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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.

General comments
This manuscript explores the impact of climate change on low river flows in Europe using a multi-model GCM and hydrological model (HM) ensemble under three global warming scenarios. The use of this ensemble allows the authors to assess the range of uncertainty in projections and the relative contributions of GCMs and HMs. Overall, it is an interesting and informative study, well-written and clear, supported by appropriate figures and references. There are some questions surrounding catchment selection for model validation and the general omission of smaller catchments, as well as the extent to which conclusions can be drawn on drought when analysing only flow percentiles. However, once these and some other interpretational aspects are addressed, I would recommend this study for publication.
Thank you for the overall positive feedback. From the altogether three reviews, we realised that the information given on calibration and validation needs to be extended in the manuscript. Smaller catchments have not generally been omitted. The catchment size at a horizontal resolution of 5 $\times$ 5 km^2 is limited by the DEM in determining the

catchment boundaries. Therefore, the results and conclusions in this study are based on catchments (or better river grid cells) with a contributing area >1000 km^2 have been used for the study, and these are shown in figure 3 and figure 5.
The selection of catchments >10000 km^2 in figure 1 and 4 in the first version of the manuscript has been done for clarity reasons. This will be changed for validation figure 1. Notably, for the validation (see attached figure) we selected 357 basins based on daily streamflow data availability (selection criteria; complete dataset of 30 yrs, 1966-1995, this time period would change (old: 1971-2000) because it resulted into largest sample size). Their median basin area is 1680 km^2.
Furthermore, the terms "hydrological drought" and "low flow" will be better specified and used in a coherent way.

Specific comments
Evaluating model performance (Page 4, line 30 to Page 5, line 4): I think more interpretation is required of Fig 1. There are only nine lines devoted to this, and I am not sure that I entirely agree with the assessment that "results show a good agreement" without some caveats. Low flows for PCR-GLOBWB and median flows for Noah-MP are systematically over-estimated across almost all catchment sizes, and there is a systematic under-estimation of low flows for Noah-MP. Whilst no-one is expecting perfect model results, there should be more attention given to the validation, as well as additional text in the discussion on the potential influence of model performance on the conclusions drawn.
Thank you for pointing this out. We suggest to include the following paragraph on the calibration of the hydrological models using observed meteorological forcing data (which focussed on headwater catchments). We suggest to include the paragraph:
"The three HMs used in this study were calibrated in nine European focus basins located in Spain, Norway and UK, which were selected based on the consultation with the user groups within the EDgE project. Besides these, we also included three more central EU catchments (located in France and Germany) to represent diversity

in hydro-climatic regimes. All HMs parameters were calibrated such that the model simulations represent a range of hydrologic regimes, rather than tailored to any specific characteristics. This was done in a consistent manner so that the model simulations can be used for a range of indicators (including high, low, and average flows) within the EDgE project. We recognize that HMs could be calibrated to a specific streamflow characteristic (in this case to low flows), but this was not considered within this study. We also note that the HMs do not consider human management effects in this study which could have substantial effect during the low flow times - as a result constraining the model to any specific low flow characteristic can result in a biased simulations. Also due to the similar reason we may expect a relatively lower model skill in matching the observed low flow characteristic."
The text "results show a good agreement" was written behind the background that GCM data for the time period 1971-2000, which differs from the observed weather in that period, was used to drive the HMs for the validation against simulated Q90 values. We agree that the discussion should be extended. Furthermore, based on Reviewer 3, Figure 1 will be re-drawn using specific discharge to remove the basin-scale dependency of the data (see above and attached figure). We acknowledge the adressed systematic biases and see only limited influence on the future results and conclusions drawn. It is not possible to determine if a model that fits perfectly in the past is also able to produce perfect results under changed climate conditions. Consequently, it cannot be concluded that imperfect models are not useable for estimating future (relative) changes.

Catchment selection for validation (Figure 2): There is no information on how or why these catchments were selected for validation. It would appear that a number of nested sub-catchments of relatively few large rivers have been selected (i.e. multiple downstream stations on the Rhone, Loire, Ebro, etc.) There is also no information on from where the river flow data were sourced. Data are freely available for some regions where the models are not evaluated but for which results are presented.

