

Response to Reviewers of bg-2020-472

General response to the Editor

Associate Editor's comments:

Dear Authors,

I thank you for providing detailed responses to the comments and suggestions offered by two reviewers.

Both reviewers recognized the importance of your exploratory study in the Shaliu River on the Qinghai-Tibetan Plateau, an understudied region in the study of the global carbon cycle, and the novelty of your findings on the seasonal shift in the hydrologically driven export of soil organic carbon. However, the reviewers also raised several critical issues including articulating key messages and structuring the Results and Discussion sections. While your responses well addressed most reviewer comments, I add additional suggestions below to facilitate your revision. I envisage that the manuscript would require a substantial revision to address all raised issues and suggestions. Therefore, I have to recommend 'reconsider after major revisions' and might need to ask the reviewers to reevaluate the revised manuscript.

[*Detailed comments were shown below...*]

I would like to ask you to make all the changes easily identifiable in a marked-up manuscript based on your point-by-point responses to the comments offered by the reviewers and myself. Please also specify the line numbers of the marked-up manuscript in your responses to comments.

Sincerely,

Ji-Hyung Park

Associate Editor, Biogeosciences

We sincerely thank the editor and reviewers for the supportive and stimulating comments. Based on the suggestions, we have made substantial changes to our manuscript.

For your convenience, the original comments are listed below in black and our replies follow in blue font. As shown in our detailed responses, we have made every effort to address the concerns of all reviewers. We sincerely hope that our responses have adequately addressed all the comments. Thank you again for your consideration.

Detailed comments:

- Definition of headwater streams: Headwater streams have often been restricted to the first- to third-order streams in the literature. Please define your use of "headwater streams" and explain how you can consider the main stem and tributaries of the Shaliu River based on specific stream order information for the river system.

According to Lin et al. (2019), stream order information for the Shaliu River is added in Lines 90-91 and Figure 1a as follows:

"The Shaliu River is composed of first- to third-order streams (Figure 1a) and hence a typical headwater stream (Lin et al., 2019)."

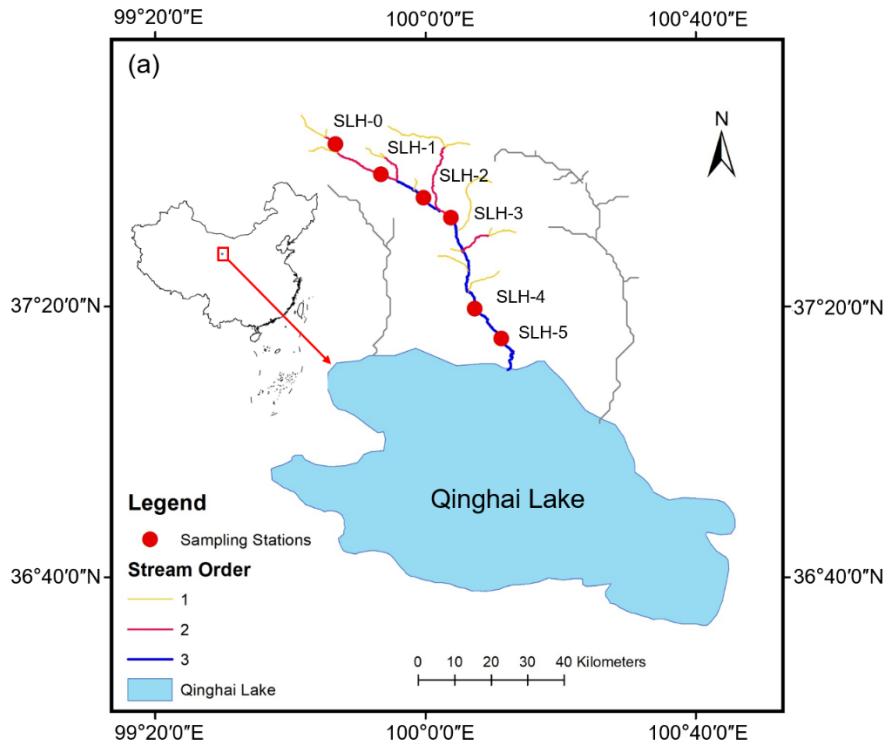


Figure 1a. Sampling sites along the Shaliu River (a). The map in panel (a) is processed with ArcGIS 10.0. Stream order information is obtained from Lin et al. (2019).

Reference:

Lin, P. R., Pan, M., Beck, H. E., Yang, Y., Yamazaki, D., Frasson, R., David, C. H., Durand, M., Pavelsky, T. M., Allen, G. H., Gleason, C. J., and Wood, E. F.: Global reconstruction of naturalized river flows at 2.94 million reaches, *Water Resour. Res.*, 55, 6499-6516, <https://doi.org/10.1029/2019wr025287>, 2019.

