Reply to anonymous Reviewer #2

General Comments

This study examines the projected change in hydrologic drought severity, duration, and frequency due to climate change across Europe. It employs a unique GWL perspective to merge projections and represents a significant effort to combine climate, land cover, and population projections with hydrologic modeling to estimate drought exposure. Overall the work is of a high quality; however, I have a number of reservations, as described below. The majority of these issues are clarifications of the methodology, which are needed to fully assess the findings. It is also important to clarify the interpretation of some results. I therefore recommend a significant revision.

We thank the reviewer for the constructive comments on the manuscript. Here we provide brief replies to the major issues in order to highlight how we are planning to accommodate these comments in the revised version of the manuscript.

Major issues

M1. Is it possible to provide the range of years the ensemble members reach the GWLs for context? It would help to confirm that the present conditions have not surpassed 1.5K and provide some context to how fare off +1.5K is from the present. If this is not possible, at least provide delta K for the reference period.

We agree that this information will give more context to our results. We will provide the temperature difference between the preindustrial period and the baseline (delta K = +0.7K) and an indication of the ensemble variability in the years to reach GWLs for the two RCPs.

M2. Related to A1, you are incorporating changes in population, land cover, and water abstraction with time through 2050. But, because the endpoints are tied to GWL, rather than a year, each member of your ensemble will have slightly different values for these model inputs. Are you accounting for this? Can you provide a relative estimate of the water abstraction changes? This would help provide sensitivity/scale for this portion of the model.

We are indeed accounting for this, and the reviewer is correct that ensemble members may have slightly different values for some of the underlying socioeconomic variables. The projected changes in socioeconomic variables are available in 5-year time steps. Demographic and land use changes in Europe are relatively mild up to 2050, while they remain constant afterwards. Hence, spread in water abstraction driven by the socioeconomic drivers and the effect on water availability are small compared to the effects of climate change, with the latter also affecting water demand for crops irrigation. We will better clarify these issues in the revised manuscript.

M3. Changes in snowmelt patterns and seasonality have a potential impact on future hydrologic changes at higher elevations and latitudes. You mention this on Line 372. Does your model incorporate a snow accumulation/melt module?

Yes, Lisflood has a snow module that is based on the degree-day factor method. We will better emphasize this in the model description.

M4. Please provide more clarification as to how the return periods are being derived. More detail is needed than the reference to Cammalleri et al (2017) paper. It appears you are using a peak-over-threshold/partial duration series approach. I am most familiar with using the generalized pareto distribution for return periods in this context. It appears like you are using the Pareto Type II. Please explain this choice. Also, be aware that in the context of a partial duration series, your statement on line 163 "the probability that one event is topped in any one year" is slightly less accurate than for an annual maximum series.

The Pareto Type II is a special case of the Generalized Pareto distribution, hence analogous considerations can be made. We agree with the reviewer that the statement on the return period can be confusing in our specific case for readers that are familiar with annual min/max series, hence we are going to revisit the text to clarify the definition and meaning of return period in the context of non-annual events.

M5. Please provide the methodology for calculating the change in drought duration shown in Figure 2. Does days/year represent a summation of all drought days during the reference period? I believe this is the correct interpretation. My confusion is because the Severity (D) analysis focuses on the severity of an individual event, whereas this Duration analysis focuses on a cumulative metric.

Also, as part of this, please revise your interpretation in Section 3.1.2. If you are summing up the days under drought conditions, then you cannot say that "droughts will last longer", as you do in Line 252. I interpret longer droughts as the individual drought events lasting longer, but this metric could increase due to more frequent, but similar duration droughts. Without knowing the number of unique droughts, you cannot make this statement, only that the total time spent in drought will increase.

The reviewer's interpretation of the definition of duration is correct. We also analysed the length of individual events and will re-evaluate if it is better to report the event-based information, and adjust the text accordingly.

M6. There is no significance testing for any of these claims. It is difficult to determine whether these trends are a significant signal or noise. The consistent regional patterns suggest a true trend. But, I would strongly recommend significance testing to quantify how much agreement there is among ensemble models (Fig 1) or how significant these changes are regionally (Fig 2/3).

The robustness of the changes has been accounted by reporting the areas where at least 2/3 of the ensemble models agree on the sign of the change. The area with no-agreement (usually in grey) are the ones where this condition is not met. We will better justify this choice in the revised version of the text.

M7. Line 426 - This interpretation, which depends on your assumption on Line 184, assumes independence among sites, which is not true. Regions enter drought at the same

time, so it is not fair to say that 10% of the region will be exposed to a 10 year drought in any given year. More likely, a majority of the Mediterranean (or at least the eastern/western portions) will enter drought at the same time.

Associated with this is the interpretation of Figure 4/5. Is this based on the 10-year drought only or all droughts?

We agree that drought usually occur over large areas, hence it is likely that all population will be affected at the same time rather than 1/10 every year. We estimate and present the expected average annual exposure for each 30-year period, which is the exposure that would occur in any given year if exposure from all drought probabilities and magnitudes are spread out equally over time (here those with return period of 10 years or less frequent). As correctly pointed out by the reviewer, this does not mean that each year has the same exposure to drought. Rather, in some years there will be high exposure, while in (most) others there will be low or no exposure. We understand that this caused some confusion, since it has been pointed out by both reviewers. We will revisit the text to clarify this, and if required supported by an additional figure on the relationship between different return periods, as suggested by the other reviewer.

M8. Please provide a data availability statement. This is required by HESS and is not included in the version I had access to.

All the data produced by the JRC are freely available to the public upon request. We are also planning to disseminate some of the key outputs throughout our Risk Data Hub (https://drmkc.jrc.ec.europa.eu/risk-data-hub). We will add this information to the manuscript.