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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impacts of climate and forest changes on streamflow and water balance in a mountainous headwater stream in Southern Alberta" *by* V. Mahat and A. Anderson

V. Mahat and A. Anderson

mahat@rams.colostate.edu

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We thank you for your helpful comments on our paper. Please find below our response to the comments.

Comment: General remarks: The manuscript (MS) demonstrates the effects of climate change and forest change on streamïňĆow in a mountainous catchment in Alberta, Canada. The impacts are shown with the HBV-EC model, which is calibrated based on data from a climate station within the study area. Disaggregated data are used to simulate different scenarios and to compare the effects under different conditions.

The manuscript presents an interesting and current issue and is well structured, but I believe it needs some changes and more details before it can be published in HESS. Main concerns are the lack of basic information on how forest change is treated within the approach.

Reply: In the revised manuscript we will add the following details that describe how forest change was modeled in this study.

This project parallels another project investigating the effects of a 2003 wildfire and some salvage harvesting on the hydrology of the headwater catchments following methodology presented by Seibert et al. (2010). Here the objective is to investigate a plausible worst case scenario of changes to the hydrology at larger scales following a large catastrophic forest change (such as wildfire) under present and possible future climates by simulating the removal of forests. Using a relatively simple conceptual model (e.g. HBC-EC) to simulate streamflow with simple precipitation and temperature input data does limit the ability to describe detailed forest processes (e.g. interception, transpiration, changed to radiation, and sensible and latent heat fluxes etc.) using physical processes. However, HBV-EC parameters such as interception factor and MRF (Ratio between melt factor in forest to melt factor in open) (see table in supplement) allow the simulation of different land covers by calibrating the differences in precipitation interception and snowmelt processes between the forest and the open areas. Under our scenario of catastrophic change and no forest regrowth, the parameters controlling interception and snowmelt process are likely the most important process in the mountainous regions where catchment hydrology is dominated by the snowmelt.

We also found that many parameters interacted causing the possibility of unrealistic calibration parameters RFCF (Rainfall correction factor) and SFCF (Snowfall correction factor) values (see table in supplement). For example, calibration of interception in addition to the parameters RFCF and SFCF results in a negative number when rain/snow gauge catch deficiency is larger than the forest snow interception loss. So, we fixed the interception parameters based on some available data and focused our efforts on the calibration of the MRF parameter for the watershed in reference condition. To investigate the importance of the forest in the hydrology of the region and how it interacts with changing climate, the forest was completely removed from the watershed by substituting the parameter set of open areas to approximate the effect of a catastrophic forest wildfire.

Comment: It should also be clariïňĄed in more detail, which parameters of the HBVEC model were calibrated, why these parameters were included, etc. For more details, please see the speciïňĄc comments below.

Reply: Parameters that are included in the calibration are given in the table (see supplement). These parameters are the most sensitive parameters in the model, and are suggested to calibrate by many researchers who have used HBV model in their research.

Comment: SpeciïňĄc remarks: Section 2, P. 8507, L 12ff: You name the different tree species. Here,listing fractions (%) of the land cover (grass, brushes, forest or else) would be of great value. Later on, you could refer to that when you deïňĄne the forest change in your simulations (there is more on that issue below).

Reply: Though there are different kinds of tree species in the watershed, we don't distinguish the tree species in our study. The model characterizes the land cover as forest, open and water body (lake, pond). We consider grass and bushes as open as these are covered by the snow during the winter in our study. We will clarify all these and present the fractions of watershed covered by forest, open and water body in the revised manuscript.

Comment: Section 2, P. 8507, L. 18ff: I believe that one climate station in a catchment of >300 km2 with an elevation range of more than 1000 m leads to a high degree of uncertainty, especially if the area is dominated by snowfall (P. 8507, L. 18). You mention that there are additional climate stations with shorter time series. Did you check how the data relate for existing temporal overlaps? Could you give some hints on this? You

may at least get an idea of how your climate data behave, compared to each other and it may increase the reliability of your input data.

Additionally: Why don't you add the average annual amount of precipitation from your Coleman climate station (in context with the 50-70% in P. 8507, L. 18)? It would be interesting to learn about some longterm average of climate parameters in your catchment. Could you present the average temperature, precipitation, evapotranspiration, etc. from your available data set? A comparison with the average annual streamïňĆow would also be great.

Reply: We do agree with the reviewer that for the elevation range more than 1000 m there is a high degree of uncertainty in the reanalyzed climate data from one climate station. It is hard to reproduce the variability of climate across the watershed with one climate station and applying a constant lapse rate. But the truth is that climate stations in the mountainous regions are rarely available. Regarding few other climate stations in our watershed, few short term (summer) data are available at least from one nearby station located in the top of the mountain. We will present some comparisons of our derived climate with the available observed data from other station in the revised manuscript. As reviewer suggested we would present long term average annual, temperature, precipitation and streamflow in the revised manuscript. We will not be able to present the evapotranspiration as measurements of this are not available.

Comment: Section 2, P. 8508, L. 3ff: Could you give the recording intervals of your monitored climate and streamïňĆow data.

Reply: It is daily data. We will clarify in the revised manuscript.

Comment: Section 3.3.1/3.3.2: Here, more information on the whole modeling approach is needed. The short description of the model is ok, but details on your calibrated parameters would allow to reproduce your work. Did you include all parameters in the calibration? If not, which ones were included and why were they included? Was there any kind of sensitivity analysis prior to calibration? Did you validate the model?

While dealing with scenarios, a validation is very supportive, I think. Additionally, you have a valuable long time series, which you can easily split up into a calibration and validation period. Maybe you can add a table with the most important parameters you calibrated. There, you can also show the parameter values (and obtained NSEs) of your best performance, for example.

