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Comment on hess-2021-321

Anonymous Referee #2

Referee comment on "A large-sample investigation into uncertain climate change impacts on high flows across Great Britain" by Rosanna A. Lane et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-321-RC2, 2021

I reviewed the manuscript "A large-sample investigation into uncertain climate change impacts on high flows across Great Britain" by Lane et al. The main goal of this work is to present climate change impact projections on high flows for GB including climate model and hydrological model parameter uncertainties.

There is nothing substantially new in this study: national scale studies already exist, studies with more options considered for each step of the modelling chain exist, and studies with a more thorough evaluation of the uncertainties exist.

Overall, the study has some potential. Unfortunately, some methodological choices are questionable and do not reflect the state-of-the-art. Namely, the fact that one RCP only is used, one GCM only is used, that periods are not 30-year long and the reference period overlaps the "future" of RCPs is disturbing. Some figures are not properly introduced (I am not sure I understood well what is presented based on the captions).

To summarize, I must say I failed to learn new insight neither on a methodological point of view nor on the future of GB floods (as one GCM and one RCP are used only).

In addition, I second all comments from reviewer 1.

Below are my major remarks, as well as miscellaneous minor or moderate points to tackle.

Major remarks:

A single GCM is used. I was very surprised to discover that the authors made the choice of using a single GCM. That's a rather unusual set up for this analysis: GCMs account for a large part of uncertainties in hydrological projections!

The justification that comes at lines 145-147 (I understand it as the will to only sample the warmer range of possible climate outputs) is not convincing to me: another GCM, with a rather similar temperature pattern, might result in a very different precipitation evolution in the future - not all GCMs will result in intensified precipitation with similar spatial patterns, intensity and seasonality. As a consequence, the use of this single GCM might be interesting to estimate the warmer range of future temperature, but not at all to estimate the future range of hydrological variables!

Choice of the study periods. The authors state that "changes in flow metrics between the baseline (1985 –2010) and future (2050 –2075) periods were evaluated.". Why these choices? These are quite unusual for several reasons:

- the baseline contains 5 years with "future" GHG emission trajectories (2006-2010)

- the periods do not cover the classical 30-year period used to estimate climatology (WMO recommendation)

In addition, nowadays many studies assess the impact of climate change over the whole future period, using moving windows, which is a clear step forward as it allows to identify emergence times and trends.

Warmup period: Line 233: I find this justification rather disappointing. There are many ways to avoid throwing 5 years of climate data during the pre-RCP period (i.e. < 2005):

- using RCM data < 1981 for warmup (but I understand that it was not made available by Met Office, which is very surprising as most climate models usually start in 1950 or 1970 at least)

- using observed climate data from <1981 for warmup

- recycling RCM data from 1981-1985 for obtaining 1981 initial states

Partially wrong assertions: Line 331: This assertion is partially erroneous for me. The sources of uncertainties depend a lot on the indicator that is studied. This is not necessarily true for low flows, as shown in more recent studies as those cited by the authors. See:

- Vidal, J.-P., Hingray, B., Magand, C., Sauquet, E., and Ducharne, A.: Hierarchy of climate and hydrological uncertainties in transient low-flow projections, Hydrol. Earth Syst. Sci., 20, 3651–3672, https://doi.org/10.5194/hess-20-3651-2016, 2016

- or Parajka, J., Blaschke, A. P., Blöschl, G., Haslinger, K., Hepp, G., Laaha, G., Schöner, W., Trautvetter, H., Viglione, A., and Zessner, M.: Uncertainty contributions to low-flow projections in Austria, Hydrol. Earth Syst. Sci., 20, 2085–2101, https://doi.org/10.5194/hess-20-2085-2016, 2016.

- or Thibault Lemaitre-Basset, Lila Collet, Guillaume Thirel, Juraj Parajka, Guillaume Evin & Benoît Hingray (2021) Climate change impact and uncertainty analysis on hydrological extremes in a French Mediterranean catchment, Hydrological Sciences Journal, 66:5, 888-903, DOI: 10.1080/02626667.2021.1895437

In addition, RCPs also represent an important source of uncertainty that is not considered in this study

Miscellaneous:

Abstract: evolution of discharge is mentioned, but with no reference to the RCP that is chosen. Please add this information

Line 37: the authors often refer to rain or rainfall, and sometimes to precipitation. As GB is overall rather affected by snowfall, the term precipitation might be preferred

Line 69-70: This is not clear for me why the authors think (and state) that. Using a large sample of catchments results in a diversity of relationships between climate change and hydrological response (see what you say in lines 37 to 39).

Line 114-116: Now I understand better and interpret lines 69-70 differently. This needs to be clarified

Section 2.1 : RCP still not mentioned. The use of a single GCM is also not mentioned here.

Line 127: it is rather surprising to see quantile 50 introduced as a high flow metric.

Line163-166: RCMs are blamed for precipitation overestimation and PET variance overprediction. Since RCMs are forced by a GCM, this GCM could be blamed as well for these issues. I see no proof that this comes from RCMs only.

Line 222-225: Not 100% clear for me: does that mean that among the 35000 parameter sets, only 30 of them gave mean KGE above 0.8?

Line 232: Why these choices?

Section 3.1: This section deals with climatological indicators, not meteorological changes

Line 258: Figure 3 is cited before Figure 2. Please inverse the 2 figures

Line 343-344: The P-Q relationship seems rather linear to me in this graph, except for the highest increase values. Maybe develop how you identify a non-linearity on this plot, as I might have misread it.

Line 384: Not clear, what do you mean by large studies?

Line 435: this precision come 100 lines too late (see comment for line 331).

Line 446-449: RCPs are missing here

Figure 2: i) not quite sure why Npeaks is not shown here. ii) In addition, one row could be added to this plot: the position of observed indicator among the projected indicators OR the median error. The min and max errors indeed only inform on extreme errors. iii) For the observed-driven simulation part, I guess that the uncertainty bounds come from the 20 parameter sets, whereas for the RCM-driven one it comes from the 20 parameter sets * 12 RCMs. It is correct? Please precise in the caption. iv) This is again a quite unusual way to present results, as errors coming from the GCM-RCM chain are mixed with errors coming from the hydrological model. We usually separate these two sources: simulations of the model forced by meteo observations are compared to observed discharges - which qualifies the hydrological model error/performance-, and then only simulations of the model forced by GCM/RCM data over the reference period are compared with simulations of the model forced by observed meteo data - which qualifies the bias induced by climate models.

Figure 3 & 6 & 7 & 8 caption: please remind the periods

Figure 5 caption "the median flow value from the hydrological parameter sets": this is unclear, please reformulate

Figure 6 caption: Do you really average parameter values? It seems odd, as those parameter values would not be consistent together. The caption is insufficiently clear for understanding how this figure was obtained