General comments:

This paper documents the findings from field observations of subsurface routing in high and low centered polygons in continuous permafrost. The authors used a conservative tracer and hydraulic head measurements from a series of wells to estimate subsurface runoff. The authors claim that most hydrological models do not have processes to represent lateral routing and that this paper demonstrates that this process be included in land surface schemes. For the most part (with the exceptions noted below), the science seems sound, however a mass balance of the bromide tracer was unachievable due to possible cryoturbation or other redistribution processes during freeze-up. I feel that the findings of this paper could merit publication; however there are some very major revisions that are required, including substantial rewriting. As it is written, the paper does adequately place this study in the context of previous research and the results are not clearly defined. The abstract and conclusion need to be re-worked to identify the scientific observations that will benefit the hydrology community.

It is my understanding that the authors are claiming that lateral transport across the frost table after infiltration is the most important finding of their study. The idea that frost table topography controls subsurface runoff has been well documented in the literature (Morison *et al*., 2016, Helbig *et al*., 2013; Quinton *et al*., 2000; Wright *et al*., 2009) and should be acknowledged as such, instead of as a novel finding. It was surprising that the authors briefly cited some very relevant studies for general water balance estimates (*i.e.* Helbig *et al*., 2013 for evapotranspiration; Liljedahl *et al*., 2016 for biogeochemical comparisons; Quinton *et al*., 2000 for hydraulic conductivity), but did not mention these studies in their discussion of subsurface routing in Arctic environments (and specifically ice-wedge polygons). By citing these papers the authors demonstrate that they are aware of these studies, but for some reason do not frame their research in the context of work that has already been completed. In the abstract, the sentence, "Estimates of horizontal hydraulic conductivity were within the range of previous estimates of vertical conductivity, highlighting the importance of horizontal flow in these systems" appears to be the most conclusive sentence in the abstract but does not convince the reader of a novel finding. The main finding in the conclusion is that, "horizontal flow is important". After reading this paper, I have not been convinced that horizontal flow is 'important', nor do I have an idea of how important it is on the total flux of subsurface runoff. I am also not convinced that this study, as-is, will provide a basis to improve hydrological models. In making these claims, the authors should: a) quantify horizontal hydraulic conductivity rates (this could be done directly in the field); and b) identify lateral flow routing mechanisms and attempt to quantify a landscape flux to demonstrate the relevance to this study. To do this, the results and discussion sections should be re-written to better position the paper's objectives and the authors should consider upscaling their findings to the subcatchment scale. The discussion section should be better framed with more reference to existing literature. As currently written, most of the discussion lacks references, with the exception of occasional sentences having many references (*i.e.* page 20, line 8). The discussion section is a major weakness of the paper and could be written much better. Specific comments are listed below.

Specific comments:

Page 1 lines 29-32: List references after each point instead of at end of the sentence. For example, "… as it affects hydrology (hydrology refs), biogeochemical transformations (biogeochemical refs)" etc.

Page 1 line 32: How is 'northern' Arctic permafrost zone defined? All Arctic landscapes are northern, are you referring to the northernmost Arctic landscapes?

Page 2, lines 19-20: I struggle to understand the notation of the 'relative' roles of vertical and horizontal fluxes and that no other studies have been conducted toward quantifying this. It is generally accepted that in permafrost environments precipitation inputs: 1) infiltrate organic soils; 2) percolate to the frost table; and 3) produce lateral runoff where it is routed in accordance with frost table topography and is governed by fill-and-spill. There has been considerable work evaluating this principle, and other bodies of work that have evaluated subsurface runoff through ice wedge polygon terrain at the landscape scale (Helbig *et al*., 2013; Liljedahl *et al*., 2016). I am also not convinced that if regional and pan-Arctic land models ignore horizontal fluxes that they would be well positioned to incorporate results of a study that document flow at the individual polygon scale. Furthermore, there are many hydrological models that include modules for subsurface routing, and even have options for different ways to parameterise that routing (*i.e.* Raven hydrological framework, Cold Regions Hydrological Model, Canadian Land Surface Scheme). I think the authors should also stress that this study seeks to better understand the differences in subsurface hydrology between low and high centered polygons, as this is a key component of the research.

Page 3, line 10: Again, routing mechanisms for lateral flow in polygonal terrain have been discussed. Helbig *et al*. (2013) conclude, "The prominent microtopography of the polygonal tundra strongly controls lateral flow and storage behaviour".

Page 4, line 5: How representative are the properties of the polygons that were selected? Can you provide mean surface area and elevation (DEM?) for the study site?

Page 5, line 2: Are the pressure transducers absolute or vented? If the former, where is barometric pressure being collected?

Page 5, line 2: How were the elevations of the well casings surveyed?

Page 5, line 29: How frequently was the sampler at the frost table moved down?

Page 8, figure 5: I would include this in the results section

Page 9, line 34: Why were values (and a subsequent range) for porosity used from the literature and not measured at the site?

Page 10, line 1: There are numerous instances where this long list of references is used. It would be much more beneficial (and more informative) to include all of these studies in a table

with their associated values for each parameter and then reference that table throughout the paper.

Page 10, line 2: What was the time period over which the average head difference was calculated?

Page 10, line 10: How was the flux through organic soil calculated?

Page 10, line 19: How does 'infiltration dominance' explain a rising water table? These sentences are worded awkwardly. This information would be much more clearly explained by showing a combined plot of cumulative evapotranspiration and cumulative precipitation.

Pages 10, 11, figures 6, 7: These are very nice figures and display a lot of data in a format that is easy to read and digest.

