Reviews and Responses:

Thank you to both reviewers and the editor for the feedback, insights, and suggestions to our manuscript. We believe that the quality of writing and science has improved significantly thanks to these contributions. In response to the reviews received, we are prepared to adjust the manuscript as we have outlined in this response to improve clarity to our research. Additionally, we performed additional analyses in order to answer questions that have been outlined by the reviewers. We have additional presentations of data as tables or figures to answer these, or we have provided justification to what has already been presented in the manuscript. Please see below our responses to the reviewer's comments. Each reviewer has a section on this document (reviewer 1 followed by reviewer 2). The original comments are *italicized*, and our responses are non-italicized and indented.

Reviewer 1

General Comments:

There is a reasonable study behind this. Needs a good re-read by an editor so that typos and poor English are sorted, eg. the opening sentence of the Abstract contains a typo and line 34, the opening sentence of the Introductions, does not make sense.

> Thank you for your comment. We will make sure to read through the manuscript and check for clerical errors, and include the ones noted by the reviewer.

Supplement Comments:

> Thank you for the detailed revisions in the attached supplemental document. Grammatical and clarity issues will be edited using "track changes". Specific responses to longer comments not included in the interactive discussion can be found below:

The models in the table seem to intercept only models. Why not allow for slope variation between sites?

> We specified Site ID as a random intercept to account for repeated measurements and baseline differences among catchments, recognizing that individual creeks may differ due to characteristics not explicitly captured by the measured predictors. Site ID as a random intercept therefore allows baseline concentrations to vary among catchments. Additionally, differences in responses to hydrological conditions among catchments were represented through interactions between flow percentile and measured catchment characteristics. For example, the interaction between flow percentile and MAT allows the modeled effect of flow to vary among catchments according to their MAT. Thus, although catchment-specific random slopes were not included, the models allow for systematic variation in slopes among catchments where this variation can be explained by measured catchment characteristics. This approach is consistent with our objective of estimating how hydrological responses vary as a function of environmental and catchment characteristics, rather than estimating an independent flow-response relationship for each individual catchment. The influence of additional catchment characteristics is explored further in our response to Reviewer #2 below.

subjective judgements how was comparability tested and what does similar mean?

Our intention was not to statistically compare the relationships between high and low flow conditions or to assess whether the regression equations differed. Rather, the figure was included as a descriptive illustration of the co-mobilization of these parameters by presenting the relationships separately (i.e., stratified by flow condition). Its purpose was to demonstrate that the expected relationships among these parameters are evident under both flow regimes, rather than to formally test for differences between flow conditions. Accordingly, we have revised the text to clarify this intent and avoid implying a statistical comparison

So your test relies on the significance of the interaction terms? You need make this clear.

Candidate mixed-effects models were compared using AIC and model performance metrics. The best-supported model was then used to generate predicted concentrations across a range of MAT values to visualize the effects of the fitted interaction terms. We have clarified that in text.

This is just a visulisation of the interaction term that could be estimated without LME

We agree that the figure represents a visualization of the interaction effect; however, the predictions shown were generated from the fitted linear mixed-effects model rather than from independent regressions. The LMM was used to estimate the interaction between FP and MAT while accounting for repeated measurements among catchments through the random intercept for Site ID and incorporating the other fixed effects included in the selected model. The figure was intended to facilitate interpretation of the modeled interaction by illustrating predicted TMeHg responses across a range of MAT values, rather than to serve as an independent statistical assessment. We believe that the LMM was essential to be able to then visualize the responses of each parameter at different MATs.

Is it limited evidence or is that you found no significant role for permafrost control on inorganic nutrients.

By "limited evidence," we intended to indicate that the analyses provided little support for a consistent influence of peatland permafrost conditions on inorganic nutrient concentrations, rather than suggesting that the assessment was constrained by limited data. We have revised the text to clarify this distinction: "There was no significant role of peatland permafrost conditions on the concentrations of inorganic nutrients (NH_4^+, $NO_2^- + NO_3^-$, SRP) in the study region. Only SRP moderately flushed in catchments where permafrost was absent."

