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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Climate change alters low flows in Europe under a 1.5, 2, and 3 degree global warming" *by* Andreas Marx et al.

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We thank the reviewer for the time and effort in commenting on our manuscript. We provide responses to each individual point below. For clarity, comments are given in normal font, and our responses are given as blue text.

The authors present a comprehensive study of change in low flows for Europe using downscaled GCM output fed into three different hydrologic models. I am happy to recommend publishing of the manuscript subject to maybe some clarifications.

This paper is looking at changes in the percentile (as the abstract says) ? but the introduction is focuses on droughts. As it is currently phrased I am not sure I feel comfortable with research Question 1. I think this should be changed to say it is looking at changes in low flows. The introduction needs some text to relate drought to low flows. I understand that at the bottom of page 4 it is stated that Q90 is the drought metric but this comes too late in the piece.
Thank you for this helpful comment. We agree to include the clear differentiation between the terms "hydrological drought" and "low flow" and we will adapt research question 1, accordingly. We suggest to include the paragraph:
"This study investigates low streamflow, defined as Q90, representing daily streamflow exceeding 90% of the time, which has the potential to impact hydrological drought.

Hydrological drought is associated with shortfalls on surface or subsurface water availability which can occur in low streamflow, groundwater, or reservoir levels. Changes in low flows shown in this study can, but will not in every case, result in drought. Exceptions are e.g. riverine based transportation, where streamflow values below a threshold level are defined as hydrological drought."

I think there are a few papers that could be cited in the introduction, for example, Hall et al. (2014); 10.5194/hess-17-325-2013; and a recent article that looks at the sensitivity of flows to temperature 10.1038/s41598-017-81-1084.
We agree to include 10.5194/hess-17-325-2013 in the introduction. Two other suggested paper are deemed beyond the scope of this manuscript.

I did find it odd that a lot of material was introduced in the discussion on Page 10 and Page 17/18. Given it is relevant I think the introduction needs to (at least briefly) incorporate these references to put this works novelty in context.
Thank you for your suggestion. We will extend introduction with the studies mentioned in the discussion.

Could the bias correction be elaborated in a sentence or two because the choice of bias correction can make a huge difference to the results? Especially if the focus is drought, authors need to correct for low-frequency variability biases - see 10.1016/j.jhydrol.2016.04.018.
There is a huge number of bias correction methods available, all facing advantages and disadvantages
(e.g. https://doi.org/10.1016/j.jhydrol.2012.05.052) for hydrological impact studies. The advantage of the method applied in our manuscript (Hempel et al.) is that it is trend preserving, which is of major importance for climate impact studies. We would have a different opinion, if this is meant by the comment that "authors need to correct

for low-frequency variability biases" in daily precipitation and temperature, because they are hardly directly linkable to low flow events.
We refer to the statement in in Donnelly et al (2017): "Cannon et al. (2015) and Maurer and Pierce (2014) showed that approaches like the quantile mapping used here can change the climate signal in the raw CM output significantly. Nevertheless, it is still unclear which methods give the most realistic climate change projections.".

Worth noting we are tracking for higher increases than 3 degrees probably: 10.1038/nclimate1783
Agreed.

Can the results in Table 1 be verbally contrasted with land predictions for Europe (i.e. will Europe heat up more or less than the global average). The IPCC reports will have this.
This is a good suggestion, but out of scope of this manuscript. Europe warms faster than the global mean, which has been visualised (for the underlying 5 GCM simulations) in http://edge.climate.copernicus.eu/Apps/#climate-change

I am pretty sure that the low flow statistics in Table 2 are based on average of all the grid cells in a region but I am not sure. This could be mentioned in the text.
We we will reformulate accordingly.

Figure 4 ? not really clear to me what the blue dashed line indicates. I think the lines need to be described in the legend.
Agreed. The dashed lines show two regressions (for positive and for negative deviations).

It is a bit hard to assess Table 3 because the step changes aren't linear. You could compare the following: Table1 Row 1 (0-1.5K) increase equivalent to 22, -7, -4, 8, -12 changes and comparing to Row 3 in Table 2 (again a 1.5 K increase but now from 1.5 to 3K) of 24, -13, -12, 23, -23.
For clarity, we suggest to include a row "absolute warming" (tab. 2: 1.04 K, 1.54 K, 2.54 K; tab. 3: 0.5 K, 1 K, 1.5 K). Comparison of results in tab. 2 and 3 rarely gives added value to the manuscript as changes are nonlinear with warming and regionally different - this is already reflected.

It was not clear to me how the GCM and HM signal-to-noise ratio was split.
The SNR was calculated for the combined GCM/HM runs (no splitting). If you refer to the GCM/HM uncertainty, the approach is described on page 8, line 2-8 in detail.

Abstract Line 5: Unprecedented is a strong word and I would remove it.
For Europe, there is no study available using a multi-model ensemble with 45 members including three impact models for low flows and at a high spatial resolution of $5\times$ 5 km^2. We think this justifies the usage of the term "unprecedented".

Page 8 Line 4: Typo. "...by first fixing a HM and then calculating the range of Q90 (max-min) corresponding to give GCM outputs and repeating the previous step ..."
Agreed.