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Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving pan-european hydrological simulation of extreme events through statistical bias correction of RCM-driven climate simulations" *by* R. Rojas et al.

Anonymous Referee #1

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General

The paper describes the application of a previously developed bias correction method at the European scale using a new dataset. The paper is generally well-written and clearly structured, and the results are clearly presented and discussed. However, the results that are presented are not very surprising, because Piani et al, (2010) already showed that the method worked well at the global scale. Moreover, the current study does not use a validation period, as, for example, Piani et al., (2010) did (see below). This would assess the performance of the method much more thoroughly. Another addition that also would make the paper also much more interesting would be to compare the performance of multiple bias correction methods. As far as I know that has not been done very often (apart from maybe Themessl et al, (2010) for an Alpine region), especially not at the European scale.

I would like to see that the authors include a validation of the method, i.e., application to part of the dataset for which it was not calibrated, at least for some sub-areas. Other, more minor, remarks are outlined below. If the authors take these considerations into account I recommend accepting the manuscript for publication.

Specific remarks

Page 3889, line 20: "(e.g. 0.5 and 0.25 regular...)". "E.g." should be "i.e." I assume as there are probably not more than 2 resolutions. Please specify here which resolution you used. Furthermore, the E-OBS dataset is being repeatedly referred to as "high-resolution" (also in the Introduction). 25km, however, does not exactly seem high-resolution, given that e.g., Dankers et al., (2007) already applied HIRHAM and LISFLOOD at 12 km for entire Europe.

Page 3894, lines 13-18: As mentioned above, my main concern is the lack of a real validation period. The problem is that the "construction period" and "control period" are the same. Because the transfer functions are derived for 1961-1990 it is not very surprising the results are good for the same period. A proper validation would mean, for example, deriving TF's for 1960-1969 and validating them on 1990-1999 (as Piani et al., 2010 did). More generally, one of the methods outlined in Klemes (1986) should be used.

Page 3894, lines 18-22: Here, and also at various other places in the manuscript, it is stressed that the comparison between the control and future period are subject to the stationarity assumption. I appreciate that there are no real alternatives for this assumption, but it should be discussed that it is most probably not valid, as outlined by Christensen et al. (2008; reference already in manuscript), who found that biases

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depend on temperature and precipitation values and can grow under climate change conditions.

Page 3895, lines 16-20: The information provided about how LISFLOOD was calibrated is quite brief. First of all, 4 years is a short time to calibrate a model for such a long-term study. The chance that any hydrological extremes are included in such a period is small, although this is important as the parameters are assumed to be valid under changed climate conditions. Is the forcing data to calibrate LISFLOOD (the MARS database) purely based on observations or are also models involved? What's its resolution? Is there any information about how well MARS corresponds to E-OBS? Possibly the model calibration could be based on biased forcing data (wrt. E-OBS). Would it not have been possible to calibrate the model using E-OBS?

Page 3900, Figure 3: Although the wet day frequency is improved, the bias correction "overshoots" the observations, causing an underestimation throughout Europe. This is not commented on. Why is that? Similarly, at Page 3901, line 12 the authors note that 3, 5, and 7 day totals are underestimated in the after correction whereas they were overestimated before. Is there an explanation for that?

Page 3903/3904, Figure 8: The effect of bias correction on evapotranspiration is quite large. This is surprising considering that ET was calculated using Penman-Monteith, which is mainly radiation based and temperature only enters the equation indirectly through the vapour pressure deficit. As is explained in the manuscript, only temperature is corrected and all other variables remain constant. Do these changes in ET really come from the temperature correction only? In that case the spatial pattern in Figure 8 should be similar to the temperature patterns in Figure 6 and 7, whereas the latter seem much smoother. Is something else than temperature changing as well? Please explain.

