated the global PDSI and related drought characteristics (drought duration, intensity and severity) using GCM-outputs with their original spatial resolution (the results were thus not affected by interpolation in this step). The obtained results were then rescaled to a common spatial resolution of 0.5o $\times$ 0.5o using the bilinear interpolation, in order to (1) show the results with a finer resolution uniformly and (2) accommodate their spatial resolution to that of SSP1 population (0.5o $\times$ 0.5o). The original resolution of SSP1 population is 0.125 degree. We thus use a 0.5 degree resolution to avoid effectively making up data of the finer resolution in SSP1 data.

In the revised manuscript (Line 227-233), we clarified the justification of spatial resolution used in interpolation as follows, to make them more clear and understandable.

"...It should be noted that the global PDSI and related drought characteristics were first calculated using GCM-outputs with their original spatial resolution. The obtained results were then rescaled to a common spatial resolution of 0.5o $\times$0.5o using the bilinear interpolation, in order to show them with a finer resolution uniformly and accommodate their spatial resolution to that of SSP1 population (0.5o $\times$0.5o). The original resolution of SSP1 population is 0.125 degree. We thus use a 0.5 degree resolution to avoid effectively making up data of the finer resolution in SSP1 data..."

3) The population analysis in this study is a bit convoluted. For example, the RCP scenarios use different populations trajectories (I believe), and since you are picking somewhat arbitrary periods that just match desired warming, there will be little consistency in population structure across either the scenarios or warming targets in this analysis (see Figure 1). Since the 1.5 and 2 degree targets are stabilization scenarios, which would theoretically hold out through the end of the 21st century, I think the authors should remove all the population analyses except for the SSP1 2100 analysis (Figure 4). I would also ask the authors to turn Table 4 into one or more figures, since it is difficult for the reader to synthesize such a large table of numbers.

Excellent idea.

In response, we kept the results of SSP1 2100 (SSP1 2100 population for +1.5 and +2 degree warmer scenarios, SSP1 2000 population for the baseline period) and turned them into three figures (Figures 11-13). We removed the old Table 3-4 and Figure 11-13 from the original version of this manuscript. We updated the Method, Results, Conclusion, Abstract sections accordingly. Thank you.

4) For the analyses, how many datapoints (presumably months) are included in each warming scenario?

For the 1.5oC warmer world, there are (12+19)*12=372 data-points (12 years of 2027-2038 under the RCP4.5 scenario and 19 years of 2029-2047 under the RCP8.5 scenario) included in the analyses. For the 2oC warmer world, there are totally (29+12)*12=492 data-points (29 years of 2053-2081 under the RCP4.5 scenario and 12 years of 2042-2053 under the RCP8.5 scenario) included in the analyses.

In response, we added details of this information in Section 2.2 (Line 171-172) of the revised manuscript as follows,

"…It finally results in totally 372 (31 years) and 492 (41 years) data-points (months) for the 1.5oC and 2oC warming scenarios in the following analysis..."

What are the units for drought duration (Figure 5), drought intensity (Figure 7), and severity (Figure 9)? Please add this information to the figure captions.

The unit of drought duration is "months" in this study. Drought intensity and severity are two dimensionless variables.

In response, we added this information to both the figure captions and the main text of the revised manuscript (Line 221-224). Thank you.

Was significance/robustness/consistency only assessed in terms of agreement across the multi-model ensemble (right columns in the aforementioned figures)? If so, what was the threshold used by the authors to determine whether a given change was sufficiently robust?

In this study, we performed significance/robustness/consistency analyses in different ways. We first quantified the robustness of the results among climate models using model consistency (the right panels in Figures 3, 5, 7 and 9) and boxplots (Figures 4, 6, 8 and 10). We then characterized different impacts of severe droughts on population at continental scales using the multi-model ensemble mean and the corresponding uncertainty among climate models in Figures 11-13. For each case, we did not give a fixed threshold to determine whether a given change was sufficiently robust. However,

we gave a range of model consistency (from 6/11 to 11/11) to show the results from non-robust (i.e., <6/11), less-robust, median-robust to sufficiently robust (i.e., 11/11). For example, in the right panel of Figure 3, the robustness of projections increases with higher model consistency and vice-versa.

In response, we revised Lines 403-416 to explain the robustness of these results.

5) What is causing the changes in drought risk in these simulations? Declines in precipitation or increases in evaporative demand from warming? Since PDSI is an offline calculation, you can recalculate this index using detrended temperature/ precipitation to tease this out. This would be a valuable addition.

Whilst it is possible to perform attribution study, we have concern that it would shift away from the main focus of current study (assessing drought risk and its related population impacts under +1.5o and +2.0o warmer worlds) and our target audience (i.e., international policy-makers). There are currently many studies focusing on the changes in drought risk and its attribution (i.e., air temperature, precipitation and potential evaporation) conducted at both global and regional scales and for both historical and future periods (Cook et al., 2014; Ficklin et al., 2015; McCabe and Wolock, 2015; Li et al., 2017). It is an interesting idea worth deep investigation in a separate study, but it is obviously beyond the scope of the current study.

In response, we discussed the plausible future studies related to the review's interesting idea in Line 447-450 as follows,

"…Future studies are needed to obtain a better understanding of the causes of changes in global drought (i.e., decline in precipitation/increase in evaporative demand) under different warming scenarios (1.5 oC and 2 oC), which are very important to the mitigations and adoptions of climate-induced drought risks in the future at both global and regional scales..."

Reference: Li, Z., Chen, Y.N., Fang, G.H., Li, Y.P.: Multivariate assessment and attribution of drought in Central Asia, Sci. Rep., 7, 1316, 2017. McCabe, G.J., Wolock, D.M.: Variability and trend in global drought, Earth and Space Science 2, 223-228, 2015. Ficklin, D.L., Maxwell, J.T., Letsinger, S.L., Gholizadeh, H.: A climate deconstruction of recent drought trends in the United States, Environ. Res. Lett. 10, 044009. Cook, B.I., Smerdon, J.E., Seager, R., Coats, S.: Global warming and 21st century drying. Clim. Dyn. 43(9-10), 2607-2627, 2014.

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