Specific Comments:

Line 21 – here and throughout the text the word strong or strongly is used. This word is a subject judgement and has no definition and so should be deleted on each occurrence.

We have removed the word strong or strongly from the manuscript.

Line 63 – that is ammonium not ammonia.

Thank you. Corrected.

Line 72 – I think it should be important not importance

Thank you. Corrected.

Line 78 – "… a major northern peatland region", delete this sub-clause. What does broad permafrost gradient mean?

We have removed the sub-clause and defined the specific permafrost gradient, ranging from the sporadic to continuous permafrost zones.

Line 100 – Here or in the Methodology, I would like to know what the real test of the study is – it turns out that the real test of the study is whether there is a significant interaction between FP and MAT.

We agree that the interaction between FP and MAT represents an important component of the analysis; however, it was not the sole test of the study. Our objective was, at its core, to assess how permafrost conditions (using MAT) influenced solute mobilization across contrasting hydrological conditions. The interaction between FP and MAT was used to evaluate whether hydrological responses differed along the permafrost gradient, but the study also assessed the influence of other catchment characteristics and hydrological conditions on solute dynamics.

Line 132 – Here and throughout the manuscript sentences that start with pronouns need to have the subject of the sentence stated.

Thank you. Corrected.

Line 208 – I don't understand the standardisation, it sounds like a partial z-transform.

Thank you. We apologize for the overexplanation when it is simply a z-transformation. This has been corrected. "The data for the PCA was z-transformed."

Line 210 – what does cautiously mea? I would delete it.

We used the term "cautiously" to indicate that additional model complexity was avoided unless supported by the data and ecological rationale. We have revised the text to explicitly state explain this. "The number of components were selected using the Kaiser Criterion for eigenvalues (>1) cautiously to ensure unneeded complexity was not introduced.". As well, in lines 213-216 we have this text providing further clarification: "Interpretation was therefore accordingly focused on the first two PCs as they captured the dominant variance structure and provided the clearest ecological gradients, and the additional components seemed to add unnecessary complexity if all five components were retained (Jackson, 1993)."

Line 220 – It needs to be made explicit what are the random and fixed factors in the LMM. In reality the only random factor is SiteID and the key term is FP MAT interaction.

Thank you. Corrected.

Line 255 – here and throughout the manuscript I would avoid the use of the word similar as it has no definition and is just a subjective judgement.

Thank you. Corrected.

Figure 4 – too cluttered and should be split between panels.

We agree that separating the panels may improve readability of the grouped/coloured relationships; however, we have retained the figure as a combined PCA because its primary purpose is to facilitate direct comparison of the relationships among multiple solutes and environmental variables under the differing hydrological conditions. Presenting the variables together allows readers to evaluate similarities and differences in response patterns across parameters, which we believe would be less apparent if the PCA was separated into panels.

Line 286 – the use of visual comparison and visually similar all seem too subjective and such comparison will tend to confirm unstated biases in the authors.

We have removed the "visual" aspect of this to make it more objective: "These relationships were consistently positive regardless of flow condition; likely due to consistent co-mobilization through similar biogeochemical processes regardless of flow condition. For instance, the relationships of TMeHg with DOC and A_{254} were positive at high and low flow conditions, albeit with stronger correlations during high flow periods."

Line 301 – wildfire must be a binary variable and so how was it included in the modelling? I don't think was made clear.

Wildfire was included as a fixed effect in the mixed-effects models as a binary categorical predictor (burned = 1, unburned = 0). It was not treated as a random effect because the objective was to estimate the effect of wildfire status on the response variable, rather than account for variation among multiple levels of a grouping factor. We have clarified this in the Methods.

Table 2 – this is a mess, remove the asterisks and just state that only significant factors and variables are shown. When coefficient is negative then you don't need the plus sign as well.

We have retained the asterisk notation because it distinguishes between different levels of statistical significance (i.e., $p < 0.05$, $p < 0.01$, $p < 0.001$), thereby providing additional information beyond simply identifying significant parameters. We believe this is a widely recognized and concise format that improves interpretation of the results. We did, however, remove the plus sign when the coefficient was negative, thank you for that note.