Agreed. The selection is partly explained in the general comments answer. We selected 357 basins based on data availability (selection criteria; complete dataset of 30 yrs, 1966-1995). Their median area is 1680 km^2. Fig. 2 will be adapted (see attached figure).

Omission of catchments <10,000km2 (Page 8, lines 13-15): Perhaps this argument explains the selection of catchments in Fig 2? I am not convinced that modelled data at 5km spatial resolution cannot resolve the river flow network of catchments <10,000km2. The authors highlight the "unprecedented" (Abstract) 5km spatial resolution and on a number of occasions highlight the "spatially explicit information" in this study, but removing smaller catchments seems not to capitalise on this. This section also says that such catchments will be removed, but the maps displayed in Fig 3 onwards all feature a river flow network which contains routed flows for catchments less than 10,000km2, in which the network appears to be relatively well defined. All of this is relevant also in relation to the comment above on model performance at the lower end of the flow regime across all HMs (Fig 1). Catchments <10,000km2 also omitted from Fig 4; are the results similar?
This is not the case, explanations have been given in the author replies above.

Drought or low flows (throughout manuscript): There is some inconsistency between the use of 'drought' and 'low flows'. This paper analyses changes in median annual Q90 flows, which allows conclusions to be drawn on climate change impacts on low flows but not necessarily drought. The authors use low flows and drought at times interchangeably, including in the research questions and conclusions.
Thank you for this helpful comment. We agree do include the clear differentiation between the terms "hydrological drought" and "low flow" and we will adapt research question 1, respectively. 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. These can occur i.e. in low streamflow, groundwater, or reservoir levels. Changes in low flow shown in this study can, but will not in any case, result in drought. Exceptions are e.g. riverine based transportation, where streamflows below a threshold level are defined as hydrological drought."

Robustness (Page 10, line 6): There is detail on the hotspots of changes in low flows, but in the end the low robustness means that for the Mediterranean / Atlantic, changes are not 'likely' (as defined by the authors) for most of these areas for either 1.5K or 2K. In fact, the signal for the Mediterranean might be stronger than that for the Atlantic, but it is less robust than the Atlantic. Statements like "Nevertheless, these results are not robust" (Page 13, lines 17-18) could be useful here.
We will modify the text as suggested.

Uncertainty from GCMs or HMs: There are a number of statements on Page 18 that need to be clarified in relation to Table 4. "HMs are the major source of uncertainty in the Alpine region" – GCMs and HMs are closer together in Alpine compared with other regions, but the numbers in Table 4 are similar for GCMs and HMs across all warming levels, and GCMs are higher for 1.5K. "The Northern area shows a nearly similar contribution in GCMs and HMs" – so does Alpine (see above), and GCMs and HMs are even more comparable for 2K and 3K in Alpine than in Northern. "In the Mediterranean, the uncertainty due to the HMs rises with increased warming" – this is true for all regions. It is also strong to say that GCMs "dominates" total uncertainty for Europe (Page 18, line 33), especially given the negligible differences between GCMs and HMs for two of the five regions.
Agreed. We will modify the text as suggested.

Technical corrections

Agreed. We will implement them in the revised manuscript.

Page 2, line 32: "differ- ent" to "different"

Page 4, line 11 - Page 5, line 4: Very lengthy paragraph could be better structured and split into multiple shorter paragraphs.

Figure 1: Useful to have a legend for colour based on GCM as there are some systematic patterns.

Figure 1 will be re-drawn (attached) to remove the basin-scale dependency of the data (based on comments of Reviewer 3, see above).

Page 11, line 9: "Q10" should be "Q90"?

Page 11, line 11: "to a large extent"

Page 15, line 11: Mediterranean should be "(-16%)" not "(-24%)", reading off Table 3 for 2K to 3K?

Page 17, line 1 (and throughout): "Targus" should be "Tagus"?

Table 4: It's more editorial, but Fig 6 discussed before Table 4 despite being featured afterwards.