- Study objectives: It appears that you wanted to focus on the unique hydro-biogeochemistry of the Shaliu River as a “headwater” system. On the other hand, you gave less priority in linking your findings (and citing latest publications) to the growing interest in the carbon export from the permafrost of the Qinghai-Tibetan Plateau. Please articulate your primary objectives in the last paragraph of Introduction and the abstract (“To assess these aspects?”).

We do want to illustrate the unique hydro-biogeochemistry of riverine carbon in alpine headwater streams on the Qinghai-Tibetan Plateau (compared to tropical headwaters that are more extensively studied in the literature). The latest research on carbon export from the Qinghai-Tibetan Plateau permafrost and primary objectives of our study are added in *Lines 12-16, Lines 61-66 and Lines 70-71*.

- The hydrology during the thawing period (snowmelts): As your new description of the thawing-induced hydrologic changes nicely explain (“Thawing frozen soils and deepening of active layers can strongly affect catchment hydrology, including creating vertical and lateral flow paths, increasing soil filtration, enhancing groundwater-surface water exchange and baseflow”), “lateral flow paths” would also bring substantial amounts of carbon from the surface soil horizons (this has been observed in many systems including even temperate systems). However, you tend to contrast (in a simplifying way) the dominance of deep paths

during the thawing period and the surface flows during the monsoon storm events. I wondered if the rising water table during thawing or snowmelt events would not flush carbon from the surface horizons rich in carbon. Please provide some discussion of the relative importance of the lateral and vertical flow (or deep vs. shallow) paths during thawing or snowmelt events.

Good point! The rising water table during thawing or snowmelt events might bring amounts of carbon from surface soils into streams. However, many research show that streamflow supplied by surface meltwater mainly occurs at the early pre-monsoon season (temperature between -2°C and 0°C), and streamflow would shift from surface meltwater to water stored in subsurface frozen soils (i.e., an increasing contribution of deep soil waters) as air and soil temperature rises (Figure R1; Carey and Quinton, 2004; Tetzlaff et al., 2015; Wang et al., 2017). This transition in streamwater sources indicates the increasing contribution of deep soil water to streamflow with the progress of thawing events. The above explanations and discussion are added in *Lines 358-363*.

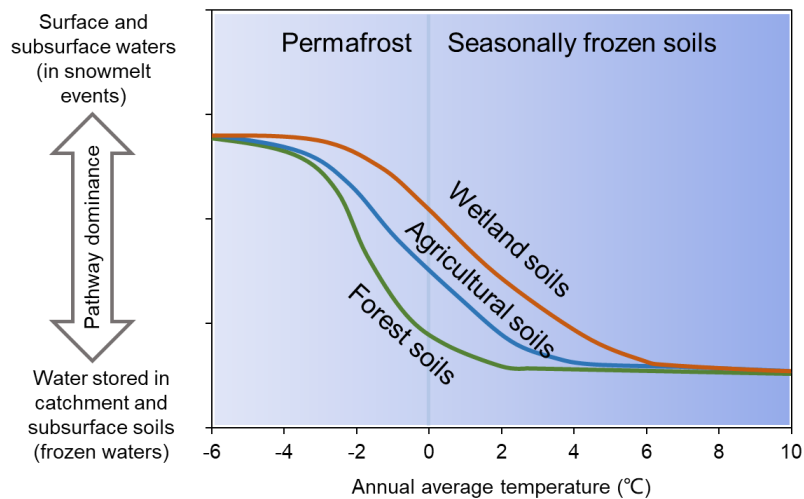


Figure R1. Conceptual model of the hypothesized change in dominant pathways contributing to stream water dominated by different land cover in response to changes in air temperature. Modified from Tetzlaff et al. (2015).

References:

Tetzlaff, D., Buttle, J., Carey, S. K., McGuire, K., Laudon, H., and Soulsby, C.: Tracer-based assessment of flow paths, storage and runoff generation in northern catchments: a review, *Hydrol. Process.*, 29, 3475-3490, <https://doi.org/10.1002/hyp.10412>, 2015.

Carey, S. K. and Quinton, W. L.: Evaluating snowmelt runoff generation in a discontinuous permafrost catchment using stable isotope, hydrochemical and hydrometric data, *Nord. Hydrol.*, 35, 309-324, 2004.

Wang, G. X., Mao, T. X., Chang, J., Song, C. L., and Huang, K. W.: Processes of runoff generation operating during the spring and autumn seasons in a permafrost catchment on semi-arid plateaus, *J. Hydrol.*, 550, 307-317, <https://doi.org/10.1016/j.jhydrol.2017.05.020>, 2017.

- L274-275 (response to the first reviewer comment): Can't you find precipitation data at a nearby site, like in some weather stations near the Lake Tsinghai? You talked about the carbon export "peaking in the summer due to high discharge brought by the monsoon", but it's really challenging to follow up this without resorting to rainfall data.