Line 329 – less apparent?

We have corrected and adjusted the wording of this.

Line346 – chemostatic is never defined – seems arbitrary to me.

It has now been defined as when solute concentrations remain relatively constant across a range of flows.

Line 349 – This sentence uses both strong and likely neither of which are defined or justified.

We agree that the term "strong control" was not explicitly quantified and could be interpreted as implying a statistical assessment of effect magnitude. We have revised the text accordingly: "Permafrost extent controlled the C-Q relationships of MeHg and DOC, likely by influencing hydrological flow paths and solute mobilization."

Line 391 – what is meant by unsaturated conditions?

By "unsaturated conditions," we are referring to peat plateau environments that are elevated above the surrounding landscape and typically occur above the water table, resulting in drier peat conditions compared with saturated peatlands such as thermokarst fens. We have revised the text to clarify this terminology: "While drier, raised peat plateaus in permafrost peatlands can produce DOC under unsaturated conditions, they are unlikely to be important sources of MeHg due to conditions that suppress the activity of microbial methylators, including cooler temperatures, a lack of terminal electron acceptors, and predominantly aerobic conditions (Olefeldt and Roulet, 2014; Thompson et al., 2025)."

Line 391 – here and throught the manuscript be careful to use significant only in the statistical sense.

Thank you. Corrected.

Line 413 – high flow and rought are not opposite conditions, the opposite of high flow is baseflow. So I am not clear what is the contrasting being drawn here

We agree that contrasting high flow with drought was incorrect. We intended to refer to dry conditions (i.e., low-flow conditions) rather than drought. We have revised the text accordingly to clarify that the comparison was between high-flow and low-flow conditions: "This stability contrasts with the non-permafrost region, where peatlands are more hydrologically dynamic, readily connecting to streams during high flows and disconnecting during dry conditions (Niu et al., 2011; Quinton et al., 2011; Connon et al., 2014; Walvoord & Kurylyk, 2016)."

Line 414 – delete Ergo

This has been deleted.

Line 422 – "showed slight" – what does that mean?

We have changed this to "weak evidence for flushing" to be more specific rather than "slight".

Line 453 – what does chemodynamic mean?

We have corrected this in the manuscript to be more clear, however by chemodynamic we mean that the concentrations do change in response to flow. We do agree, however, that it is confusing in this context, hence why it has been changed: "MAT significantly modified the concentration–discharge relationships of THg and TDP, suggesting that climate influenced the mobilization dynamics of these solutes across catchments."

Line 463 – what does concurrent behaviour mean here?

We intended to write coupled rather than concurrent.

Line 467 - "… was only relevant within the context and interaction of hydrological conditions…" I don't understand this.

The statistical result alone demonstrates an effect of catchment area within the context of the fitted Area × FP interaction, rather than directly demonstrating the underlying hydrological mechanism. Our intention was to convey that the influence of catchment area on SC was dependent on hydrological conditions, as indicated by the significant interaction term, rather than suggesting a standalone effect of catchment area. We have revised the text to clarify this distinction and have softened the mechanistic interpretation.

Discussion – I had expected a section on the limitations of the study and the implications of the study.

We have included a new section in the discussion labelled "4.5. Limitations, implications, and expected changes in a changing climate". This can be found below. Appropriate citations will be added into the references section. The current conclusion does read more like implications, so this has been moved to the Discussion and the Conclusion has been rewritten (see next comment):

