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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

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General Comments In this manuscript, the authors present a study evaluating the impacts of environmental change on streamflow and water balance components (specifically Et and SWE) in an Alberta Canada headwater catchments using the HBV model constrained by forecasted precipitation and air temperature generated under 3 IPCC (AR 4) emission scenarios. In general I believe this manuscript contributes important progress concerning the impacts of environmental change on water resources.

While the experimental approach, analyses, and results are sound, specific and im-

portant details are absent from the manuscript that should be included to strengthen the manuscript. Specific shortcoming are introduced here and further discussed below: the authors suggest the use of GLUE for uncertainty analyses in the abstract but based on section 3.3.4, uncertainty analysis was little more than a Monte Carlo analysis with an unstated number of simulations; the study explores the individual and combined effect of forest cover change (in perpetuity or recovery?) and climate change on hydrology yet how forest cover was treated in HBV-EC is not discussed. The paper is well written with clear objectives but for the shortcoming addressed above and those detailed below, I believe this paper requires revision to meet the expectations of the readers of HESS.

Specific Comments Pg 8509 - A single GCM model, CGCM3) was used in this analysis. An ensemble GCM modeling approach would contribute to uncertainty in future climate projections. While a single GCM is justifiable, discussion is required to address the limitation of a single GCM with respect to uncertainty in climate projections and hence hydrologic prediction;

Pg 8510 – Authors state that daily observed climate is "perturbed" yet no further definition/description of the perturbation process. It would be impossible to reproduce without this information;

Section 3.3.2 – My understanding is that the HBV-EC model was calibrated using climate data observed at the Coleman station to identify parameter set that is then used for hydrologic modeling (SS 3.3.3) using LARS-WG climate as input for reference period and future climate. While the authors justify the use of LARS-WG for reference and future hydrologic modeling, there is no comparison of HBV model results using observed climate forcing compared to LARS-WG climate forcing. A comparison of simulated streamflow using different climate forcing would be helpful for understanding generated climate uncertainty. This sort of comparison was conducted for modeled vs observed climate in Table 2. Why not extend it to hydrologic modeling? Perhaps simply a 1:1 line (sim Q_obs climate vs. sim Q_LARS-WG climate) for comparison?

Section 3.3.4 How many simulations were conducted to generate the 100 best parameter sets?

Section 3.3.5 – More detail to understand how you implemented change detection methods of Seibert and McDonnell 2010. Specifically Seibert and McDonnell 2010 used three methods for change detection: model residuals, comparison of parameter distributions, and comparisons of simulated hydrology different periods. It is unclear of your change detection approach

Pg 8516 – Again, how many simulations and what is the range on NSE. No ability to determine that "...NSE was not that great."

- Section 4.4 presents the results of HBV-EC application using LARS-WG input data. Unlike section 4.3 that quantifies model error between observed and simulated Q, there is no such formal quantification of error in this section. Despite that this model application using the calibrated parameter set from observed data, it is important to quantify error for this model application as well. How well or poorly did the HBV-EC model perform (besides objective function evaluation)?

Section 4.6 – a major shortcoming of this study is how forest change was considered in the HBV-EC modeling framework. The forest parameters in HBV-EC determine the proportion (0-1) of precipitation, snow, and sunlight reaching the ground. It is unclear to me how the modelers treated forest change. Were the parameters for proportion of precipitation and sunlight 'reaching' the forest floor adjusted to remove 'interception'? Were these parameters fixed overtime for future scenarios implying no recovery of forest, i.e. a permanent removal of forests through the year 2100? How would Figure 9 differ based on forest change definition?

Given that the impact of forest change on streamflow is a primary objective of the study, considerably more detail (methodology, assumptions, limitations, etc.) and therefore revision is required to understand how the parameterization of forest change is considered in the modeling endeavor.

Pg 8520 – "Usually the removal of forest results in increased summer flow..."references to substantiate this?

Section 6 Conclusions – The paper would be strengthened by understanding the implications of the results.

Table 1: "Relative" changes... to what, to calibration/reference period? Also what are the annual and annual mean values at the end of the table? Please clarify/define

Figure 6: HBV-EC simulations based on LARS-WG? Please clarify in caption

Technical Comments Pg 8513 – McDonnell misspelled in text. Require two "L's"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8503, 2013.