Page 11, line 12: I do not agree that the 2015 data shows that, "hydraulic gradients were often from the centre outward". I would argue that the hydraulic gradient was variable across the polygon. Also, this was not mentioned in the methods, but how were the wells surveyed and what was the error associated with these surveys? This may impact the hydraulic gradient measurements given that the elevation of all six water tables are within 40 cm.

Page 11, line 17: Given their close proximity, why would the purple and yellow trough wells on the HCP have water table differences of nearly one metre? Are there significant differences in soil type, topography, etc?

Page 15, line 20: Again, why not measure porosity of the mineral soil directly?

Page 15, line 21: A range of 4.8 – 93.7% for possible tracer mass to leave the polygon is very high.

Page 15, line 25: Can you conclusively say that 'most' of the tracer remains in the centre of the LCP if your maximum estimate is that 93.7% left? Is there any way to improve this estimate? As-is, you cannot make this claim.

Page 16, lines 7-8: Again, provide these references as a table with associated values

Page 16, line 23: Can you elaborate on the secondary porosity network and describe this more in Figure 12?

Page 16, line 33: It may be worthwhile to include a discussion of heterogeneity and dual porosity in peat as well (I inferred that this section is restricted to the mineral soils).

Page 17, line 10: Do the frost table elevations measured with a frost probe coincide with the GPR results?

Page 17, lines 8-18: This is a good example of a paragraph that should be linked to existing literature that has evaluated the controls that the frost table exerts on subsurface runoff. A major weakness of this paper is that the discussion section does not integrate this study with other work to advance scientific understanding.

Page 17, line 23: "… as the frost table progressively deepens each year and these ice lenses thaw …" – This sentence implies that the active layer is becoming thicker every year. Is this the case? I have not seen a site where the active layer is thicker every year. Also, this section should contain mention of the ice-rich 'transient layer' described by Shur *et al* (2005). This discussion would be strengthened by including different values for hydraulic conductivity as the thawing front transitions from organic to mineral soil, and the controls that soil type has on subsurface runoff.

Page 18, line 5: What you are describing here is the transient layer (Shur *et al*, 2005). Again, a more detailed literature review is necessary to better frame the findings from this study.

Page 18, line 29: Provide a reference for the statement that snowmelt only lasts between two and three weeks.

Page 18, section 4.2: This appears to be a long-winded explanation of why part of the experiment failed, including explanations of various permafrost processes that have been explained before. This section could be greatly reduced and moved to the results section. Was there any monitoring of tracer concentration during freeze-up? This is a period of hydrologic activity that is often overlooked.

Page 18, line 35: The initial hypothesis that the interface of organic and mineral layers does not control horizontal flux may still be true. The authors should evaluate the relative roles of the horizontal flux while the frost table is in within the organic layer and when it descends to the mineral soil layer. The effect of subsurface runoff and the interplay between soil layers and frost table dynamics is a process that has been well documented, and should be referenced as such.

Page 19, figure 13: In the high centered polygon, why is the vertical flux minimal/negligible? What happens to precipitation inputs if they do not infiltrate the soil column? If this is a conceptual diagram, should the water table in the centre of the polygon (LCP) not be higher than the trough if flow is directed outwards? Why is the major transport pathway to the right and not the left? There does not appear to be a difference in hydraulic gradient. Is this process limited by soil heterogeneity and differences in hydraulic conductivity? The rationale behind this diagram is not clearly evident.

Page 19, lines 18-19: Figure 6 does not indicate that water from polygon centres is distributed to troughs in LCPs. Actually, the data from 2016 indicates the opposite (as is stated in the results section). The discussion section should be written to better represent the data.

Page 19, line 25: Would estimates of hydraulic conductivity not have been more reliable by completing pump/slug tests in the field?

Page 20, lines 5 and 6: Can the impacts of freeze-up and thaw be elaborated? What effect does the two-sided freezing front have on subsurface hydrology in the thawed, saturated zone?

Page 20, line 20: I would not agree that field investigations are "almost totally lacking".

Page 20, line 22: A major weakness of this study is that the lateral flux is not quantified. Indicating that lateral flow is 'important' is not a conclusion. A total flux (mm) from each polygon is needed if this work is to improve hydrological models.

Page 20, line 24: Is the Arctic Terrestrial Simulator the only hydrological model that these insights can help to improve? What is the rationale for including this model?

Page 20, line 27: The final sentence is not a good concluding sentence for this paper.

Technical corrections:

Page 1 lines 35-36: The last two sentences are not sentences. Please rewrite.

Page 2 line 3: "centers, rims, and **troughs**" Misspelled.

Page 10, line 16: "From the beginning of July **until mid-August**…"

Page 13, line 20: "Frost **table** depth"

Page 16, line 1: "… tracer dynamic**s** …"

Page 16, line 21: First sentence is not a sentence
Page 16, line 24: "… range in horizontal **hydraulic** conductivity …"

Page 16, lines 34 -35: Awkward sentence

Page 16, line 35: "process", not processes

Page 16, line 36: "… a potential cause **of** heterogeneity …"

Additional References:

Morison, M.Q., M.L. Macrae, R.M. Petrone, and L. Fishback, L (2016), Seasonal dynamics in shallow freshwater pond-peatland hydrochemical interactions in a subarctic permafrost environment, *Hydrological Processes*, 31: 462-475, doi: 10.1002/hyp.11043.

Shur, Y., K.M. Hinkel, and F.E. Nelson (2005), The Transient Layer: Implications for Geocryology and Climate-Change Science, *Permafrost and Periglac. Process.*, 16, 5-17, doi:10.1002/ppp.518.

Wright, N., M. Hayashi, and W.L. Quinton (2009), Spatial and temporal variations in active layer thawing and their implication on runoff generation in peat-covered permafrost terrain, *Water Resources. Research.*, 45, W05414, doi:10.1029/2008WR006880.