Precipitation data near the Qinghai Lake is added as Figure S1 (as below) to display the

potential effects of monsoon (June-September) on rainfall and river discharge. For reviewer #1' comments "L274-275: How much precipitation fell during this rain event?", a rainfall of 1.0 mm on the same sampling date (16 August, 2015) at Gonghe weather station near the Qinghai Lake is extracted and added in *Lines 134-135*.

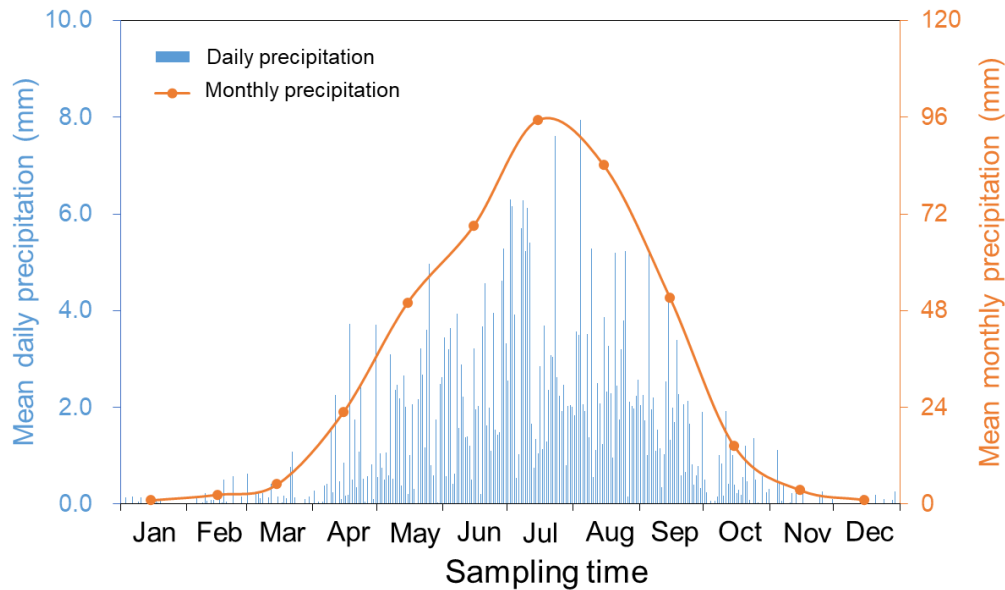


Figure S1. Variations of the mean daily and monthly precipitations during recent ten years (2009–2018) at Gonghe weather station near Qinghai Lake (data modified from <http://data.cma.cn/data/index/6d1b5efbdc9a58.html>).

- Geological information: Given the quantitative importance of DIC, it would help readers if you provide some information (and discussion) on the distribution of bedrocks in the basin.

Good point! Relevant information is added in *Lines 95-96*, *Lines 311-312* as below:

“The soils in the basin are mainly Gelic Cambisol (IUSS working group WRB, 2015) underlain by a dominance of Triassic sandstone, late Cambrian metamorphic rocks (schist and gneiss) and granites (Zhang et al., 2013).”

“The distinct lithology within the Shaliu River basin results in high carbonate and silicate weathering rate (Zhang et al., 2013), which is an important source of DIC in river water.”

- Details on sampling and analysis: Given the importance of carbon analysis in this study, you need to provide more details on sampling bottles (glass or plastic? Acid-washed?), SPE, TOC analyzer (high-temperature oxidation? Accuracy?), QC...

Relevant information is added in *Lines 110-112*, *Lines 160-166*, *Lines 149-153* in the Materials and Methods section.

Response to Reviewer #1’s comments

This paper investigates land-freshwater linkages and riverine carbon dynamics in the Shaliu River on the Qinghai-Tibetan Plateau, where there is growing interest to quantify the magnitude and sources of terrestrial carbon mobilized into freshwaters within permafrost-affected watersheds. To achieve this, the authors pursue three objectives: to (1)

determine seasonal and annual riverine carbon fluxes; (2) using biomarkers, constrain variability in riverine carbon sources across seasonal shifts in hydroclimate (pre- and post monsoon); and (3) assess precipitation effects on carbon mobilization into rivers during a rainfall event. Potential hydroclimate effects on riverine carbon sources are a particularly interesting component of this study. This paper is suited to Biogeosciences and could help to advance understanding of carbon cycling and land-freshwater linkages in permafrost-affected terrains. However, considerable revisions are needed to better articulate the key messages of this study and to clarify pertinent methods and environmental effects (e.g. freeze-thaw dynamics) on carbon cycling. The comments below are intended to help improve the clarity and depth of your manuscript.