While this study provides important insights into the hydrological and biogeochemical dynamics of streams along a permafrost gradient, several limitations constrained the scope and resolution of the findings. However, these limitations can provide opportunities for further research opportunities within the region. The large spatial and temporal scale made it challenging to maintain consistent sampling frequency across all sites; this imbalanced temporal resolution may have influenced comparative interpretations. Future studies would benefit from a more harmonized sampling regime across all sites to reduce potential bias and enhance temporal comparability (Beard et al., 1999). Related to this is the limited availability of catchment-scale geospatial datasets, particularly concerning detailed peatland composition, restricted the ability to link observed stream chemistry directly to underlying peatland types and distribution. Hydrological data is also constrained by the limited spatial coverage of official stream monitoring by Environment and Climate Change Canada (ECCC) due to the remoteness of the sampling locations. In cases where stream-specific data were unavailable, nearby gauged streams were used as proxies. While this approach provided general hydrological context, it likely masked some stream-specific variability. Integrating more catchment related research through high-resolution catchment mapping, remote sensing, as well as ground truthing with in-situ peat core and porewater sampling would provide new insights to the region (Ritchie et al., 2003). Such efforts would allow a more mechanistic understanding of solute mobilization pathways and their variability across peatland subtypes (St. Louis et al., 1994; Gordon et al., 2016; Thompson, 2023). Further investment in site-specific discharge monitoring would also enable more clear relationships to be established. The influence of wildfires on water chemistry remains difficult to assess under the drought-dominated conditions experienced during 2023 and 2024. Repeatedly dry years may have suppressed potential post-fire flushing effects (Hutchins et al., 2023). As such, continued monitoring during wetter years is essential to capture delayed or episodic wildfire-driven solute responses (Laudon et al., 2011; Rose et al., 2018).

Ongoing climate change is expected to intensify the frequency and severity of both extreme floods and droughts, accelerate permafrost thaw, increase solute mobilization, and increase wildfire occurrence across the circumpolar north (Nilsson et al., 2015; Dastoor et al., 2022; Wright et al., 2022; Burton, 2023). As these landscape disturbances continue to increase with time, we predict that the degradation of permafrost in northern peatlands may enhance the production and the mobilization of MeHg and DOC in currently low-yielding systems, as well as increase the sensitivity of these systems to floods and droughts, akin to what we have observed in the southern catchments of this study. This interpretation of future trajectories relies on a space-for-time substitution of these catchments (Wig et al., 2025). By using the spatial extent of permafrost conditions as an analogue to permafrost thaw, we predict that as climate change continues and hydrological variability increases alongside thawing permafrost (coupled with enhanced DOC and MeHg production with warmth), northern catchments will lose the stabilizing action of permafrost and flushing of these solutes may be observed. We assume that as permafrost thaws, northern permafrost-rich catchments will increasingly resemble southern, non-permafrost catchments. If this substitution holds, continued permafrost loss can be expected to increase the delivery of DOC and MeHg to downstream aquatic ecosystems as these northern ecosystems warm. Such changes have the potential to enhance eutrophication, accelerate MeHg bioaccumulation, alter water colour with increased DOC, and increase aquatic greenhouse gas emissions. These findings highlight the importance of long-term monitoring of DOC, MeHg, and nutrient fluxes in northern rivers, particularly within regions experiencing rapid permafrost degradation, to detect and manage emerging risks to water quality and ecosystem health, and to ensure traditional land use can occur safely.

Conclusion – this is not a conclusion section it is summary and interpretation – it needs a total re-write.

We have rewritten the conclusion, moving up the interpretations/implications to the above section and ensuring that this reads as a concluding remark:

In summary, this study highlights that permafrost conditions control the C-Q relationship of solutes and parameters that are associated with peatland connectivity and solute production, namely MeHg and DOC. Understanding how permafrost thaw and hydrological variability regulate solute dynamics is critical for projecting the downstream biogeochemical impacts of climate change across northern landscapes. By evaluating C-Q relationships across catchments spanning a permafrost gradient, this study demonstrates that permafrost extent exerts significant control on the production and export of MeHg and DOC. Specifically, permafrost acts as a key stabilizing element causing a chemostatic response; its presence decouples hydrological flushing from solute stores, whereas non-permafrost catchments exhibit enhanced mobilization, flushing, and heightened sensitivity to wet periods. This is in addition to well-studied seasonal influences which have been affirmed in this study, where spring freshet significantly influences concentrations. As ongoing warming continues to degrade permafrost and alter northern hydrologic regimes, permafrost-rich catchments are likely to transition toward the high-yield, flushing-dominated behavior currently observed in southern catchments. Consequently, permafrost loss will not only alter localized solute transport but will systematically increase DOC and MeHg loading to downstream aquatic ecosystems, with direct consequences for aquatic food web bioaccumulation, water clarity, and greenhouse gas dynamics.

