In this manuscript, the authors used the SPEI and run theory to define drought events, analyzed the variations of drought severity and duration by joint return period based on copula function and highlighted changes in exposures of population and GDP to global drought under three RCP scenarios (corresponding to three SSPs) at 1.5$^{o}$C and 2 $^{o}$C warming targets. The idea of studying the socioeconomic exposures to global drought is meaningful for countries concerned to understand and mitigate potential drought risks in the future. Generally, the manuscript is well organized with clear logic, before I recommend it for publication, major improvements are still needed.

When discussing the increase in the magnitude of global drought, the severity and duration of drought are both considered using a copula function and the drought is defined using SPEI< -0.5 and run theory, the methods are all good. As in table 2 indicates, SPEI <-0.5 incorporates three different levels of drought from mild, moderate to extreme drought. The authors used copula function to consider both the severity and duration, however, the severity of drought retrieved from the run theory may not reveal the distribution of different levels of drought? Although authors discussed the threshold of 0.8 to confirm relevant results, whether the selection of this threshold may further influence the results of socioeconomic exposures to droughts is worth thinking.

When calculating SPEI with Penman-Monteith-based PET, the term ($0.34u_2$) in the equation is finally obtained through the ratio $r_s/r_a$ and represents the suggested reference crop surface (assuming a standard plant height of 0.12 m, affixed surface resistance of 70 $sm^{-1}$ and an albedo of 0.23). However, considering a distinct vegetation response to elevated $CO_2$ as simulated in the fully coupled climate models, it is important to point out that some of the assumptions that underlie the computation of PET (and thus SPEI) are incorrect (or at least the projected drought is not so severe) under conditions of changing $CO_2$ concentrations (Greve et al., 2019, ERL; Yang et al., 2018, NCC; Roderick et al., 2015, WRR).The authors should at least discuss the potential impacts of the elevated $CO_2$ on their drought risk assessment in Section 4.

Given the relative coarseness of the CMIP5 models, I think interpolation of the results (especially bilinearly interpolated P and PET to a common resultion before calculating SPEI with them) to 1 degree spatial resolution is not appropriate. A 2 degree commen grid would be better, and would avoid effectively making up data at the much finer resolution. The authors should at least discuss the impact of interpolation on their results in the maintext.

Some specific parts need further clarification.

1. During the investigation regarding the exposures of population and GDP to droughts under three RCP scenarios at two warming levels, for example, under the RCP8.5 scenario (SSP5), the specific time when future warming reaches 1.5 $^{o}$C or 2 $^{o}$C

under RCP 8.5 can be different (from Fig 1), population and GDP can also possibly differ in two climates. From Line 17-Line 25 (Page 11), did the author suggest that the dynamic of population and GDP under RCP 8.5 at two warming climates was also considered using the multi-year average? If so, in section 3.4 about population and GDP exposure from increasing drought risks, it was concluded that a large percentage of population and GDP will be exposed to increasing drought risk. The drought risk has been increasing with warming climate, let's say if population and GDP have been increasing with time, then which one contributes to the increasing exposures, the increasing population or the increasing drought risks, I think this is a key question that authors should clarify when assessing the socioeconomic exposure.

2. Page 11, Line 13-16, how is the ratio of the recalculated recurrence frequency calculated and why a less than 1.0 ratio suggests worrisome drought condition. Need further clarification.

3. Page 12 section 3.1 projected changes in dryness, the author used SPEI and the run theory to define drought event, and the title of the manuscript is about the global drought, why would authors use SPEI to explain the dryness instead of using the defined event to study the changes in global drought for consistency.

4. Page 15 Line 28-29, whether the fraction of drought-affected population (or GDP) divided by total population (or GDP) can be a fairer and more impartial assessment is really hard to say given the fact that this method seems to cover up some most drought-affected countries, like the United States and China.

5. Generally, in the discussion of either the magnitude of drought or the socioeconomic exposures of droughts, the differences between two warming targets are highlighted, however, the differences among three RCP scenarios are barely discussed in the manuscript. It makes me doubt the reason and necessity of using three RCP scenarios since they present almost similar variations under two warming targets. This issue might be even obvious in Fig 9 and 10, for example, in Fig 9, under RCP 4.5, population and GDP suggest 100% exposure to drought in Australia, which drops to 90% under RCP 8.5. Possible reasons and texts are needed here.

6. Not sure whether section 3.5 is necessary since similar conclusions have been achieved in Fig 7 and 8, and these typical countries can just be used for further explanations in section 3.3. Besides, additional explanations for Fig 7g and Fig 8g are very necessary.

Minor suggestions.

1. Citation of Fig. 3 somewhere between lines 21 and 22 in Page 12.

2. Writing in the manuscript should be more concise in the data and method section, e.g. Page 6 line 7, use surface maximum, mean, minimum air

temperature to avoid repeat.

3. Table 2, extreme drought instead of extremely drought