We greatly appreciate the reviewer's positive assessment of our manuscript. We have made substantial changes to our manuscript (details below) and hope that our responses have adequately addressed all the comments.

Major comments:

1. This paper aims to present an interesting story, but the combined Results and Discussion section is a main obstacle to the authors clearly articulating (and the reader grasping) data trends and the key messages. I think that separating the Results and Discussion will help this paper to more fully reach its potential. The geochemical analyses are interesting and it would help if they were clearly presented in a separate Results section. Consider structuring your Discussion around the objectives you nicely summarize in L60-66.

The Results and Discussion section is now separated and re-written. Please refer to the highlighted version for details.

2. The effects of spatiotemporal variation in freeze-thaw dynamics on C cycling dynamics should be considered in more detail. For instance, Figure 1 nicely illustrates that “freezing period” and “thawing period” (thaw period defined as soil temperature > 0°C, L103) vary by site and depth in the soil profile (even though the box for “freezing period” suggests it has a strictly-defined time interval, approximately December 10 – March 25). This makes me wonder: How do you account for the spatiotemporal variability in freeze-thaw periods in your interpretations? If the frozen status of soil influences C mobilization into the Shaliu River, then can we presume that variability in timing of soil thaw along your sampling sites and across the watershed would influence the quantity and composition of OM entering streams? Please elaborate on this.

Good point! The spatiotemporal variation of freezing-thawing periods affects riverine carbon dynamics in the following two aspects. First, the earlier thawing of frozen soils downstream (at a lower elevation) in the Shaliu Basin enhances soil carbon release compared with the upstream basin during the freezing-thawing period, likely leading to an increasing carbon concentration along the river continuum (Song et al., 2019; Vonk et al., 2015). Second, the thawing depth of frozen soils increases with time in the pre-monsoon season due to increasing temperature, thus causing an increasing riverine carbon concentration with thawing events (Wang et al., 2017; Song et al., 2019). The above discussion is added in *Lines 379-384* of the revised manuscript.

References:

- Song, C. L., Wang, G., Mao, T. X., Chen, X. P., Huang, K. W., Sun, X. Y., and Hu, Z. Y.: Importance of active layer freeze-thaw cycles on the riverine dissolved carbon export on the Qinghai-Tibet Plateau permafrost region, *PeerJ*, 7, 25, <https://doi.org/10.7717/peerj.7146>, 2019.
- Vonk, J. E., Tank, S. E., Bowden, W. B., Laurion, I., Vincent, W. F., Alekseychik, P., Amyot, M., Billet, M. F., Canario, J., Cory, R. M., Deshpande, B. N., Helbig, M., Jammet, M., Karlsson, J., Larouche, J., MacMillan, G., Rautio, M., Anthony, K. M. W., and Wickland, K. P.: Reviews and

syntheses: Effects of permafrost thaw on Arctic aquatic ecosystems, *Biogeosciences*, 12, 7129-7167, <https://doi.org/10.5194/bg-12-7129-2015>, 2015.

Wang, G. X., Mao, T. X., Chang, J., Song, C. L., and Huang, K. W.: Processes of runoff generation operating during the spring and autumn seasons in a permafrost catchment on semi-arid plateaus, *J. Hydrol.*, 550, 307-317, <https://doi.org/10.1016/j.jhydrol.2017.05.020>, 2017.

3. The Introduction focuses on headwater streams. While interesting, it would greatly benefit the reader to include more background information on other pertinent components of your study, like DOC and DIC sources in permafrost regions and on the Tibetan Plateau (e.g. Song et al. 2020, DOI 10.1088/1748-9326/ab83ac), what lignin phenols can reveal about OM composition, how hydrology changes during freeze-thaw cycles, etc. This would help to familiarize the reader with key concepts which are at the foundation of your study.

Good point! The relevant background information is added in *Lines 56-61, Lines 61-66, Lines 78-81*.

4. As detailed in my comments below, it appears that reporting of data and statistics is incomplete. Please see my comments below regarding the ANOVA (in minor comments), L302, and Figure 3.

Data and statistics are added to the Supporting Information as “*Dataset for LOADEST*” and in *Lines 201-202* of the main text as below:

“Differences in the Ad/Al ratios between soil solutions and leachates at SLH-1 station during thawing events were determined using one-way ANOVA followed by post-hoc test.”

Minor comments:

1. Sec 2.3. What is the analytical uncertainty of your TC and POC analyses? Please report this. It would generally useful to know and would also help to assure the reader that your [PIC] values (Table 1) are robust and not within the range of analytical uncertainty for TC or POC.

The analytical precision (standard deviation for repeated measurements of standards) is $\pm 0.1\%$ and is added in the text.

