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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The critical role of uncertainty in projections of hydrological Extremes" *by* Hadush K. Meresa and Renata J. Romanowicz

Anonymous Referee #1

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General comments

This paper is about the uncertainty of extreme flows with climate change. For that purpose, the authors use seven combinations of global climate models (GCMs) and regional climate models (RCMs) with one greenhouse gas concentration scenario to represent uncertainty in climate change. Furthermore, they use the GLUE method to represent hydrological parameter uncertainty and uncertainty in extreme value distribution parameters to represent the uncertainty in the statistical extreme value distribution. These three sources of uncertainty are investigated using the HBV hydrological model applied to a medium-sized Polish catchment.

Although the topic is interesting and relevant for this journal, the paper is moderately

written, lacks clarity in parts of the methodology and only briefly discusses results and insufficiently puts outcomes into perspective. For instance, the seemingly arbitrary choice to consider the three uncertainty sources is not justified. Are these three sources the most important ones or the easiest ones to quantify? Furthermore, the uncertainty due to the use of a particular extreme value distribution is not clearly and completely incorporated. A final example is the presentation and analysis of results, such as the analysis of annual maximum precipitation and temperature in relation with annual maximum flows and in particular annual minimum flows. In this case and several other cases it is not always clear which results are shown, why they are shown and what can be concluded from the results. Many other specific (and important) comments can be found below. Furthermore, the English writing style and grammar is moderate (including several typos); some examples can be found in the section 'technical corrections'.

Specific comments

1. P1, L7-9: It is not clear what is meant with a 'multi-model approach' and which steps are followed.

2. P2, L9-11: The first question probably is related to the magnitude of the uncertainty, since this is still largely unknown and not systematically investigated.

3. P2, L15-16: "... can never be accurately evaluated ..." is a very strong statement, please rephrase.

4. P2, L24-P3, L2: The authors mainly consider hydrological model and parameter uncertainty in their review. It might be worthwhile to firstly give an overview of all uncertainties involved in this type of studies including a classification. One such classification could be input, (hydrological) model system and output, and the literature can be reviewed accordingly. Now, uncertainties in the input (scenarios, GCMs, RCMs, downscaling, initial conditions etc.) are hardly reviewed. A complete overview of the uncertainties will also enable a better justification of the uncertainty sources considered

in this study (see also page 3, lines 4-5).

5. P3, L14-15: The question is whether you can determine the uncertainty due to the choice of the extreme value distribution ('distribution fit') using time series of different lengths. When assessing effects of time series with different lengths on the results you might get an estimate of the influence of data quantity on the uncertainty in the results, but not of the influence of the goodness-of-fit of the distribution on the uncertainty. Furthermore, it seems only part of the statistical uncertainty is assessed in this way, since for instance the influence of different extreme value distributions and extrapolation uncertainty is not taken into account.

6. P3, L29-30: How many precipitation stations have been used to assess the catchment average precipitation (assuming lumped hydrological modelling has been carried out)? Has any elevation (or other) correction been incorporated?

7. P4, L11: An important uncertainty source in climate impact studies is the uncertainty due to greenhouse gas emission scenarios. Hence, a limitation of this study is the use of only one emission scenario (RCP4.5) while one would expect the use of at least two scenarios (which are available in EURO-CORDEX). At least the authors should explain the implications of this limitation for their results.

8. P5, L9: Why is QM applied in this study? The reasoning behind this choice is not completely clear from the preceding sentences.

9. P5, L18-19: Did Osuch et al. (2015) model the same catchment as in this study and therefore, can it be assumed that the same five parameters are sensitive? And are the same five parameters sensitive for low flows and for high flows? That would be remarkable.

10. P6, L15-16: How many Monte Carlo simulations have been executed and is this number sufficient (compare with literature)?

11. P6, L22: Is it common practise to determine the thresholds in an iterative way? The

determination of the threshold based on the requirement that 95% of the observations should be in the 95% confidence interval seems to be reasonable. However, please refer to other studies employing the same approach.

12. P7, L4-5: In general it is doubtful whether distributions with a 'large' number of parameters will model data in a more accurate way than distributions with a small number. This partly depends on the data quantity and quality and similarly as in hydrological modelling there will be a balance between the complexity of the distribution (i.e. number of parameters) and the amount of data (and quality).

13. P7, L7-8: What does an 'overall good performance' mean? Compared to which other distributions?

14. P7, L25-27: It is not completely clear why the analyses are performed for a period of 130 years. Since the manuscript is about impacts of climate change on hydrological extremes, you would expect a comparison between historic and future climate conditions. Furthermore, climate change automatically implies the existence of non-stationarity and as such, by considering a period of 130 years assuming stationarity by using the same extreme value distribution will result in serious flaws.

15. P8, L7-12: The idea behind this section is not clear. Why is the trend in daily annual maximum precipitation and temperature analysed while the interest is in uncertainty in hydrological indices with climate change? Moreover, why is the daily annual maximum precipitation of interest and not for instance the two-day or three day precipitation (which might be stronger correlated to annual maximum discharge values)? Which temporal resolutions of precipitation are relevant for annual minimum flows? And what is the supposed role of daily annual maximum temperature values?

16. P8, L14-20: How have the different criteria for high and low flows been applied in continuous hydrological modelling for periods of 30 years (or more)? When is the 'high flow' parameter set being used and when the 'low flow' one? What is the threshold for low flows and high flows; a specific discharge value or exceedance frequency?

17. P9, L5: Which best parameter sets are meant here? When is the best low flow parameter set used and when the best high flow parameter set?