Reply: We will present a table (see supplement) with calibrated model parameters and also briefly describe the importance of the parameters in the model. NSE value for the best performance will also be presented.

As we included most of the important parameters in the calibration, we do not think sensitivity analysis is necessary. The direct GLUE approach does help display the sensitivity of the parameters, by showing the ranges that achieve good (best achieved NSE minus the threshold) model results. Although validation would help, our climate change analysis uses only 32 years of data, we were concerned that a shorter length of data would invite more error in the stochastically generated data. Semenov and Barrow (2002) recommend the use of at least 20-30 years of daily weather data while generating stochastic climate in order to be able to capture some of the less frequent climate events.

Comment: Section 3.3.1, P. 8512, L. 22: With regard to the following forest change, it would be helpful to be more speciïňĄc on land use types at this point. Which land use types did you include, and what is the share on the total area?

Reply: We will describe this on detail in the revised manuscript. Also, see our response to previous comment.

Comment: Section 3.3.2: In section 3.2 (P. 8511, L. 18), you state that you [. . .use stochastically generated climates to provide input to the hydrological model to simulate referenceperiod streamïňĆow. . .] for a better comparison of the scenarios. In section 3.3.2 (P.8512, L. 19), you write that [. . .the model is driven by the thirty two years of climate data recorded at the Coleman climate station. . .]. Does this mean you calibrated

the model with observed data from the Coleman climate station and recomputed the time period with generated data to compare the performance of the model?

Reply: Yes, that is true. Observed data at Coleman climate station are used just for the calibration purpose. Data used to drive the model that simulates the reference period streamflows that are compared with future periods streamflows are stochastically generated data. This makes the reference and future period streamïňĆows comparable because both reference and future periods climate data are generated with the same methods, but reïňĆect the statistical properties of the climate periods.

Comment: Section 3.3.4: Please give details on how the 100 parameter sets were obtained. How many runs were necessary to fulïňĄll your criteria? What range were your NSEs in?What is your threshold?

Reply: We will include the details on how 100 parameter sets were obtained and how many runs were necessary to fulfill the criteria in the revised manuscript. Ranges of NSEs and the threshold will also be presented. Also, see our response to previous reviewer.

Comment: Section 3.3.5: As forest change is a major issue of the presented study, the MS would beneïňĄt of a revision of this section. I believe it is ok to give references for methods (P. 8513, L. 18ff), and not describing them in detail in your own MS, but some short information on how the quoted authors deal with the changes would be supportive. To me, it is not clear how you incorporated the forest change in your model. You say you removed the forest (P. 8513, L. 22ff). All of it? For the whole simulation period? How about ‘regrowth’, or do you assume your catchment remains with bare soils during the whole simulation period? If your soil does not remain ‘bare’, with which land use type did you exchange forest?

Reply: See our previous response.

Comment: Is a complete forest removal a realistic scenario for the study site? How about comparing different degrees and different locations of forest removal?For instance, remove only 25%, 50% or any chosen fraction, or, remove forest only from speciïňĄc parts of the area (along the riparian zone, in the upper area, or elsewhere). It would be interesting to see how this affects your streamïňĆow. I would recommend to add at least another variation of forest removal to your scenarios.

Reply: The objective of this study is to investigate a plausible worst case scenario of changes to the hydrology at larger scales following a large catastrophic forest change (such as wildfire) under present and possible future climates by simulating the removal of forests. So the complete forest removal was the scenario developed to assess the worst case scenario following the catastrophe. We don't think any kind of intermediate results would add any new information on catchment hydrology to support our objective to assess the worst case scenario catastrophe and strengthen the manuscript. Also see our previous response.

We would also change the manuscript title from "......forest change" to "....catastrophic forest change" to justify our objective of looking at complete forest "removal" .

Comment: Section 4.4, P. 8516, L. 27: Could you give NSE values?

Reply: Yes, we will provide NSE in the revised manuscript.

Comment: Section 4.6, P. 8517, L. 7: How were the best 100 parameter sets deïňĄned? Which parameters were included in the calibration (please see comment above)? Could you give the NSE ranges?

Reply: We will provide these in details in the revised manuscript. We will include the calibrated model parameters and also include the NSE ranges. Please also see our response to reviewer no. 1 comments.

Comment: Section 5, P. 8518, L. 22: [...Performance of mean precipitation and temperature were good..]. Could you deïňĄne 'good'?

Reply: This line summarizes the section 4.2 where LARS-WG model performance in terms of reproducing precipitation and temperature are discussed. Good in terms of model ability to reproduce the average behavior of precipitation and temperature is written. We will, however, rewrite this sentence in the revised manuscript.

Comment: Section 6: With regard to your title, you should mention the effect of forest change on streamïňĆow in your conclusion, somehow.

Reply: We will include the forest change impacts on streamflow in our conclusion.

Comment: Figures and Tables Table 1: I assume the changes/deviations in Table 1 are given with regard to the Coleman station data?

Reply: We will correct the title. Also, see our response to previous reviewer comments.

Comment: Figure 3: To me, it seems a little confusing that the axis title says 'Mean daily T....,while you are presenting monthly values. In your MS you write 'monthly mean values of daily T.. Maybe you could name this the same.

Reply: We will correct this in the revised manuscript.

References:

Seibert, J., McDonnell, J. J., and Woodsmith, R. D.: EïňĂects of wildïňĄre on catchment runoïňĂ response: a modelling approach to detect changes in snow-dominated forested catchments, Hydrol. Res., 41, 378–390, doi:10.2166/nh.2010.036, 2010

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C5166/2013/hessd-10-C5166-2013-supplement.pdf

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