2. Particulates are interesting and important for considering C species and mobilization in cold regions. Although particulates account for a relatively small proportion of total C (Table 1), it would be interesting to elaborate on trends in particulate C, or at least consider them within the broader perspective of particulate mobilization in permafrost terrains.

Particulate dynamics are added in *Lines 287-288, Lines 340-343*.

3. ANOVA is missing from the summary of statistical analyses you performed (Sec.2.6). Further, from your Results (Fig. 5d, L269), it seems that you must have done a post-hoc test following the ANOVA to determine which categories differed and to assign the letters indicating this. Please clarify.

This is now clarified in the *Materials and Methods* section.

Additional comments:

L52, L72, L78, L245, etc.: Unlike the active layer, permafrost is not a seasonal phenomenon. Permafrost is defined as ground material remaining at or below 0° C for two or more consecutive years (Muller 1943). Therefore, “seasonally thawed permafrost” should be replaced with “active layer”. Further, you nicely demonstrate that increases in riverine C pre-monsoon may be sourced from the active layer, but there is no evidence to support that the OM originates from permafrost (L15-18). Additionally, the correct definition of active

layer should be provided early in the manuscript, to provide clarification.

Muller, S.W., 1943. Permafrost or permanently frozen ground and related engineering problems. Special Report, Strategic Engineering Study, Intelligence Branch, Office, Chief of Engineers, no.62, 136 pp. Second printing, 1945, 230 pp. (Reprinted in 1947, J.W. Edwards, Ann Arbor, Michigan, 231 pp.)

Thank you for the correction! We realize that our study area, although falling in the Qinghai-Tibetan Plateau permafrost zone, harbors both permafrost and seasonally frozen ground/soils not underlain by permafrost. As we do not have deep ground temperature data to confirm the distribution of permafrost within the basin, we now use “seasonally frozen soils” in the revised text to avoid misunderstanding. This is now explained in the Introduction. Your thorough correction is much appreciated.

L12: What is meant by “divergent carbon transport dynamics”?

We meant that the carbon transport dynamics of headwater streams may be different from large rivers. This is rephrased.

L14-16: High discharge facilitated DIC production. What actually caused it? Enhanced chemical weathering of minerals associated with increased precipitation?

This sentence may be misleading. We have revised this sentence in *Lines 18-19 as below*:

“We show that riverine carbon fluxes in the Shaliu River was dominated by dissolved inorganic carbon, peaking in the summer partly due to high discharge brought by the monsoon.”

L18: As noted above, there is no evidence provided to support your attribution of a permafrost C source.

The “permafrost” is revised as “frozen soils” in *Line 22*.

L61-62: “. . . annual fluvial carbon fluxes on a monthly basis. . .” is a bit unclear. It would be clearer if you change to something like, “. . . to estimate monthly dissolved and particulate carbon fluxes for one year.”

Thank you! This sentence is revised accordingly in *Lines 73-74*.

L62: “bulk” as in “bulk concentration”?

The “bulk” is revised as “bulk concentration” in *Line 74*.

L63: “dense” = high temporal resolution?

Here “dense” means high spatial resolution, and we have revised the word “dense” to “a high spatial resolution” to avoid ambiguity in *Line 75*.

L70: What is the annual discharge of the Shaliu River? Would be interesting to know.

The annual discharge of $25.4 \text{ m}^3 \text{ s}^{-1}$ (Wu et al., 2019) is added in *Lines 88-89*.

Reference:

Wu, H. W., Zhao, G. Q., Li, X. Y., Wang, Y., He, B., Jiang, Z. Y., Zhang, S. Y., and Sun, W.: Identifying water sources used by alpine riparian plants in a restoration zone on the Qinghai-Tibet Plateau: Evidence from stable isotopes, *Sci. Total Environ.*, 697, 11, <https://doi.org/10.1016/j.scitotenv.2019.134092>, 2019.

L86 and Sec. 2.5: Because discharge was measured only at SLH-4, I presume that C fluxes were estimated using the concentration measurements from SLH-4? Please clarify in the Methods and Results.

It is clarified in the revised manuscript that carbon fluxes were estimated using data from SLH-4 station in *Line 186, Line 211 and Line 214*.

L104-105: What is a “pre-arranged ceramic head”? Is this a porewater sampling device, like lysimeter?

Yes, the ceramic head is a porewater sampling device like soil pore water suction lysimeter, which is composed of a porous ceramic head connected with a small diameter tube for pulling a vacuum and retrieving the sample. Relevant explanation is added in *Lines 129-130*.

L165-167: Does the statistical analysis for downstream trend in [DOC] account for autocorrelation among samples? (see also comment for Figure 3)

We tested the autocorrelation of DOC concentrations among samples using Durbin-Watson test, and the results (Durbin-Watson values around 2) indicated no significant autocorrelation (Table R1 below). The clarification is added in Methods section in *Lines 204-205*.

