Review for manuscript "Diverging hydrological drought traits over Europe with global warming"

Authors: C. Cammalleri, G. Naumann, L. Mentaschi, B. Bisselink, E. Gelati, A. Roo, and L. Feyen
Journal: Hydrology and Earth System Sciences

Summary

Cammalleri et al. present a study on low flows/droughts in Europe under three different global warming levels (1.5, 2, and 3K temperature increase). They use the hydrological model LISFLOOD to simulate streamflow for a reference period (1981-2010) and for future 30-year periods corresponding to the three warming levels using an ensemble of future climate projections as model input. The model setup allows for the consideration of human water abstraction from the hydrological cycle. They determine the drought characteristics deficit, duration, and frequency for the reference and the future periods using a fixed drought threshold at the 15th flow percentile. Drought frequency is assessed using univariate return periods with respect to the deficit variable. The characteristics of the future periods are compared to the ones of the reference period. The study shows that the response of droughts to warming differs regionally within Europe. The Mediterranean and Atlantic regions are projected to be affected by longer, more severe, and more frequent droughts in future, while the Continental and Boreal regions are expected to experience shorter, less severe, and less frequent drought events. In addition to the drought characteristics assessment, the authors present an assessment of drought exposure with respect to population and agricultural area showing that the Mediterranean region needs to expect an increase in exposure both in agricultural land and population while the opposite is the case for the Continental region.

General comments

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. 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. 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. 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). 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. 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.

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:

1. Which quantile mapping approach was used? (l.102)
2. What is the reason for using the dataset EOBSv10 as an observed dataset? (l.103)
3. Was the assumption that 'between-pathway differences are generally much smaller than the within-pathway variability' verified for the drought characteristics analyzed? This assumption does not seem to be very intuitive to me. (107-109)
4. Was the LISFLOOD model calibrated, if so, how? If not, why not? (l.114)
5. How was the LISFLOOD model evaluated for the drought characteristics under study? Some evaluation plots are in my opinion required to prove the suitability of the model setup for the analysis performed (e.g. distribution of simulated vs. observed drought durations and deficits).
6. Is it correct that no downstream routing is performed but that the results presented are based on a grid-by-grid analysis of local streamflow generation? Please specify. (l.119-120)
7. How is irrigation water demand estimated? I.e. what does 'dynamically' mean and how was crop transpiration estimated? (122-123)
8. How was water demand estimated for the industrial, energy, livestock, and the domestic sectors? I.e. what was downscaled, how, and to which resolution (5*5km)? (125-126)
9. How are economic, budgetary, and demographic projections assumed to affect the individual water demand sectors considered? (l.126-128)
10. How does the Territorial Modelling Platform perform the downscaling? (129-131)
11. Why were the population and land use projections assumed to be static after 2050? (l.131-132).
12. What is the reason for the threshold choice? I assume that a fixed threshold is used? See e.g. [*Thiel et al.*, 2018] (l.138-139)
13. Did you do any smoothing to ensure the independence of events as e.g. suggested in [*Tallaksen and Hisdal*, 1997].

14. Why did you choose the Pareto Type II distribution to model drought deficits instead of the commonly used Generalized Pareto Distribution for partial duration series [*Coles*, 2001] and why is the threshold zero? Some goodness-of-fit test is required here (e.g. Anderson-Darling [*Chernobai et al.*, 2015]) (l.158-159).
15. How was the return period defined? (l. 163-165) The definition of a univariate return period of $T = \frac{1}{1-p}$ is valid when using annual maxima or annual minima time series. In the case of partial duration series as identified through a threshold level approach, the definition is $T = \frac{\mu}{1-p}$, where μ is the mean inter-arrival time between events (see e.g. [*Gräler et al.*, 2013; *Brunner et al.*, 2016]).
16. I am lost in the sentence in l.184-186. We would expect a 10-yearly event to occur on average every 10 years. This would expose all the people in the corresponding region to the event once every 10 years on average. How do you go to the assumption that one tenth of the population per NUTS 2 region is affected every year? I do not see the reasoning here because droughts are mostly larger scale phenomena and we can expect that most people in a region will be exposed at the same time instead of one 10th of the population being exposed every year.

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

The study reads generally well but would still profit from editing.

Minor points

- Title: I personally would use the word 'characteristics' instead of 'traits'. This comment applies to the whole manuscript.
- l.14: I would not talk about an index in this case. I would already point out in the abstract that you are looking at drought characteristics derived using a threshold-level-approach with a fixed threshold.
- l.15: I would mention the model name already in the abstract.
- l.22: by 'interested', do you mean 'characterized'?

