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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.

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We would like to thank to Anonymous Referee #2 for detailed revision providing many helpful comments and we apologize for the delay in our response.

General comments

RC: 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 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.

AC: It is a reasonable objection. We have used data from two more RCMs (HIRHAM and CHRM) with A2 and B2 emission scenarios. It provided seven possible future projections in total.

RC: 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).

AC: The integration of more grid points does not seem to be more helpful in our case because the other surrounding points have much lower mean elevation than the selected ones. It means that the points are even less representative of our catchments. We are aware of the discrepancy in the grid size and catchment size. Therefore we have used the correction of input data that should be understood as a correction to local scale conditions. The low ratio signal to noise for individual point is probably true in case of extreme events projections such as precipitation extremes (discussed in the above mentioned papers). However these would affect more flood occurrence than the drought spell and the long-term flow pattern we are interested in. The occurrence of flood events is out of our scope.

RC: 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

HESSD

7, C1389–C1396, 2010

model parameters. A sensitivity study with a simple model as the Brook90 would be fairly straightforward, and I strongly recommend this.

AC: We have performed a local sensitivity analysis using parameter estimation software (PEST) to identify sensitive parameters. We have further combined it with the nominal range method calculating the percentage change of outputs due to the change of model inputs (relative to their baseline values). We tried to identify the change in individual months, since the effect of some parameters is seasonally determined. We have compared the effect of the parameters change on runoff in the present to future effect.

RC: 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.

AC: We have shortened these sections significantly and changed the Figure design to show actual data and not percentage change. It enabled us to see some of the previously discussed values in the charts. Besides we have transferred some of the data to tables to the appendix.

RC: 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.

AC: The manuscript has been checked by a native English speaker. Specific comments:

RC 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

AC: We have referred to ENSEMBELES project in the section describing regional climate model data and added the information about high resolution runs (0.22°) to the

HESSD

7, C1389–C1396, 2010

introduction.

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

AC: The figure describing the processes has been added.

RC 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 were then aggregated to monthly values? Could you please be a bit more specific in this section?

AC: It is true that the visual inspection is a rather subjective method; therefore we have decided not to mention it. The Nash-Sutcliffe criterion has been used on monthly data. We have specified it in the corresponding section.

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

AC: The authors of the original paper did not provide a more detailed explanation, nor supported the claims with results. Therefore, we decided not to mention the statement in order to avoid possible misinterpretation.

RC 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

AC: We have rephrased the sentence to: "The regional climate model RCAO was used with large-scale lateral boundary conditions from two GCMs . . . "

RC 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,

HESSD

7, C1389–C1396, 2010

I would suggest using the term “error correction” or “model output statistics” instead of bias correction, unless your method explicitly corrects the bias.

AC: We have described the used approach in more detail and removed the Schoof reference since their approach differed from two other mentioned articles. We changed the bias-correction to error correction or simply the correction.

RC 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.

AC: We have added the missing information at the beginning of the corresponding section.

RC 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.

AC: The parameter values seem to be robust based on the sensitivity analysis showing a narrow confidence interval for the individual parameters. It is likely that there will be different model parameterisations that would yield the similar result, however a detailed search for it within the parameter space is beyond the scope of our paper.

RC 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.

AC: We have skipped the section 3.2 and showed the effect of the correction in a separate figure.

RC 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

HESSD

7, C1389–C1396, 2010

assumption is that the applied correction factors are stationary. This need to be further discussed.

AC: In case of February precipitation and April maximum temperature, the trend is significant only in the period 1967–2006 and not in the period 1967–1990 used for estimation of the error correction. It indicates that the trend is probably a result of changes in years 1990–2006. May maximum temperatures were however significant in both periods. It caused a shift in the average maximum temperature and as a result of this the correction used for RCM data was lower than it would in case of detrended time series. We have discussed it in the manuscript.

RC 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.

AC: It is questionable, nevertheless the claim is not supported by rigorous analysis, and therefore we have omitted the statement from the discussion.

RC 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.

AC: Based on the testing of sensitivities of the selected canopy parameters we have assumed that the model is robust enough to handle the temperature increase. Besides the model checks for the inconsistencies in water balance calculating water balance error. It would have stopped the model in case of exceeding threshold value, indicating the model failure.

RC 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

HESSD

7, C1389–C1396, 2010

uncertainties involved. A mean underestimation of 2% in the annual runoff is most certainly within the observed uncertainty ranges.

AC: We agree with this objection, therefore we have removed this part.

RC 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 changes, so the changes in ET are very uncertain.

AC: It is in general true; we have discussed it in the article in more detail. We do not expect significant vegetation changes as a result of man-made activity. Changes due to forestry management such as large harvesting are unlikely since the catchments are located within protected landscape area, regulating the land-use. Significant change of forest species composition as a result of natural process would probably take more time than 100 years unless a natural disaster such as storm or fire affects the area notably. It is, however, hard to predict and it is beyond the scope of our paper.

RC 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%.

AC: We have avoided such exact numbers.

RC 16. P1262, L26. With "redistribution", do you mean seasonally?

AC: Yes we rephrased the sentence to: "...confirms that runoff will be more sensitive to changes in seasonality of precipitation, ..."

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

AC: We have discussed in more detail within the discussion section.

RC 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.

HESSD

7, C1389–C1396, 2010

AC: Concerning our study the biggest impact on hydrological pattern change has the error correction of the RCM climatic data followed by the selection of the GCM, we have mentioned it in the discussion.

RC 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.

AC: We have rephrased the sentence.

Technical corrections

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

AC: We have provided the reference.

Figures

RC: 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 catchments. Could you add this information?

AC: We have added the information.

RC: 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.

AC: We plotted the simulation with uncorrected RCM data in Fig. 3 as you suggested.

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