Table R1. Model Summary of Durbin-Watson test^b.

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate	Durbin-Watson
1	.964 ^a	.929	.911	.13273	1.749

^a Predictors: (Constant).

^b Dependent Variable: DOC

L175: Based on your pH values reported in Table S2, it might be worth noting here that DIC was primarily $\text{HCO}_3^- + \text{CO}_3^{2-}$, rather than CO_2 .

Thank you! Relevant information is added in *Lines 306-307*.

L181: Units of concentration should be consistent throughout the paper (mg L^{-1} , mmol L^{-1} in Figure S1b).

The Figure S1b is revised as Figure S2b.

L225-227: This text is better suited for a Discussion.

This text is included in the Discussion section of revised manuscript in *Lines 347-358*.

L227-230: Useful rationale for this analysis. It would help the reader if this were earlier, perhaps in the Methods (end of Sec. 2.4).

This rationale is added in the Methods in *Lines 182-183*.

L232-234: Interesting. This text would fit nicely in a Discussion.

This text is included in the Discussion section in *Lines 353-358*.

L243-245: Another example of good Discussion material.

This text is included in the Discussion section 4.3.

L246: “. . . soil-river carbon transfer inducing riverine carbon variations. . .”. I have no idea what this means! Please clarify.

This sentence is clarified in *Line 272* as “*To reveal the riverine carbon variations induced by soil-river water transfer in the Shaliu River...*”

L247: By “anticipated” do you mean “hypothesized”? It would be interesting and helpful if you clarified your hypotheses early on in your paper, perhaps at the end of the Introduction.

Thank you! Our hypotheses are added in *Lines 81-84*.

L249-250: But, this increase was only in topsoil. Was it a significant increase? Would help to clarify.

It was only an increasing trend (not a significant increase). The sentence is clarified in *Lines 274-275* as “*Topsoil DOC and lignin phenols showed an increasing (albeit not statistically significant) trends from 19.1 to 22.0 mg L⁻¹...*”

L252-254: How could thawing of subsoil (active layer thickening) increase DOC in the topsoil? Especially given subsoil [DOC] appears to be lower than topsoil [DOC]? (Fig.5a,d) Would subsoil DOC not be mobilized downslope as the active layer thaws?

Thank you! Our previous phrasing was unclear. The increase of DOC in topsoil solution over time was not caused by thawing of subsoil. The increase was likely caused by carbon release from partially frozen topsoil and/or inputs via lateral flow paths. This sentence is revised to avoid ambiguity in *Lines 372-375*.

L274-275: How much precipitation fell during this rain event? This would be interesting to know, as rainfall can be generally important for mobilizing sediments and POC (e.g. Beel et al. 2018).

Beel, C. R., Lamoureux, S. F., & Orwin, J. F. (2018). Fluvial response to a period of hydrometeorological change and landscape disturbance in the Canadian High Arctic. *Geophysical Research Letters*, 45(19), 10-446.

Unfortunately, we did not monitor the rainfall during this short-term precipitation event due to logistical reasons. However, rainfall on the same day (16 August, 2015) at Gonghe weather station (near Shaliu River and Qinghai Lake) is added to show the potential rainfall within this basin. In addition, as rainfall normally increases (i.e., accumulates) over time within one rain event, rainfall influences on mobilizing sediments and POC may partially be deduced from the positive correlations of time points (sampling time within the rain event) with TSS concentrations ($p < 0.05$). Relevant information and illustration are added in *Lines 134-135, Lines 387-389*.

L302: Data Availability: It does not appear that all data are available within the paper and Supplement. I was interested in exploring the raw data used in LOADEST (Sec.2.5) to estimate C fluxes, but I could not find this data. Please make it available, as indicated.

Data is added as “*Dataset for LOADEST*” in the Supporting Information.

Table 1. From L274-275, the time points at which these measurements were made is important. Please include this information.

Time points are added in Table 2 of the revised manuscript.

Figure 2: (a) The terminology here (“. . . concentrations exported. . .”) could be clearer. In other words, the points show measured concentrations and the lines show modeled concentrations from LOADEST? (b) I think It would be more interesting and useful here if you showed measured fluxes as points and modeled fluxes (from LOADEST) as a line. This would allow the reader to more easily visualize DIC and DOC fluxes and assess model fit. Instantaneous discharge is shown in (a), so I think it would be redundant to include in (b).

Thank you! Figure 2 is revised as below.

