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Interactive comment on “Estimation of permafrost thawing rates in a sub-arctic catchment using recession flow analysis” *by* S. W. Lyon et al.

Anonymous Referee #1

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The paper by Lyon et al. drawn on classical recession analysis approaches to estimate permafrost thawing rates in a discontinuous permafrost subarctic catchment in northern Sweden. The approach taken by the authors is interesting in that it utilizes valuable long-term data to assess whether changes in streamflow recessions (notably dQ/dt) for the late-summer period are manifest, and whether this can be related back via recession analysis to changing permafrost depths.

While the paper is certainly well written and interesting, it is lacking in a number of important areas that make some of the conclusions suspect, and there is indeed a danger that this method will be applied elsewhere to infer similar results. While field-based studies have confirmed a degradation of permafrost, I believe that the method outlined here is highly equivocal, and some of the associated data point to a much more

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complex picture. If this method was applied to a continuous permafrost catchment, the picture may in part be clearer, yet the nature of discontinuous permafrost and its complex geo-hydrology do not facilitate this type of analysis at larger and long time scales.

With regards to referencing, there is a lack of proper representation of the hydrological cycle and runoff pathways in this environment. While the paper sets out the importance of the research in the context of the carbon cycle, there is much work from subarctic and arctic North America describing the role of permafrost and the pathways that water takes beneath the surface that is neglected. In addition, there has been significant research on recession parameters for permafrost-underlain catchments that is absent. Notable conclusions from other work that affect the interpretation of the data here is that there is a strong seasonal signal in recession coefficients as the seasonally thawed active layer thickens and deeper (and slower) runoff pathways are activated. As recession parameters are highly variable on a seasonal basis, it is difficult to used binned data to detect long-term trends, even if they are integrated over several month periods.

The authors should be consistent with the permafrost literature in their terminology. The depth of the active layer (the seasonally thawed zone) is what is changing seasonally and inter-annually. The statement that there is no mechanism involved to decrease the original pre-tawing groundwater flow or depth ignores large inter-annual variations in active layer thaw.

The hypothesis is that thawing permafrost extends the depth of the active groundwater system layers in which flow can occur in the summer season. To paraphrase, the authors believe that supra-permafrost aquifers will thicken in response to thaw at the top of the permafrost. A thickening active layer will indeed allow for increased storage capacity of the supra-permafrost aquifer. However, this will allow the water table to fall farther from the surface atop the descending cryo-front. The statement (p. 71, ln 25 and on) that there is little variance in hydraulic conductivity with depth below the surface is unfounded, and in fact against all published literature for subarctic and organic-

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mantled permafrost soil which show extremely strong depth-dependent decreases in Ksat with depth. Presumably, deeper thaw will relate to slower transmisson of water (and shallower recessions) with time. Examination of Figure 2b suggest the opposite is occurring.

The conclusion that long term changes in intercept a along with the trends in winter flow and spring temperature suggest an increasing effective aquifer depth. In all previously reported literature, decreasing permafrost disposition and thaw leads to increased winter flows; an observation that has been reported in both North America and Russia. This is supported by the physical argument of deeper flow paths, more deep percolation and recharge, etc. The declines in winter flows observed in the Abiskojokken are at odds with this. The authors state that these declines are because deeper reservoirs receive less input, yet this is not supported by increased precipitation throughout the study period. The argument that increasing spring temperatures will lead to increasing evapotranspiration and a decrease in recharge is weak as much of the recharge will come from snowmelt when ET is low anyway. It is unlikely that small changes in ET will dramatically reduce winter flows. I agree that streamflow is an integrated signal of many different subsystem fluxes, but there are many secondary affects not discussed such as changes in vegetation throughout the period, the role of changing precipitation, etc., While I believe that there has been significant thaw (as validated by direct field observations), I do not believe that this recession analysis is appropriate to discern a thaw rate. The manuscript does not indicate what percentage of the catchment is underlain by permafrost. If it is only a small fraction (say $<$30%), what are we really seeing here and it reasonable to attribute these changes to declining permafrost, particularly as winter flows are going down as well? I believe that for this methodology to be validated in any manner, it MUST be utilized in other permafrost catchment where similar data exists. For just this one site, the conclusions are simply not justified. However, I do encourage the authors to pursue this work further.

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