

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

### **Anonymous Referee #1**

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This paper presents interesting results from a hydrological modeling study examining how runoff partitioning from arctic catchments is changing. The authors present an updated version of the Pan-Arctic Water Balance Model to better represent soil freeze-thaw processes and have renamed it the Permafrost Water Balance Model version 3 (PWBM v3). In general, the authors use the model to demonstrate that cold season discharge and groundwater flows are increasing in four arctic basins underlain by continuous permafrost. The authors do a very nice job of characterising how runoff and terrestrial water storage is changing in arctic catchments. This study is limited to basins underlain by continuous permafrost and differs from other work in that they do not attempt to generalise findings from large northern regions spanning different per-

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mafrost distributions (which is a good thing). The results and discussion are limited explicitly to model outputs, which are supported by only basic model validation from observed measurements. Without a better understanding of how the model performs, it is difficult to determine how valid the model outputs are, as well as potential errors associated with the outputs. Additionally, the novelty of this study is questionable as the main conclusion of this paper (as is stated many times in the discussion) is that arctic catchments are exporting increased runoff via subsurface pathways, which has previously been demonstrated in the literature. I think that this modeling study could be an important contribution; however there are several significant revisions and additions that are required.

Major Points: • A major weakness of this manuscript is the lack of model validation and performance evaluation. At this point it is impossible to understand how well the model performs, and consequently impossible to comment on whether the outputs are a realistic interpretation of the physical system. By only discussing the outputs of the model there is potential for a large disconnect between what is being presented and the system for which the authors are trying to represent.

- Why is only one basin (Kuparuk) used for validation? There are other suitable gauged basins by the United States Geological Survey and the Water Survey of Canada that could be used as validation. This component is crucial to the success of the paper. The only validation presented in the results section states that freshet volume was similar, yet even on a monthly time step the model performance is weak (~30% error in both May and June). If the authors want to describe how the partitioning of runoff is changing by exclusively examining model outputs then it is imperative to prove that the model can simulate observations. To do this, it is necessary to use a finer resolution than monthly time-steps
- Why is modeled cold season discharge not evaluated against observations? Surely USGS publishes this data.
- Why is there no model performance evaluation? There are many different evaluation techniques (e.g. Nash-Sutcliffe Efficiency, Root Mean Square Error, Percent Bias, Kling-Gupta Efficiency), but none are presented in the paper, nor is the reader referenced to other papers where they may be presented.
- Is there any model cali-

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bration? Are there any empirical factors used? More information is needed – This manuscript describes intensification of the hydrological cycle and is supported through re-analysis data and modeling efforts. The manuscript would benefit from supporting data from observations. It would be useful to plot precipitation from climate stations across Alaska and northern Canada to prove this, as well as using or referencing snow survey data. Modeling these changes is important, however these modeled changes need to be supported by observations. – The authors use much of the discussion to suggest that the proportion of groundwater runoff is increasing, yet there is very little discussion of how the structure of these flowpaths is changing. As the study site is located exclusively in continuous permafrost I would assume that these changes would be through supra-permafrost groundwater flow, but this is not explicitly stated. Is the ice-rich transient layer (Shur et al., 2005, Permafrost and Periglacial Processes, 10.1002/ppp.518) accurately represented in the model? This ice-rich layer retards active layer thickening due to the high latent heat requirements for thaw, and would also provide an additional water source once thawed. Does changing seasonality of precipitation affect runoff generation? – Some sentences are taken directly from other papers. These sentences should be changed in an attempt to synthesize other literature. For example, lines 436-39: “St. Jacques and Sauchyn concluded that increases in winter baseflow and mean annual streamflow in the NWT were caused predominantly by climate warming via permafrost thawing that enhances infiltration and deeper flowpaths and hydrological cycle intensification (Frey and McClelland, 2009; Bring et al., 2016)”. This text appears almost exactly word-for-word in the abstract of that paper. I also find it odd that a sentence from another paper has two additional references after it. Actually, St. Jacques and Sauchyn (2009) propose re-activation of deep groundwater flowpaths by making linkages between streamflow and climate. Also, many of the basins in this study are underlain by discontinuous permafrost, which would promote recharge of sub-permafrost groundwater aquifers that provide baseflow to rivers, a process not applicable in thick, continuous permafrost. Again, the changing physical processes need to be explored. – If the authors are going to validate and calibrate

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a model, why only use it for a period in the past? Analysis of past data can be conducted reasonably well with measured data. The authors may be better served to also use the model as a predictive tool to demonstrate how a changing climate may affect the streamflow regime of arctic rivers. The figures need substantial revision and improvement. They are not suitable for publication in their current form.