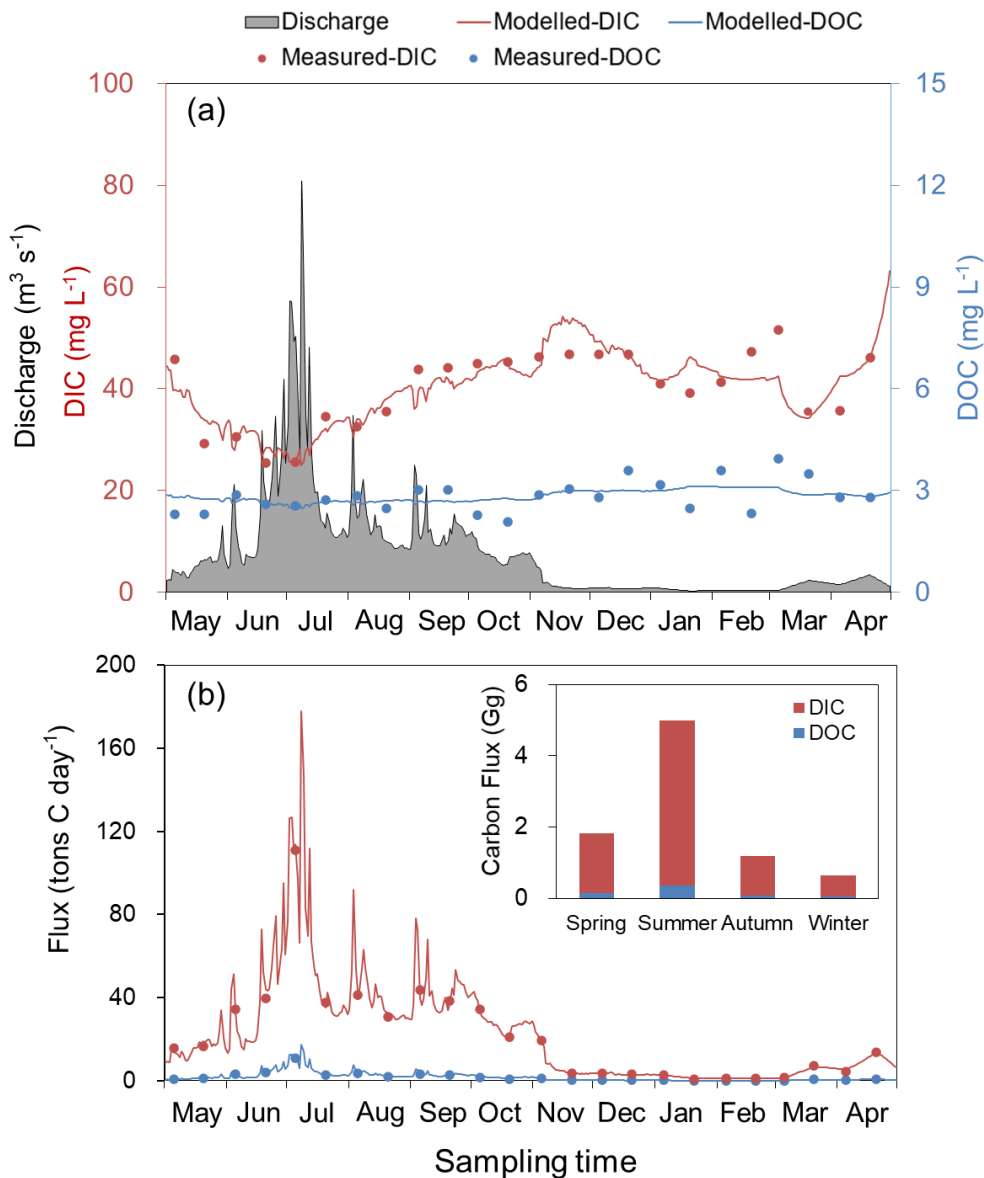


Figure 2. Discharge, dissolved inorganic carbon (DIC) and dissolved organic carbon (DOC) concentrations (a) and fluxes (b) exported from Shaliu River at SLH-4 station during 2015 and 2016. The modelled concentrations in (a) and modelled fluxes in (b) are derived from load estimator (LOADEST). The inserted columns in panel (b) show the seasonal variations of carbon fluxes classified as follows: spring (May to June), summer (July to September), autumn (October to November), and winter (December to the next April).

Figure 3: (a) Does the statistical analysis for downstream trend in [DOC] account for autocorrelation among samples? (see also comment for L165-167) Trends in geochemistry along the Shaliu river reported in Sec. 3.2 would be more clearly shown if (b) and (c) were plotted as points vs. distance, as in (a). (d) Interesting figure. It would be easier to interpret if the data points and inset boxplot were larger. For instance, I can't tell if there are any subsoil solution data points.

Good point! The autocorrelation of [DOC] among samples are excluded using Durbin-Watson test (details in our response to comment for L165-167). This explanation is added in Methods section.

