Hydrol. Earth Syst. Sci. Discuss., 12, C5904–C5911, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C5904/2016/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "High-end climate change impact on European water availability and stress: exploring the presence of biases" *by* L. V. Papadimitriou et al.

Anonymous Referee #2

Received and published: 4 January 2016

In this manuscript, the authors assessed (the change in) average and low flow conditions and drought climatology in the European region for the period 1971-2100 using high-end scenarios of climate change. Moreover, the authors evaluated the impact of bias correction on projected hydrological simulations using two observational datasets: WFDEI and E-OBS.

The paper addresses relevant scientific questions within the scope of HESS. Although evaluating the impacts of climate change on average and low flow conditions and drought is not particularly new in general, it is using the JULES dataset in combination with the Euro-CORDEX projections. Moreover, the focus of this manuscript on the im-

pact of bias correction and different bias correction datasets on projected hydrological simulations is a fairly new and emerging topic. Overall, the paper reaches some substantial conclusions which are supported with figures and results in the results section and discussed in the discussion section. Therefore, I would support the manuscript for publication but with substantial revisions taking into account the following general and technical comments/suggestions:

General comments:

1. The title of the manuscript does not reflect the general topic of the manuscript. Also within the main body of text, the authors mix up the definition of water stress (which is a function of demand versus supply) with the definition of hydrological drought/low flows. I suggest the authors to stick to the definition of average and low flows/runoff and hydrological drought throughout the manuscript.

2. The introduction section is too long and consists of redundant information. I takes too long for the reader reaches the main goals of the manuscript. Please remove redundant text and restructure this section please, specifically related to: a. Flooding (not studied in this paper: can be removed, also in the discussion section) b. Comparison of all the GCMs/LSMs: keep it short and focus on results for the EU continent. Do not present everything here, might be more appropriate in the discussion section. c. Do not mix up the concepts of drought and water stress: focus on drought also when referring to literature in this section. d. The model JULES is now explained both in the introduction and in the methods section. I would suggest to replace the majority of this piece of text to the methods section. Only briefly mention JULES in one/two sentences in the introduction. e. Use consistent namings: e.g. when referring to the climate change scenarios +2/+4 degrees global warming. f. Please rephrase the research goals. The goals in itself are fine but they could be defined more precisely.

3. One of the goals is to evaluate the average runoff and low flows under a +2 and +4 degrees global warming scenario. I would suggest therefore to add to table 1 the years

in which a +2 degree global warming is reached for each of the GCMs.

4. Only 2 of 5 models reach a 4 degree global warming before 2100, can we really speak of a 4 degree global warming scenario then? And is it fair to compare the output of these GCM modelling results with each other or to estimate and ensemble-mean value? Please elaborate.

5. The +2 and + 4 degrees refer to a 'global warming condition' whereas this study looks specifically to the European conditions. Could you elaborate a bit more on the temperature differences (and differences in precipitation accordingly) between the GCMs for the European continent when using the +2/+4 global warming scenario time slices? How could these differences influence your analysis/results?

6. In this study, only the JULES model is being used for hydrological simulations. I would suggest the authors to elaborate a bit more on the performance of JULES compared to other models, both in the baseline situation and given the future simulations. Moreover, it would be good to show/discuss how well the JULES model matches observational data, preferably with a focus on the pan-European continent.

7. The authors used to hydrological indicators to identify changing climate trends, the average and 10th percentile of runoff production. Reading the manuscript, it did not became clear to me however how the authors applied these indicators. Did they use monthly or yearly values? And if they used monthly values, did they used a variable threshold approach to estimate the 10th percentile values? Or did they use a fixed 10th value over all months? Please clarify.

8. With respect to the examination of drought climatology the authors explain that they 'counted the number of days per year that extreme lows in flow occur'. First, using a 10th percentile value is not yet really an extreme low (I would say extreme lows would be using a 5th percentile or 1st percentile). Secondly, I'm wondering whether the authors used any buffering methods (defining minimum/maximum inter-event times or minimum length of dry conditions in order to be considered a drought) to estimate

the drought climatology, see for example Tallaksen et al. (1997) and Sung and Chung (2014). Thirdly, could the authors argue why they used a 'total number of days per year with extreme lows', rather than a 'total/max number of consecutive days with extreme lows', which might be a more appropriate indicator for drought climatology. Finally, Wander et al. (2015) argue that – when evaluating drought conditions under climate change- it is better to use a transient variable threshold approach as (aquatic and terrestrial) ecosystems are able to adapt to changing drought conditions. I would suggest the authors to at least discuss the use of this transient variable threshold here or in the discussion section.

9. Please mention that the results presented in section 3.1 refer to the use of the Euro-CORDEX data without bias-correction.

10. Section 3 is quite wordy about (significant) positive and negative trends to describe the changes in average and 10th percentile runoff values towards future conditions. However, only few statements are actually backed up with numbers/statistics. I would suggest to execute some extra statistical analyses to give your results some extra body. E.g. the trend observations could easily be backed up with a simple regression analysis giving a 'number' to the trend (coefficient) and a feeling of (in)significance of the result (R-squared and significance level of the estimated coefficient in the regression). Outcomes could be mentioned briefly in text – e.g. between brackets- and in the figures. 11. I miss in the discussion section a piece of text elaborating on the use of JULES, the performance of JULES and the potential use of other GHMs/LSMs.