- o The authors should provide a study site map delineating all four watersheds, as well as a layer identifying each underlying permafrost zone. Line 108 states that the study area is underlain by continuous permafrost. Is this the case for the entire study site?
- o Figure 1 is a very important figure and does not suffice as model validation. For example, the figure should be presented on a daily time-step (not aggregated into monthly intervals) to demonstrate how the model captures individual events. For example, there are substantial differences between May and June runoff, suggesting that the hydrological behaviour of the basin may not be captured. Also, all time series plots should include each data point instead of a continuous line-graph. The dashed-line in the simulation makes it difficult to observe performance. The formatting of all figures should be improved in this manner
- o Figure 4 should present discharge normalised over basin area. As a result, the North Slope shows disproportionately more discharge due to the much larger basin area. I am not sure why the authors decided to present the data this way, considering that Figure 1 presents normalised runoff. Also, the current formatting makes it next to impossible to discern runoff trends for the three smaller basins.
- o Figure 5 is slightly misleading as the plot only shows the grid cells with significant changes. Figure 6 shows that many grid cells do not have significant change – but Figure 5 suggests that there is an increasing proportion of subsurface runoff in June and decreasing in July, when in fact these proportions may be relatively constant if the whole dataset it included.

#### Minor Points:

Line 21: Can you better define region based on watersheds? Line 21: Do not need the word ‘annually’, this is given in your units Line 22: Is this volume derived from

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modeled results or gauges? If the former, this needs to be stated, if the latter, these gauges should be used for validation Line 24: The authors need a better preface for their results. At this point it is unknown what the results are describing. Line 34: I am not convinced that this shift is representative of the physical system, given section 3.3 states errors in freshet timing. Again, displaying data on a daily timestep for all basins would be beneficial Line 47: Provide references for “mean freshwater budgets across the land” Line 52: This sentence is redundant given the previous sentence Lines 53-55: What about shorter ice duration on lakes and longer seasons for evaporation? Lines 69-71: This areal loss of permafrost applies to sporadic and discontinuous permafrost. The study site described by the authors indicates very thick continuous permafrost. This discussion should be limited to continuous permafrost environments so that the physical processes occurring in different environments are not confused Lines 75-77: Similar comment to above, most of the rivers described in the cited studies are either subarctic or underlain by discontinuous permafrost. Runoff generation is very different between the two environments and this needs to be stated if there is extensive discussion about these systems. Line 95: Why do you need to leverage a modeling framework to investigate changes in peak daily discharge? Would observational daily data not be a better method for this? Lines 108-110: The study area is underlain by thick, continuous permafrost. This context needs to be explored in more depth in the discussion. The authors should describe how the flowpaths in this environment would differ from other studies in the literature. This has the potential to be a novel contribution and differentiate this work from other studies that it cites. Line 112: Provide a table of all observational data, agency responsible for collecting the data, locations of data collection, and period of data record Lines 157-159: I am not sure I understand this sentence. How do you compare modeled SWE against observed river discharge? These are very different parameters. Storage exerts a large control over how much snowmelt water is delivered to the stream network. Lines 161-164: The authors either need to provide more information on how the model was parameterised and how it performs, or provide references to previous publications that have previously done this.

Lines 218-19: Can you provide more justification for why effective velocity was set to  $v = 0.175$ ? This appears to be an important parameterisation of the model but there is very little justification given. Line 233: Are there any CALM sites or other field based observations from which the authors could compare their modeling results? Line 255: Why is only one basin used for validation? Line 263: Typo, “this occurs despite” Lines 267-268: Again, please display on daily timesteps and provide model performance evaluation Line 296: Please provide observational data to validate the modeled data Line 309: Is surface runoff defined as overland flow? How are surface organics handled in the model? Many sites in the tundra have surface organics or peat layers where the porosity of near-surface soil is very close to 1, effectively eliminating overland flow due to the lack of resistance to flow exerted by the soil. In these situations would all runoff be subsurface? A better description of soil layers and modeling structure is needed to allow the reader to conceptualise the processes that are being explained. Lines 361-363: Provide references Lines 362-363: “materials exports to coastal zones” typo Lines 371-372: Why not test this and include in the current model? Lines 395-396: Which processes? The authors should be explicit about how hydrological processes are changing and cite field-based research to do so. For example, there have been quite a few relevant papers published from studies in northern Canada that are not referenced.

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