18. P9, L7-8: 'twice as large'; where do we see that?

19. P9, L12-22: This evaluation is not clear to me. Why do the authors evaluate results at a monthly scale? How can you assess annual maximum flows for each month? What do the authors mean with 'range' of annual maximum flows?

20. P10, L9-10: The decrease in the spread of Q30 in the far future compared to the near future is strange. The authors should reflect on this. Is it related to the fact that only one RCP scenario is taken into account?

21. P10, L20-22: Also this observation needs discussion. Why the spread is more evenly distributed for minimum flows compared to maximum flows?

22. P11, L13-14: Are the relative differences for annual minimum flows also smaller?

23. P12, L7-9: This is an interesting topic, but has not been investigated in this study since only one catchment has been considered.

24. P12, L11-14: This is an interesting result assuming that all methodological steps are logical and correctly carried out. What is the reason for the importance of uncertainty due to climate models for high flow and the important of hydrological model parameter uncertainty for low flows? This is very important and interesting to discuss.

25. P12, L23-24: What do the authors mean with 'this allows the problem of nonstationarity of model parameters to be avoided'?

26. P12, L29-31: This statement seems to be obvious; the larger the ratio of return period vs. data length the higher the uncertainty. However, this extrapolation uncertainty is not explicitly assessed in this manuscript.

27. P23, Table 2: The ranges defined by the lower and upper bounds frequently do not match with the optimal values (e.g. for ALFA, PERC, CLFUX). Can you explain this?

Furthermore, some lower and upper bounds are exactly the same. Does this indicate that these parameters are deterministic? What about CFMAX (not mentioned as sensitive in section 3.3)? Finally, an upper bound of 2 for LP is impossible and an optimum value of 1 is remarkable at least (it would mean only potential evapotranspiration under fully saturated conditions).

Technical corrections

1. P1, L11: What is the distribution fit?

2. P1, L13: What kind of weighting do the authors mean?

3. P1, L16: What is the difference between climate model variability and climate projection ensemble spread? Please use a consistent terminology.

4. P2, L3: What is inverse modelling in this respect? Is this term commonly used for calibration and validation purposes based on observed (historic) data?

5. P2, L6: "weighting" instead of "weighing".

6. P3, L8: What is the 'relevant variability' of extreme index estimates?

7. P3, L19: The case study has already been mentioned.

8. P3, L30: The maximum daily precipitation? During which period?

9. P3, L30-31: Which period for the streamflow) Isn't 0.4 m3/s a very low value for catchment area of about 1000 km2?

10. P4, L12-14: Why do the authors use these complex abbreviations for the GCM-RCM combinations? It is not clear what the meaning of all the numbers is. Try to be consistent with the descriptions in Table 1.

11. P5, L12: Do you have a reference for the Matlab version of HBV?

12. P5, L15-17: Only 12 out of 14 HBV parameters are mentioned. In which routines can we find CFLUX and PERC (see line 19)?

13. P5, L17: ‘routines’ instead of ‘routing stage’?

14. P6, L24-P7, L3: This general description of the GEV distribution is not necessary here and can be found in many text books.

15. P7, L16-17: What do the authors mean with “. . . aggregated speared of flow quantile change . . .”?

16. P7, L19: ‘squared’ instead of ‘squere’.

17. P7, L22: The title suggests that the results of this study will be described. Please rephrase the title.

18. P7, L23: Different temporal resolutions? Shouldn’t it be different lengths of data periods?

19. P7, L18: The meaning of all variables should be explained in the text.

20. P8, L6: “Results and discussion”?

21. P9, L2: ‘the 10-year moving average from the ensemble mean’?

22. P9, L15-16: Fig. 5a is mentioned twice.

23. P9, L29-30: Decreases in minimum flows and increases in maximum flows? Shouldn’t it be the other way around (according to the caption of Fig. 6)?

24. P10, L6-7: Here, the annual minimum flows increase (see previous comment).

25. P10, L9: What is Q30? Commonly, that is a discharge with a non-exceedance frequency of 30%. However, here it seems to be an annual maximum flow with a return period of 30 years?

26. P11, L7: ‘Table 4’ instead of ‘Table 3’.

27. P11, L26-P12, L2: The first part of the conclusions can be omitted (can be part of introduction section).

28. P12, L9: ‘hydrological parameter uncertainties’ instead of ‘hydrological model uncertainties’?

29. P12, L24-27: This is a repetition of lines 11-14.

30. P13, L3: A paper in preparation should not be included in the reference list.

31. P13-17: The reference list and referencing contain many errors, typos and inconsistencies. This should be carefully and thoroughly double-checked.

32. P18, Fig. 1: What is the unit of the DEM map?

33. P18, Fig. 2: The interquantile range of what? Of the seven GCM-RCM combinations? In that case it would be better to show the individual model results, i.e. one annual maximum for each combination so 7 points per year.

34. P19, Fig. 3: In particular the scale of the upper panel looks strange. Flows in cubic mm? How accurate is your model? Please use the same (realistic) x-axis ranges.

35. P19, Fig. 4: This figure (and also Fig. 2) is too small. What do we see here?

36. P20: The differences between historic and future periods cannot be clearly seen in these figures.

37. P21, Fig. 6: What are the different lines in these figures? And is baseline and reference period the same?

38. P21, Fig. 7: In the caption ‘right hand panel’ is mentioned twice.

39. P22, Fig. 8: Idem, annual minimum flow is mentioned twice.

40. P23, Table 1: Which meteorological institute is connected to RACMO?

41. P23, Table 2: The caption is not clear.

42. P24, Table 4: What do the authors mean with ‘change in width of . . .’? What compared to what?

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