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Discussions

Interactive comment on "A large set of potential past, present and future hydro-meteorological time series for the UK" *by* Benoit P. Guillod et al.

Anonymous Referee #2

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Dear Editor,

Comments to authors: Review of "A large set of potential past, present and future hydro-meteorological time series for the UK" (Manuscript number: hess-2017-246) submitted by BP. Guillod, RG. Jones, SJ. Dadson, G. Goxon, G. Bussi, J. Freer, AJ. Kay, NR. Massey, SN. Sparrow, DCH. Wallom, MR. Allen, JW Hall to Hydrology and Earth System Sciences (HESS; May 2017). This paper presents a new set of hydro-meteorological projections for the United Kingdom, based on a regional climate model driven by a global atmospheric model, which accounts for uncertainty in the climate system response by sampling a range of changes in the ocean state from CMIP5 models. This is really interesting papers, in particular as it describe a new methodology which could help in accounting better for internal climate variability, which is one of the

main source of uncertainty in the global/regional climate models, and which, at the local to regional scales, has been describe to be as important as anthropogenic climate change, even for intervals as long as the next 50 years in the middle and high latitude (Deser et al., 2012, 2013, 2014, 2016; Wallace et al. 2014, 2015), through the development of probabilistic scenarios for hydroclimate variables (including extremes such as drought) which could be used as input in a hydrological, ecological, agricultural models. It is therefore susceptible to interest a lot of researcher, and it is surely appropriated for publication in HESS. However, I have got some concerns regarding the absence of statistical analysis to test the significance of changes, and regarding the choice to only apply bias corrections to precipitation, while there is a clear significant bias in potential evapotranspiration (and probably in temperature). In addition, I feel like some results are a little over-interpreted, e.g. the raw (uncorrected) precipitation output performs better than bias corrected precipitation, as it can only be because the selected bias correction methods is not appropriated.

So my initial rank is to recommend major revisions.

Major comments: (1) Throughout the paper the authors are describing changes compared to a historical baseline, discrepancies between scenarios and while using different input (e.g. raw precipitation vs. bias-corrected precipitation), but none of these changes/differences are shown to be statistically significant. Although the novelty and the robustness of your approach in developing new hydro-meteorological projections is unquestionable, your interpretation of potential future changes in precipitation (including extreme events), potential evapotranspiration and temperature, or of discrepancies between scenarios, should still be supported be statistical tests (Student's t-test for changes in mean, F-test for changes in variance etc. . .) to make your results more objective or, at least, less subjective. In addition, as precipitation, temperature and evapotranspiration are likely to be auto-correlated, you might have to consider a test accounting for serial correlations in the time series.

(2) In Section 4.1.2 the authors decided not to apply a bias-correction to temperature

and evapotranspiration, as they consider their biases “relatively small”. However, I don’t think an overestimation of about 20% in evapotranspiration could be considered as small, and it could have a big impact on drought projections, as well as in hydrological and agricultural models which both used evapotranspiration as input. If this is really a small bias, it should be supported by a statistical test showing that bias is not significant, and that the results would have not been statistically different with or without applying bias correction. I agree there would always be large uncertainties when quantifying evapotranspiration, but, at least, this could be quantified (e.g. difference between different formulas, difference between raw and bias-corrected estimates), as it has been proposed in some studies (Sheffield et al., 2012; Zotarelli et al., 2013; Begueria et al. 2014; Raible et al., 2017). Overall, I would recommend to apply a bias correction to precipitation, temperature and evapotranspiration systematically, and then statically assess their potential differences.

(3) The choice of the bias correction is one of the main source of uncertainties while developing hydro-meteorological scenarios, and it different choices could lead to different results, more or less significant. The authors have chosen to use the simplest possible bias correction method, i.e. a linear bias correction, which is a fair choice knowing the large uncertainties related to the different bias correction procedures. However, at the same time, I would not recommend to conclude that the raw (uncorrected) precipitation output performs better than bias corrected precipitation, as another more sophisticated methods could have perform better (cf. Maraun et al., 2015; Maraun, 2016), and those uncertainties could have major implications, in developing scenarios for droughts or water resources (Clark et al., 2016). For instance, using a simple linear bias-correction, you should perform quite well in fixing the bias in the mean state, and probably the seasonal cycle (as it will captured most of the variance), but it might be totally unlikely to reduce the bias in interannual and/or decadal variability (while it is exactly where the climate models show the lowest skills; cf. Ault et al., 2012; 2013). This could explain why the bias looks proportionally stronger with a prolonged period of droughts in Figure 6. For instance, in a very recent study, Massei et al. (2017) proposed another

approach which could account better for low-frequency variability. However, I’m not sure this could applied in your study, and you must just be careful when interpreting your results, and should discussed more this issue.

Minor comments: P12-line 3-8: what would have been the bias if you would have used E-OBS for precipitation, and would have been the bias using E-OBS? Why did you not choose to keep the same data set for precipitation and temperature? P13-lines 29-30: It’s especially true for the long return period (prolonged periods of droughts), do you have an explanation for that? It could be because your bias correction is only performant for the mean state, and for interannual variability which would expressed most of the variance, while lower-frequency variability might be important for long return period.

P-14-lines 3-6: Return time plots of low precipitation amounts in 1 –4 consecutive hydrological years primarily showing you that as much you increased the length of the records, as more the model shift from observations, in particular for return period greater than 10 years. However, this should more accurately tested by considering more consecutive days, and then comparing the results.

P-15-lines 20-22: summer precipitation changes are most sensitive to the North Atlantic SST gradient, but is the North Atlantic SST gradient likely to increase in summer in the coming 50 to 100 years? It would be great to discuss it in the paper (even briefly). In addition, the North Atlantic SST gradient is closely related to atmospheric zonal circulation patterns, such as the North Atlantic Oscillation (NAO). It would therefore be interesting to discuss the potential implications if the summer NAO was becoming more positively/negatively persistent in the future. It could be an interesting discussion.

P16 lines 14-15: This suggests a change in the annual cycle, which would be more sensitive to changes in the North Atlantic ocean-atmosphere coupled variability. I should be discuss more in conclusion, for instance.

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