Reviewer 2

General comments:

The manuscript is generally well written and presented. Figures are easy to read and represent the study adequately. This manuscript may be of interest to permafrost scientists, biogeochemists, hydrologists, and climatologists. However, a few major missing pieces are needed. These missing pieces can be addressed but until they are I suggest this paper needs major revision.

Thank you for your feedback, we have addressed the concerns below.

A space for time assessment must account for different bedrock, soil, vegetation, gradient, slope, and aspect characteristics of the watersheds. These are barely mentioned or addressed at all.

Thank you for this valuable comment. We agree that landscape-scale physical characteristics play an important role in shaping catchment hydrology and biogeochemistry. In our original submission, our primary focus was centered on permafrost extent because we had specific, mechanistic hypotheses linking permafrost thaw/distribution directly to MeHg and DOC dynamics. Additionally, a comprehensive regional characterization of these catchment parameters and their influence on water chemistry across 97 streams synoptically sampled three times in this exact study region was recently conducted by one of our co-authors, Renae Shewan (M.Sc. Thesis, University of Alberta, 2024; https://doi.org/10.7939/r3-hfk7-b706). This study showed that climate (and thus permafrost conditions) was the main control on MeHg and DOC concentrations, while geology, landcover, slope, and catchment size only had minor secondary effects. Our study chose 27 of these streams to sample more intensely to determine whether climate also influenced temporal variability. However, to directly address these concerns and verify whether these watershed attributes confound or explain additional variance in our dataset, we have now run our LMMs incorporating the data available to us, including: % Devonian geology, % forest, and mean slope degrees. This is in addition to the included % peatland cover, mean annual temperature, flow percentile, and catchment size. We have updated the Methods section appropriately to include these changes:

"Moreover, we ran supplementary models to account for additional physical catchment characteristics, hydrological condition, and seasonality that could influence water chemistry. This includes Devonian geology extent, forest cover, mean catchment slope, antecedent conditions (calculated as flow percentile conditions from 7 days prior), and season (spring versus summer/fall; categorical). For a list of the twelve candidate models generated for each parameter, and the supplemental models run, see Section S3 in the Supplementary Materials."

None of these catchment characteristics led to models that were more parsimonious than what we have presented, which is aligned with previous research in the region; we have included a sentence in the results to this effect: “Devonian geology, forest cover, slope, and antecedent conditions did not improve model fitness for any parameter of interest.”

We have also added the following sentence to our introduction for further clarification: “While static landscape characteristics (e.g., geology, forest cover, slope, topography, precipitation, etc.) across this study region were previously evaluated across 97 streams by Shewan (2024), our study sampled 27 of these streams to prioritize high-resolution temporal sampling of flow dynamics and permafrost controls.”

Also the authors barely mention how the timing of a given flow regime/sample collection affects catchment responses. For example spring melt versus late fall or high versus low flow.

In addition to running the models to include the above physical catchment characteristics, we have run models for each of the parameters with a binary categorical indicator of seasonality, where 0 indicates summer/fall conditions and 1 indicates spring (freshet) conditions. This variable does improve the already selected best fitting model for: TMeHg, DOC, and TDN. These were: Parameter ~ FP + MAT + FP:MAT + (1 | ID). Now they are: Parameter ~ FP + MAT + FP:MAT + Season + (1 | ID). Further, NH4-N and NO2-N + NO3-N previously had no well fitting model, but season improved these. These are the updated most parsimonious models, with the bolded sections reflecting changes:

TMeHg = 0.204 * (Intercept) + 0.080 * FP + 0.072 * MAT **- 0.058 * Season1** + 0.052 * FP:MAT

DOC = 22.021 * (Intercept) + 3.655 * FP + 3.431 * MAT **- 7.004 * Season1** + 1.277 * FP:MAT