Specific/Technical comments:

12. 'GFDL and NorESM1 exhibiting generally wetter patterns' (p 7281, line 8-9). Looking at figure 2 I would say that NorESM1 is also generating relatively dry patterns for southern Europe.

13. 'all models agree' (p 7281, line 11 & 20): I would suggest to add a sub-figure to figure 4 & 5 that shows the modelling agreement with the ensemble mean in terms of

estimated change (+/-) in average/10th percentile runoff.

14. 'with MIROC5 being the only ensemble member that expands this wetter climate down to central Europe': Looking at figure 3 I would argue that HadGEM2-ES is also showing some significant expansion of this wetter climate down to central Europe.

15. 'Thus averaging . . . projected changes' (p 7282, line 2-3): Isn't this always the case with taking an average ensemble-mean?

16. 'making it easier to identify clear patterns of change' (p 7282, line 3): Is this really the case? I would argue that an ensemble-mean might be useful but that it could also create pseudo-results (the average value is not per se the true value namely), therefore it is important to consider the full spread of GCM-forced outcomes as plausible results (unless you have information on the reliability of the different GCM-forcings).

17. 'For 10th percentile . . . part of Europe' (p 7282, line 16-19): Could you argue why this is the case?

18. 'bias adjustment of the forcing data resulted in a drier hydrological response from the JULES model' (p 7282, line 24-25): Could you support this statement with some numbers? E.g. xx % of the total pan-European land surface area shows a drier output using the bias-corrected forcing data compared to using the non-bias corrected forcing data, xx % shows insignificant change, xx% shows a wetter output.

19. 'with increases in runoff in northern Europe getting more pronounced in the runs after bias correction (p 7282-7283, line 28-1): does this hold both for the absolute and percentage change? Or only for the percentage change? And should in that case the difference be considered as a results of the baseline values becoming reduced in magnitude?

20. 'sign change' (p 7283, line 2): how about significance of the values? Doesn't it just all fall under 'insignificant change'?

21. 'bias correction has . . . model agreement' (page 7283, line 3-4): That makes sense

as we bias-correct all forcing data-sets using the same WFDEI data-set. Point for discussion should be whether this is actually a desired outcome (all outputs merging towards 'one single line'). How big is the confidence in the WFDEI data-set, for example?

22. 'For the baseline . . . southern Europe' (p 7283, line 6-8): Could you explain/clarify this?

23. 'basin average runoff production' (p 7283, line 20): Please clarify how you got these averaged values. Did you first averaged all runoff values and afterwards took the average and 10th percentile? Or did you basin-averaged all average (temporal) and 10th percentile values?

24. 'A common observation . . . input forcing' (p 7283,26-27): These decreases are really large. Did you find any corresponding decreases in the literature, or did you check the values with observed time-series (from gauges). Could you somehow explain these large decreases?

25. Figure 9 is not correct. The figure shows twice the results for the Danube and Rhine whilst the results for the Elbe and Guadiana are missing.

26. 'the effect of climate warming is far more pronounced for the low flows', 'significantly' (p 7285, line 16-17): Is this really significantly? Did you tested this? Please use some statistical methods to support these statements.

27. 'there is a significant decrease from 0 to +2 C' (7285, line 23): How did you estimated the values for 0 degrees Celsius warming as they are, following figure 10, not equal to the baseline values. Please, clarify this in the methods section.

28. 'probably due to its very low values of 10th percentile runoff' (p 7286, line 7-8): In how many of the GCM runs you reach zero flow? And how reliable is zero flow for this river? Please mention this in the text.

29. 'E-OBS corrected data . . . the observed values' (p 7287, line 11-14): Could you think of an explanation for this observation? Are there any differences between E-OBS

and WFDEI that might clarify this result?

30. Section 3.6 focuses on the basin averaged average runoff whilst I think it is (more) interesting to show also the results for the 10th percentile runoff and using not the basin-averaged numbers. Optionally figures could be placed in a supplementary.

31. Could you elaborate a bit more on how the differences between the two bias-corrected data-sets and their hydrological output develops, comparing the difference in input data (precipitation, temperature) with the order of magnitude differences in hydrological outputs (local and routed runoff)?

32. 'it is . . . model agreement' (p 7287, line 14-15): Incomplete sentence

33. 'changed' (p 7288, line 9): change

34. 'it is . . . climate change' (p 7289, line 1-2): please leave out as this is only deduced and not studied.

35. 'It should be . . . average-state' (p 7289, line 4-5): Please back-up with some references.

36. 'of' (p 7289, line 15): delete

37. 'remarked' (p 7291, line 25): remarkable/significant

38. 'thus' (p 7292, line 1): and

39. 'are expected' (p 7292, line 2): please remove floods. Moreover I wouldn't say this so frankly, replace with: 'could be expected'

40. 'two degrees warming' (p 7292, line 12); +2 SWL (p 7292, line 14): please be consistent in naming

41. Table 1: (1) All RCMs are RCA4 - column could be deleted. (2) Is it of interest to present the equilibrium climate sensitivity?

42. Table 2 and 3: Absolute and percentage change to what? Please clarify in table or

heading.

43. Please merge figure 2 & 4 (3 & 5): add one/two rows to figure 2 and 3 to show the ensemble-mean changes and the modelling agreement.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 7267, 2015.