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Hydrology and Earth System Sciences Discussions

Interactive comment on "The impact of climate change on hydrological patterns in Czech headwater catchments" *by* A. Benčoková et al.

Anonymous Referee #2

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This paper describes a case study of applying two GCMs and two scenarios, downscaled through one RCM. The methods applied are not novel, and the analysis does not account for all uncertainties involved, neither does it fully explore the possible future changes in the catchment. The conclusions are therefore to my judgment not supported by the analysis. I recommend that the author carry out a more rigorous test of the modelling chain in order to support their claims. My recommendation is that the paper is not suitable for publication in HESS in its current status.

General comments

My main concern with this paper is how the RCM is applied. Firstly, the project only uses data from one RCM driven by two GCMs. Why is this? It has been shown in the PRUDENCE and also ENSEMBLES projects that the individual RCMs add to the

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uncertainty of the climate model output and have different biases. It is therefore advisable to use more than one RCM, especially when they are easily accessible through portals.

Secondly, the authors have used only on RCM point for future climate studies. The discrepancy between the area an RCM grid point covers and the areas of the headwaters is very large, and one single RCM grid point cannot be seen as representing the weather at these small catchment sizes. The signal to noise ratio has been shown to be low for individual points (Fowler et al 2009). An approach to increase the sample size of the RCM, and therefore increase the signal to noise ratio is spatial pooling, where the surrounding grid points are used as well as the nearest (Kendon et al, 2008).

Thirdly, no estimate of the uncertainty in the modelling process is attempted. Hydrological modelling itself is subject to huge uncertainties both in the driving input data, model structure and parameter values. Then, in the context of climate change impact studies the uncertainties from the climate model output has to be added. Although the authors apply two different GCMs and scenarios, there is no rigorous assessment of uncertainty, for example the interaction between the driving data and the hydrological model parameters. A sensitivity study with a simple model as the Brook90 would be fairly straightforward, and I strongly recommend this.

The paper is not very well balanced. The result section is too long, and can be heavily shortened, and a lot of the results discussed can be visualised in figures and tables instead.

The claims in the discussion and conclusion are not backed up by the analysis (see specific comments). It does not mean that they are wrong, I find them to be likely to be true, but the analysis is too weak to support them.

Language: The grammar is not up to standard for a scientific publication in English. I suggest that the authors have a native English speaker go through the manuscript.

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Specific comments

1. P1247, L1-4. You make references to the PRUDENCE project here, but the latest RCM runs for Europe are provided by the ENSEMBLES project, which I would suggest you refer to as well. See http://ensembles-eu.metoffice.com/ where you can find more references. Also, you mention the typical resolution being 0.5 degrees (L13), but the new runs are available at 0.22 degrees

2. Section 2. I would suggest that you describe the Brook model with a figure showing the different processes

3. In section 2.4, it is a bit unclear how you evaluated the calibrated model. How did you use the visual inspection? Was NS-used on monthly values, or was it used on daily values which where then aggregated to monthly values? Could you please be a bit more specific in this section.

4. P1252, L1-5. What do you mean here with the sentence that starts "This model emphasizes"...? Also the next sentence is very unclear.

5. P1252, L6. You state that "The RCAO model uses large-scale lateral boundary conditions from two GCMS,..." Yes, in this particular setting, but the model can use boundary conditions from any GCM. Please rephrase this

6. P1254, L4-5. The authors use a simple monthly "bias-correction" to get the monthly precipitation modelled by the RCM to compare with observed. However, the method used in the Schoof paper which is referred to seems to use a quantile mapping, which is not the same thing. Can you describe the method more in detail please? Also, I would suggest using the term "error correction" or "model output statistics" instead of bias correction, unless your method explicitly corrects the bias.

7. Section 2.7 is confusing. On which data set did you test for trends, and what was the reason for doing this? The explanation comes later in the paper, but please state here on what you plan to apply the test on.

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8. Section 3.1. Here you show the performance of one single model run with the Brook90, but it would be interesting to see whether the parameter values are robust, I e if there are other model parameterisations which yield similar results, and if the results for 1996 are bad for all model realisations.

9. Section 3.2. I would suggest showing the biases in a figure rather than just explaining them in the text. You could then at the same time show the effects of the error correction.

10. Section 3.4. You find significant trend both in temperature and precipitation on a monthly basis between control period and the near present. This implies that the climate is not stationary, and this will affect your error correction, since a common assumption is that the applied correction factors are stationary. This need to be further discussed.

11. P1260, L12-16. I do not agree with this conclusion. With the bias-correction you have shown that the observed monthly values are similar to RCM after error correction, which is to expect. This does not imply that they represent the long-term averages since they are on totally different spatial scales.

12. P1260, L20-21. Also here I disagree with the conclusion. Nowhere in the paper have you showed that future patterns of flow can be assumed to be captured. To do this you would need a sensitivity analysis and show that the model is robust enough to handle such huge temperature increases as predicted.

13. P1260 L26- and onwards. The discussion about the difference between the monthly averages in comparison with annual averages is not very useful in light of the uncertainties involved. A mean underestimation of 2% in the annual runoff is most certainly within the observed uncertainty ranges.

14. P1261. L14-24. The large change in temperature would surely result in a large change in the evapotranspiration, but the model does not account for land-use

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changes, so the changes in ET are very uncertain.

15. P1262. The changes in winter runoff are probably more robust than summer and autumn, but I would refrain from using such exact numbers as 69%.

16. P1262, L26. With “redistribution”, do you mean seasonally?

17. P1263, L18. Here you bring in uncertainty into the discussion, but it should have been discussed together with all your results.

18. P1263, L23-25. This might be true for many catchments, but it is not certain that it is true everywhere. On the contrary, at small scales the difference between downscaling methods can be larger than those from different GCMs.

19. P1264, L3. Horacek 2008 is a conference paper, and just the fact that a similar study has been carried out does not support this claim.

Technical corrections

1. P1248, L9. The first time you mention the Brook90 model, please provide a reference

Figures

Figure 1. There is a lot of important information in this figure, but it is difficult to see where the RCM grid point is in relation to the cathcments. Could you add this information?

Figure 3. I would suggest adding the results from hydrological modelling driven by the “raw” RCM data to show the effect of the error correction.

References

Fowler, H. J., and M. Ekström, Multi-model ensemble estimates of climate change impacts on UK seasonal precipitation extremes, Int. J. Climatol., 29 (3), 385-416, 2009.

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Kendon, E. J., D. P. Rowell, R. G. Jones, and E. Buonomo, Robustness of future changes in local precipitation extremes, J. Climate, 21, 4280-4297, 2008.

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