TDN = 0.769 * (Intercept) + 0.027 * FP + 0.118 * MAT **+ 0.083 * Season1** + 0.036 * FP:MAT

NO2N+NO3N = 28.197 * (Intercept) **+ 99.518 * Season1**

NH4N = 29.798 * (Intercept) **- 18.206 * FP + 183.821 * Season1**

Therefore, this means that for TMeHg and DOC, the models predict that **spring concentrations are significantly lower** than summer/fall concentrations, holding FP, MAT, and the FP × MAT interaction constant. For the nitrogen species, the models predict that **spring concentrations are significantly higher** than summer/fall concentrations, with NH4-N decreasing with flow as well. We hypothesize that this is likely driven by either a dilution due to higher flows during freshet, or less efficient production in catchment peatlands due to a colder environment. Since flow percentile significantly increases concentrations of both TMeHg and DOC, it is implausible that a dilution with increased flows is driving lower springtime concentrations and it is likely microbially driven via temperature. Nitrogen species, on the other hand, are evidently being significantly flushed during the high flow spring freshet periods, especially the inorganic species. However due to sampling constraints (ice conditions, site access, potential sampling period etc.) there was considerably more sampling in the summer/fall (June - October; n = 322; ~64 samples per month) compared to spring (April & May [2 months]; n = 77; ~38 samples per month). We have updated Table 2 in the manuscript to show these new findings, have described this in the results, and included further interpretation and citations to the discussion to reflect what was described above as well.

Most of the results/findings of this work have already been shown in other regions and I recommend weaving some of these studies into the discussion.

We appreciate the recommendation to ground our findings in the broader regional context. We have further expanded our review and woven additional relevant studies into this manuscript following the edits that have come from this review process. This includes the following (but not limited to) studies that we have referenced with prior findings in similar regions, including work done in Scandinavia (Laudon et al., 2011; Blaen et al., 2014; Olefeldt and Roulet, 2014; Gindorf et al., 2025), Siberia (Frey et al., 2007; Vorobyev et al., 2017), Alaska (Shogren et al., 2019), the UK (Clark et al., 2008; Ramchunder et al., 2011), and northern Canada (Quinton et al., 2011; Burd et al., 2018; Thompson et al., 2025).

For a given catchment sampled during a variety of flow regimes what was the range of values? How does this relate to the "differences" reported here?

We agree that the range of concentrations observed within individual catchments across the sampled flow regimes provides important context for the differences among catchments. The distributions of each solute across Site IDs are presented in Figures 2 and 3, which illustrates the within-catchment variability associated with the range of hydrological conditions sampled. We also have provided the flow percentile range captured at each site in Table 1. Additionally, the mixed effects models visualized in Figures 6 and 7 accounted for flow percentile and therefore evaluated whether catchment characteristics were associated with water chemistry across this hydrological variability rather than at a single flow condition. For catchments in the warmer parts of the study region, DOC and TMeHg concentrations were 50% and 80% higher during high flow (>90th percentile) than low flow (<10th percentile), while this difference was 10% and -30% for catchments in the colder regions. Thus the observed effect of climate on the C-Q relationship is highly significant from both a statistical and ecological perspective.

For a given catchment sampled in spring or late summer what was the range of values? How does this relate to the "differences" reported here?

We have produced boxplots that will be added to the Supplementary Materials (S4; and referenced in text) to show the range of values for each of the 12 parameters at each site, split by season, with spring being April and May, and Summer/Fall being June – October to help visualize seasonal differences. These boxplots are on the next page. This is in addition to the addition of season into our statistical analysis (mixed effects models) that show a significant influence on TMeHg, DOC, TDN, NO_2-N + NO_3-N, and NH_4-N.

