The Cryosphere

EGU

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Discussions

Interactive comment on "Changing Characteristics of Runoff and Freshwater Export From Watersheds Draining Northern Alaska" by Michael A. Rawlins et al.

Anonymous Referee #3

Received and published: 3 April 2019

The paper aims at the analysis of baseline conditions and changes of hydrological elements at 42 catchments over the period 1981-2010. For this purpose, an updated version of the Pan-Arctic Water Balance Model (PWBM) was applied. The presented results indicate statistically significant increases in cold season discharge. A significant increase in the ratio of subsurface runoff to total runoff was found for 24 of 42 studied catchments. These changes correspond well to the increase of the active layer thickness due to higher air temperature and general climate warming.

The topic is potentially interesting for the hydrological society, especially the analyses of the non-stationarity of hydrologic processes in cold climate due to climate change.

However, I have many concerns and comments on the methodology that should be addressed. The most important is the lack of model validation. The presented results are mostly simulation-based and cannot be analysed without appropriate model validation. The results of validation for the Kuparuk catchment are not promising. I would not use this model for the assessment of changes in the timing of maximum flows. The error of maximum flows was estimated to 9 days while shifting in peak spring occur around 4.5 days earlier. The error is higher than the simulated changes.

In my opinion, the model description is not sufficient. There is no information regarding solved equations, water balance, thermal balance. Is the energy balance included in the model? Is soil temperature modelled separately or is it included in the PWBM? There is no information regarding the model parameters (number of parameters, their meaning, how the model parameters were determined? By optimisation? Or just assumed? There is also lack of information on applied optimisation method). Some of the parameters were selected in a strange way without any explanation (for example changes in f parameter that is described in lines 207-209, the assumption of the effective velocity v=0.35m/s). Are the values of this parameter constant for the entire domain? Why is evaporation reduced to 1/3 of the potential ET rate?

There is a lack of map and description of the study area. It is stated that 42 catchments are analysed, but only results for one or three catchments are presented. Are the results the same? Are there any differences in the results between catchments? How were these results summarised?

SWE simulations were evaluated using average values from observations collected at a 200x300 km domain. The PWBM was run at 25 km resolution. These are completely different scales. Large differences in SWE especially for 2004 and 2007.

Why was a linear trend analysed? I suggest using a modified Mann-Kendall trend test for autocorrelated data for this purpose. The test should be applied separately for each catchment and then the results should be analysed I don't have any great advice for

tidying up the manuscript, but basically I think it somehow needs to be streamlined, made easier to read and corrected. Some of the conclusions should be reconsidered, better highlighted and more concisely presented. The authors should be more clear about the meaning of statistical significance of their results and more careful when drawing conclusions from non-significant results. There are major errors or gaps in the paper but it could still become significant with major changes, revisions, and/or additional data.

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