- l.23: specify reduction in what? Drought durations, deficits, and frequency.
- l.27: by 'this', do you refer to 'the regions most affected by changes'?
- Keywords: I would add LISFLOOD and global warming level.
- l.41: Yes, but the drought definition chosen also depends on the question at hand/problem of interest.
- l.41-43: the sentence seems incomplete. Suggested rephrasing: droughts are commonly looked at from a meteorological (), agricultural (), or hydrological () perspective.
- l. 44-47: I agree that there are more studies on meteorological drought than on soil moisture or streamflow drought. But there are many more potential examples for hydrological drought studies, e.g. [Hao and Aghakouchak, 2014; Laaha et al., 2017; Brunner et al., 2019].
- l.48: by 'This', do you refer to the smaller number of non-precipitation drought based studies?
- l.48: I would challenge the statement 'meteorological drought indicators have lower input data requirements than streamflow or soil moisture drought indicators'. If one would like to compute the Standardized runoff index [*Shukla and Wood*, 2008] instead of the SPI, a streamflow instead of a precipitation time series is needed, which is the same amount of data, i.e. one time series.
- l.49: specify 'this'. The focus on meteorological drought?
- l.52-54: cite the European Drought Impact Report Inventory (EDII) here?
- l.60-62: sentence would profit from rephrasing.
- l. 62: There are some studies that have looked at drought characteristics on a European scale and expected changes, e.g. [*Marx et al.*, 2018; *Samaniego et al.*, 2018; *Brunner and Tallaksen*, 2019].
- l.64-69: I would split this long sentence into two.
- l. 74: is there a reference documenting this paradigm shift?
- l. 82: 90th percentile of annual minima? or of annual mean? Do you actually mean the 10-th percentile with respect to non-exceedance probabilities? The 90th percentile is more commonly used for floods but I am aware that the drought and flood communities sometimes follow different conventions. Statistically, however, it would be more correct to talk about the 10th percentile.
- l.89: which mitigation targets are you referring to here?
- l. 90: water 'availability' instead of water 'budget'?
- l. 92: I would specify the model name.
- l.90-95: split long sentence into two.
- L. 93: I would add a reference to [*Moss et al.*, 2010].
- L.102: Euro-CORDEX initiative.
- L.168-171: I would move this information to the introduction.
- 173-174: this statement is not very true for hydropower production, which mostly happens in mountainous areas which are not very densely populated. And it is neither true for ecological purposes which can also be highly impacted by droughts but not considered in this study.
- L.177: could you shortly describe the properties of the LUISA projections?
- L.178: what is the average scale of these NUTS 2 areas?
- L.182: do you mean to refer to a '10-yearly' event, i.e. an event with a return period of 10 years?
- Figure 3: I would add a vertical line at 10 years as a reference, e.g. in light grey.
- L.205:209: I would move this information to the introduction of the methods section.
- L.223: I would indicate the Seine river basin on one of the maps (for non-European readers).

- L.354-355: start new sentence?
- L.356: reduction in drought severity and frequency?
- L. 391-394: could you clarify this sentence?

References used in this review

Brunner, M. I., and L. M. Tallaksen (2019), Proneness of European catchments to multiyear streamflow droughts, *Water Resour. Res.*, *55*, 8881– 8894, doi:10.1029/2019WR025903.

Brunner, M. I., J. Seibert, and A.-C. Favre (2016), Bivariate return periods and their importance for flood peak and volume estimation, *Wire's Water*, *3*, 819–833, doi:10.1002/wat2.1173.

Brunner, M. I., K. Liechti, and M. Zappa (2019), Extremeness of recent drought events in Switzerland: Dependence on variable and return period choice, *Nat. Hazards Earth Syst. Sci.*, *19*(10), 2311–2323, doi:10.5194/nhess-19-2311-2019.

Chernobai, A., S. T. Rachev, and F. J. Fabozzi (2015), Composite goodness-of-fit tests for left-truncated loss samples, in *Handbook of financial econometrics and statistics*, edited by C.-F. Lee and J. Lee, pp. 575–596, Springer Science+Business Media, New York.

Coles, S. (2001), *An introduction to statistical modeling of extreme values*, Springer, London.

Gräler, B., M. J. van den Berg, S. Vandenberghe, A. Petroselli, S. Grimaldi, B. De Baets, and N. E. C. Verhoest (2013), Multivariate return periods in hydrology: a critical and practical review on synthetic design hydrograph estimation, *Hydrol. Earth Syst. Sci.*, *17*, 1281–1296, doi:10.5194/hess-17-1281-2013.

Hao, Z., and A. Aghakouchak (2014), A nonparametric multivariate multi-index drought monitoring framework, *J. Hydrometeorol.*, *15*(1), 89–101, doi:10.1175/JHM-D-12-0160.1.

Laaha, G. et al. (2017), The European 2015 drought from a hydrological perspective, *Hydrol. Earth Syst. Sci.*, *21*(6), 3001–3024, doi:10.5194/hess-21-3001-2017.

Marx, A., R. Kumar, S. Thober, O. Rakovec, N. Wanders, M. Zink, E. F. Wood, M. Pan, J. Sheffield, and L. Samaniego (2018), Climate change alters low flows in Europe under global warming of 1.5, 2, and 3 °C, *Hydrol. Earth Syst. Sci.*, *22*(2), 1017–1032, doi:10.5194/hess-22-1017-2018.

Moss, R. H. et al. (2010), The next generation of scenarios for climate change research and assessment, *Nature*, *463*(7282), 747–756, doi:10.1038/nature08823.

Samaniego, L., S. Thober, R. Kumar, N. Wanders, O. Rakovec, M. Pan, M. Zink, J. Sheffield, E. F. Wood, and A. Marx (2018), Anthropogenic warming exacerbates European soil moisture droughts, *Nat. Clim. Chang.*, *8*(5), 421–426, doi:10.1038/s41558-018-0138-5.

Shukla, S., and A. W. Wood (2008), Use of a standardized runoff index for characterizing hydrologic drought, *Geophys. Res. Lett.*, *35*(2), 1–7, doi:10.1029/2007GL032487.

Tallaksen, L. M., and H. Hisdal (1997), Regional analysis of extreme streamflow drought duration and deficit volume, *Friend'97 - Reg. Hydrol. Concepts Model. Sustain. Water Resour. Manag.*, *246*, 141–150, doi:10.1212/WNL.0b013e31823ed0a4.

Thiel, M. Van, A. J. Teuling, N. Wanders, M. J. P. Vis, K. Stahl, and A. F. Van Loon (2018), The role of glacier changes and threshold definition in the characterisation of future streamflow droughts in glacierised catchments, *Hydrol. Earth Syst. Sci.*, *22*(1), 463–485, doi:10.5194/hess-22-463-2018.