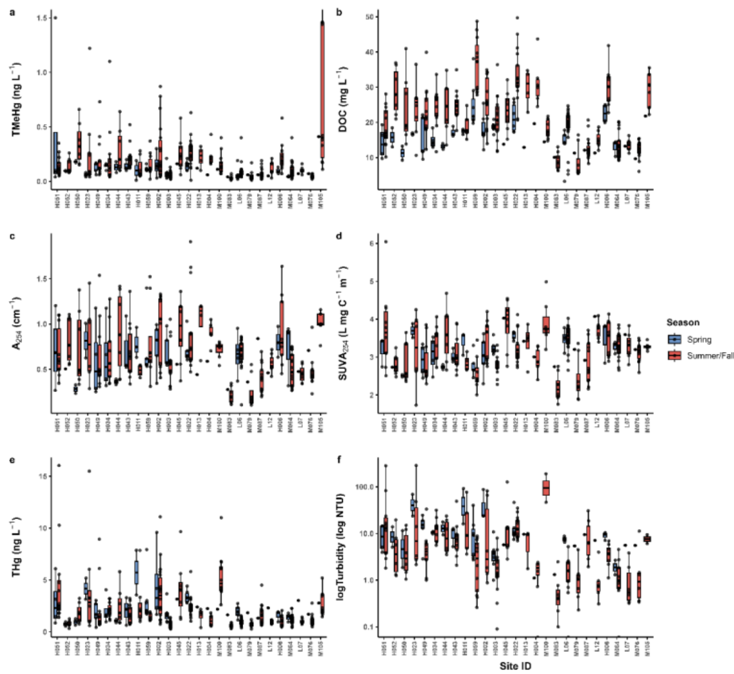

Figure S3. Boxplots of water chemistry parameters across 27 sites by Site ID, and coloured/ordered by MAT. Panels show (a) TMeHg, (b) DOC, (c) A_{254}, (d) $SUVA_{254}$, (e) THg, and (f) logged turbidity. Bolded site IDs indicate catchment areas greater than 10,000 km2. Further, each site has been split by season as well to assess differences in water chemistry spread per site, between seasons. The number of samples with high concentrations not shown in the figure are indicated on top right.

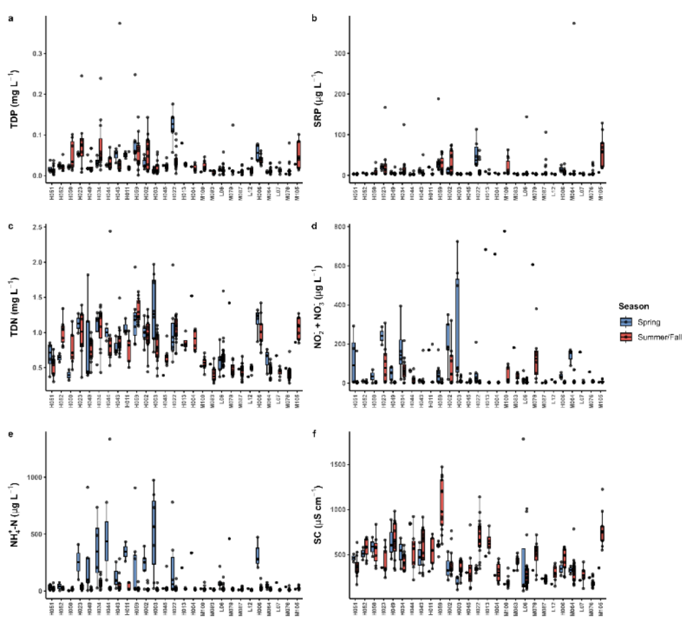

Figure S4. Boxplots of water chemistry parameters across 27 sites by Site ID, and coloured/ordered by MAT. Panels show (a) TDP, (b) SRP, (c) TDN, (d) NO_2^--N + NO_3^--N, (e) NH_4^+-N, and (f) SC. Bolded site IDs indicate catchment areas greater than 10,000 km2. Further, each site has been split by season as well to assess differences in water chemistry spread per site, between seasons. The number of samples with high concentrations not shown in the figure are indicated on top right.

For a given catchment sampled immediately following a major precipitation event or during a dry period what was the range of values? How does this relate to the "differences" reported here?

