

Krogh and Pomeroy Response to RC2

We appreciate the thoughtful comments and insights provided by Reviewer#2, and below in **bold** include a detailed response to each comment.

I think this is a generally well-written and important paper that attempts to separate the drivers of climate change and vegetation change on hydrology over a historic period. My biggest concern is the observational data used to drive the model, particularly precipitation and snow. I understand that the experiment compares three scenarios (changing climate, changing vegetation, and both) so I would like to see how each of these is impacted by uncertainty in the quality of the precipitation estimates. If the cold season precipitation is most biased, it could be that changes in the length of the cold season cause changes in this bias with time. I think the springtime precipitation trend is suspicious and I'd like to know how uncertainty in those data impacts the robustness of the results. The description of the precip data is useful, but I'd have more confidence if a more thorough comparison of different precip data sources (and nearest other stations) were performed. There are a lot of discontinuities in these datasets, as described. Have the authors considered doing a scenario that separates temperature and precipitation change? In the discussion, I'd like to hear more about impacts of the uncertainty in the precipitation data on the larger results of the study. Same for snow measurements and to a lesser degree, streamflow.

ANS: The reviewer identifies important aspects of the uncertainty associated with some of the meteorological records used in this study. The measurement of snowfall in this environment is of particular interest to the authors (Pomeroy and Goodison, 1997). Compared to much of northern Canada, uncertainty in precipitation measurement is confined at Inuvik due to the low winter wind speeds and the meticulous data quality control and corrections used in the AHCCD dataset (Mekis and Vincent, 2011). Observations that are not subject to corrections by Mekis and Vincent involve automated weather stations to which well-established wind undercatch corrections were applied to achieve similar corrections. Nevertheless, we agree that the dataset is not perfect and there are discontinuities in the mid-90's when the system changes from manually to automatically observations. Fortunately, snow surveys in sheltered taiga provides a means to evaluate snowfall records, as snow redistribution and sublimation in small clearings in the taiga are minimal and so provide an alternative method to estimating seasonal snowfall in the region (Pomeroy et al., 1997). In the revised version of the manuscript we have provided a more comprehensive discussion about the potential impacts of the uncertainty in precipitation records on the results as per reviewer's suggestion, which are supported by a comparison of previously published trends in nearby stations. We did not consider a scenario separating temperature and precipitation change, though we note that it would be an interesting exercise, it is out of the scope of the study.

Minor comments: like the other reviewer, I don't care for the use of the term 'hydrologically resilient' without a technical definition provided. This is too vague. I would also like a little more information on this basin. Is this a well-instrumented research basin? It doesn't really seem like it, based on the description of the single station observations. Why was it chosen? Are there no

research basins that fit the description (tundra-taiga boundary with permafrost)? In your introduction, it might be worth mentioning the NASA ABoVE (Arctic-Boreal) campaign, focused on exactly these eco-zones because it has a hydrology component. Finally, while most of the paper is readable, the abstract could use some work. Go for shorter, simpler sentences that really convey what is interesting and exciting about this paper.

ANS: We agree with the reviewer and have revisited the term “hydrologically resilient”. Havikpak Creek has had hydrological studies since 1992 and we now refer more to the detailed description and history of the basin in Krogh et al (2017), and we would like to refer to that work instead of repeating those details, with the idea of keeping the manuscript as concise as possible. We have added a paragraph discussing why Havikpak Creek was chosen as the study basin and also include a reference to the NASA ABoVE project as suggested by the reviewer. We have re-written the abstract to make it more readable as per the reviewer’s suggestion.