

Reply to anonymous Reviewer #1

General Comments

We would like to thank the reviewer for his/her thoughtful revision of the manuscript. Here we provide some brief replies to his/her main comments, outlining the edits that we are planning to make in the revised version of the manuscript.

The study presented by Cammalleri et al. addresses a societally relevant question, i.e. how does global warming affect droughts in Europe with respect to duration, deficit, and frequency? While the study in itself is well motivated, the novelty of the approach could be made clearer and I see potential ways of extending the analyses into domains which have so far not received as much attention. I see many parallels to the study by [Marx et al., 2018] who studied low flow characteristics under different global warming levels for Europe. The main advancement of this study compared to the study by [Marx et al., 2018] are in my view threefold: (1) the authors use a drought definition instead of a simple low flow index which allows them to look at different drought characteristics including deficit and duration; (2) their model allows for the consideration of human flow modifications; and (3) they combine the hazard with an exposure analysis. I would make this clear in the introduction and clearly state what the added value of considering these three aspects is.

We thank the reviewer for properly identifying the main novelties of the study, and we plan to revisit the introduction to make them more evident.

In my opinion, the study presented could gain in profile, if the authors intensified the analysis of these aspects. Point 3 is probably easiest to tackle. They authors could highlight the exposure analysis in the introduction as this is something which goes beyond what previous studies have done.

We will further expand the focus on the exposure analysis in the introduction.

Points 1 and 2 could profit from some additional analyses. Regarding point 1, I would find a bivariate frequency analysis of deficit and duration interesting.

We are indeed exploring the possibility to focus future researches on a multi-variate analysis of different drought characteristics. Drought deficit and duration, however, are typically strongly correlated, hence a bivariate analysis of these two indicators would likely not deviate strongly from the analysis presented in our study. We think that a proper multi-variate analysis is worth of a full paper dedicated to the topic.

Regarding point 2, it would be very interesting to show how drought characteristics change in a human-modified world as opposed to a world where such modifications are not considered (i.e. run model with and without the water use module and compare the changes in drought characteristics resulting from the different model runs).

We agree with the reviewer that the effect of human water use is relevant for the analysis of drought. This is also why we considered this in our analysis. Forzieri et al. (2014)

showed in detail how water use alters river flows and streamflow drought indicators in different regions of Europe performing the analysis as suggested by the reviewer. In this study, a more detailed modeling of the dynamic socioeconomic condition is included, focusing on different aspects, namely on understanding drought hazard and exposure in a future world in case of climate inaction and different mitigation targets (warming levels). We believe that in order to address these questions a dynamic socioeconomic setting based on EU demographic, economic and budgetary projections is more appropriate and worth of the full focus of the paper.

While the results of this study are well presented and tell a nice story, the methods section is in my opinion very vague and it is hard to judge how suitable the model strategy is with respect to the analysis presented. The methods section would profit from specifications regarding model calibration and evaluation (was it calibrated at all?), an evaluation of the model simulations regarding the two drought characteristics deficit and duration (is the model able to well reproduce the phenomena studied?), a description of how the water demand estimates for the different sectors considered were derived (how was the disaggregation done?), and more information on the climate projections used.

The methods section was kept intentionally at minimum in order to maintain the focus of the readers on the results of the analysis. The LISFLOOD hydrological model that we used in this study has been applied in several pan-European studies on hydrology and climate. There is a large literature dedicated to several aspects of the modelling chain, including its calibration and validation. We will expand this section and better incorporate the relevant literature to accommodate your concerns.

I think that this study will be a nice contribution to documenting future changes in drought characteristics once/if the validity of the methodology is clearly demonstrated and the novelty of the paper is clearly worked out.

Specific comments

Introduction: I would strengthen the two novel aspects of the study and use them as a motivation for the study: (1) the drought modeling considers water use and (2) the future evolution of drought exposure is assessed. I would also address the topic of drought definition and already point out here that you are using a fixed threshold to define droughts.

We will reshape the introduction to better highlight the novelties of the study.

Methods: The methods section is in my opinion very vague and it is hard to judge the validity of the results in the absence of methodological detail. I suggest to address the following questions by making specifications accordingly... (follow a list of 16 questions).

We will incorporate the reviewer's suggestion by expanding the methods section, but also by better highlighting the relevant literature on each detail.

Results: The figures are clear and the results well presented. I think that the results section would profit from a display of the 'reference' situation and the seasonality of droughts over Europe (especially to highlight that drought seasonality using a fixed threshold will in Alpine regions happen during winter).

The language used is pretty deterministic even though the results of projections are presented. I would rephrase sentences such as 'will increase', 'will last',... to something expressing that these results are uncertain e.g. 'are projected to increase', 'are expected to last',...

We agree that projections of climate and consequently drought characteristics are uncertain. We will revisit the text to remove the instances where a deterministic language is misused.

Furthermore, it would just be interesting to present a few more results. Here, some suggestions for further analyses:

1. It would be interesting to see Figure 3 for two more return periods (e.g. 5 and 50 years) representing more frequent and rarer events, respectively to see how changes in frequency depend on the magnitude of events.

We observed that there is a rather strong relationship between the results at different return periods, as we were considering to incorporate the results for other return periods in the manuscript. We will add a figure summarizing these results, without replicating the same figure for different return periods, which may be too redundant and break the flow of the text.

2. It would be interesting to look at drought duration return periods and at bivariate return periods of deficits and durations.

As detailed above, we agree on the interest of the topic, but we consider the subject worth of a full paper that is currently under consideration.

3. It would be very nice if the model could be run another time without the human water use component/module to illustrate the impact of human impact on future changes in drought characteristics. Adding this aspect would make this a truly novel analysis.

As discussed above, this topic has been explored by other research studies, albeit with a less sophisticated modeling of socioeconomic conditions (e.g., Forzieri et al., 2014). Here we prefer to focus on the expected impact and exposure in case of climate inaction and different mitigation when the dynamic socioeconomic conditions are modeled at the best of our possibilities.