We will address this by incorporating an antecedent flow metric into our analysis using flow percentile, as widespread precipitation data at a high enough resolution is not available in our study region. For each sampling event, we have calculated the difference between the flow percentile (FP) on the sampling day and that observed seven days prior ($\Delta FP = FP_{sampling\ day} - FP_{7\ days\ prior}$). Positive values represent increasing discharge leading up to sampling, whereas negative values represent declining flow conditions. We added this metric to our mixed effects models, including the existing best performing models outlined in the manuscript; antecedent conditions did not improve model fitness for any parameter. Although antecedent hydrological conditions may contribute to within-catchment variability, antecedent conditions do not provide a consistent explanation for the broader among-catchment differences reported in this study.

Perhaps add a map of fire scars by year to the supplemental information?

Wildfire history was considered as a potential catchment characteristic; however, wildfire was not found to be an important explanatory variable for the water chemistry patterns examined in this study. We therefore chose not to include a map of fire scars by year in the supplementary materials, as we felt that it would not contribute meaningfully to the interpretation of the results.

I recommend presenting the geologic differences in the Study Site section and providing a general geologic map of the study region in the Supplemental. Also, a basic ecotype or vegetation map, a permafrost map, and a basic soils map in the Supplemental. And percent coverage of these different characteristics as represented "downstream" at a given sample location. Without this information the authors are excluding a broad variety of potential controls on their biogeochemical measurements and what they really represent.

The regional geology, vegetation/ecotypes, permafrost, and soils have been described in detail in Shewan's thesis, which provides comprehensive characterization of these landscape attributes across the study region. Given that this information is already available in detail and that these characteristics were not the primary focus of the present study, we have chosen not to reproduce additional regional maps in the Supplementary Materials. Instead, we focus our analyses on the hydrological and catchment characteristics most directly relevant to our research questions and hypotheses.

These potential drivers of different stream chemical characteristics are being ignored with the focus on "climate change" when it is possible that there is not much of a climate signal at all. They are hanging their entire objective on a space for time (climate) aspect of the study but are ignoring the variety for other drivers that may affect flow regimes and biogeochemical characteristics. Perhaps the general discussion can address that the area is warming and permafrost is thawing but until these physiographic differences are adequately addressed the study impact is limited.

Thank you for this important consideration. The objective of this study is not to attribute observed differences in stream chemistry solely to climate change, nor to assume that climate is the only factor controlling flow regimes and biogeochemical characteristics. Rather, we examine how hydrological variability interacts with catchment characteristics to influence water chemistry across the study region. The broader physiographic characteristics of the region, including geology, vegetation, soils, and permafrost, have been comprehensively characterized in Shewan's thesis and provide important context for interpreting the study catchments, and is in the process of being published. As described above, we have run our models to determine whether these characteristics can explain the differences in water chemistry, and all but seasonality did not improve our models.

Comments keyed to the text:

20-21: "climate and this permafrost": suggest a reword. Climate controls all sort so hydrological processes in catchments worldwide whether permafrost is present.

We have corrected and adjusted the wording of this. "Our results demonstrated that MAT, as a proxy for permafrost conditions,"

364-265: and the soils and vegetation are also different in this "drier climate" region yes?

We agree that differences in soil characteristics, vegetation communities, and climatic conditions among permafrost regions can influence hydrological and biogeochemical processes. The Taiga Plains represent a relatively dry permafrost environment with distinct peatland, vegetation, and soil characteristics compared with other boreal and temperate regions. We have clarified that in the text of the manuscript.

382-384: "We postulate that the absence of a flushing C-Q relationship in the permafrost region of our study is due to the role of permafrost a higher proportion of peatland cover remains hydrologically disconnected from the stream network regardless of hydrological conditions (Quinton, 2009)."

Hard to follow this…

We agree that this was poorly written. We have corrected and split the sentence to improve clarity: "We hypothesize that the presence of widespread permafrost in peatlands results in the absence of a flushing C-Q relationship. Widespread permafrost in peatlands is associated with isolated thermokarst bogs, which remain disconnected from the stream network regardless of hydrological conditions (Quinton et al., 2009)."

453-456: I recommend presenting these geologic differences in the Study site area and providing a general geologic maps of the study region in the Supplemental

We have responded to a similar comment in the General Comments section.