Page 3905, Figure 10: It is not entirely clear to me what is plotted. Are these 30 year averages/maxima for all basins (554 points), or annual averages/maxima (30*554

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points)? Also, what do you mean by model efficiency, is that Nash-Sutcliffe coefficient? If that's the case, and the plot refers to 30 year averages, it does not make much sense to calculate a Nash-Sutcliffe because that indicates the efficiency with respect to an average. Or is it the model efficiency averaged over all basins? In that case, a value of 0.99 is not really credible, even when the temporal resolution is only annual.

Page 3906/Figure 11: It is not clear what the black crosses are exactly. They are said to be observations but in cases go up to a return period of about 100 yr, whereas about 30 years were available (page 3896)? If they are based on a Gumbel-fit, why are they not shown as lines? If much more than 30 years of observations is available for these basins, please mention that. Also, the numbers in Table 3 do not seem to be completely consistent with the crosses in Figure 11 – for example, the 100-yr discharge for the Daugava (misspelled as Daugana in Table 3 by the way) is about 6000 in Figure 11, and not even 4000 in Table 3.

Page 3906/Table 3: how is the percentage reduction calculated? I did not manage to get exactly the same numbers from the confidence intervals in the table. Sometimes close, but sometimes way off, e.g. for the Danube the reduction seems much more than 2.5%, and the Nemunas much less than 28.5%. If the Gumbel-fits themselves (ie not the confidence intervals) are used, please show those as well, and check for switched or miscalculated numbers.

Page 3908, lines 26-28: I do not agree with the notion that the future recurrence interval does only depend on the discharge magnitude change between control and future period (also mentioned in the Conclusions). The point is that the discharge magnitude is the same (and thus does not change) when you compare two recurrence intervals based on two Gumbel-fits. To obtain those Gumbel-fits, you do need time series of (annual maximum) discharge magnitudes, both for the control and future period. Whereas the authors seem to use the correct method for this comparison, it is explained rather confusingly. Please reformulate. However, because the time series are only 30-years, the 100-year discharges are based on very uncertain extrapolations as the authors correctly noted. It would be worthwhile to consider also shorter intervals (30 years or less) that are less prone to the uncertainty of the fit.

Page 3908: The grammar here gets a bit sloppy, e.g., "..bias-corrected driven simulations" should be something like "simulations driven by bias-corrected forcing data". Similar formulations occur several times, and a few more examples (not all) from other sections are noted in the Technical comments. Please check for grammar mistakes.

Technical comments

Page 3884, line 6: "Predictand" is not the correct term here, as no prediction is being carried out, and, more importantly, the observations are the target and not the variable that is to be predicted.

Page 3887, lines 18-27: This is a very long and confusing sentence. Please split into segments (e.g. for each method) or indicate the methods with i), ii), iii) etc.

Page 3902, line 18: "is A function of..."

Page 3892, line 8: Xcor and Xsim were explained before and can be removed here

Page 3893, line 4: "functions are obtainED for ..."

Page 3893, line 8: "weighted linear interpolation ..." weighted based on what?

Page 3894, line 12: 5 and 0.2 should be reversed.

Page 3895, line 24: remove "on" in "discuss on the calibration..."

Page 3896, line 9/10: "series of daily discharge maps for each river pixel"...this is a confusing formulation. I suppose you have time series for all river pixels?

Page 3902, line 18: Tmin and Tmax were corrected indirectly, but the range was corrected for directly. Hence the improved diurnal range is not very surprising.

Page 3906, line 11: "...explain why () the annual maxima FOR the greater part of the stations FALL below..."

Page 3907/Figure 12: The comparison of corrected and uncorrected in text and Figure are opposite I.e, the figure shows uncorrected/corrected (although this is not indicated), whereas the text mentions the corrected values being higher north of the Carpathians. It would be logical to show corrected/uncorrected in Figure 12, in order to present it such that the ratio north of the Carpathians is higher as well.

Page 3916, line 12: The journal for this reference should be JGR, not GRL

References

Klemes, V. (1986), Operational testing of hydrological simulation models, Hydrological Sciences Journal, 31, 13âĂŤ24.

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