Figure 3 is revised as below:

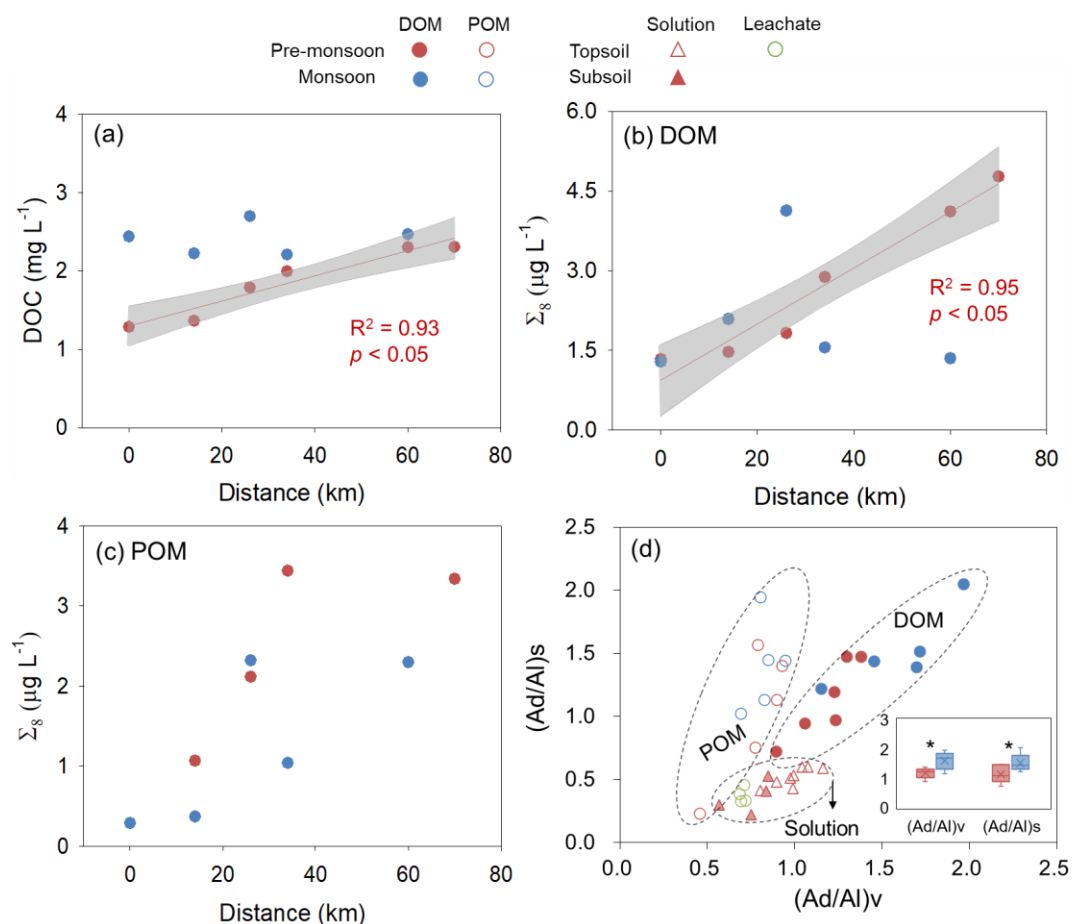


Figure 3. Variations of dissolved organic carbon (DOC) in Shaliu River water (a), absolute concentration of lignin phenols (Σ_8) in riverine dissolved organic matter (DOM; b) and particulate organic matter (POM; c) during the pre-monsoon and monsoon seasons in 2015, the acid-to-aldehyde (Ad/Al) ratios of syringyl (S) and vanillyl (V) phenols in the riverine DOM, POM, soil solutions and leachates (d). The abscissa in panel (a), (b) and (c) mean the distance of sampling sites from SLH-0. The red lines in panel (a) and (b) correspond to the linear regression of data ($p < 0.05$), and the grey shaded regions in panel (a) and (b) show 95% confidence intervals. The inserted box in (d) is the comparison of $(Ad/Al)_v$ and $(Ad/Al)_s$ ratios of dissolved lignin phenols between pre-monsoon and monsoon seasons, respectively, with asterisks indicating significant differences (independent sample t tests, $n = 5$, $p < 0.05$). The solid bar and cross in the inserted box mark the median and mean of each data set, respectively. The upper and lower ends of box denote the 0.25 and 0.75 percentiles, respectively.

Table S2: This table is as interesting and important as Table 1. It would be useful to include in the main text and also include DOC and POC concentrations, rather than their ratio.

This table is moved into the main text as Table 1 in our revised manuscript.

Figure S1: (a) Please indicate the sample size for each boxplot. Interesting that particulate concentrations are higher pre-monsoon, whereas dissolved concentrations are lower. Why? What does this say about hydrologic effects on C mobilization?

Sample size for each boxplot is added in the caption of Figure S2a.

The higher particulate carbon was directly related to the higher TSS concentrations in the pre-monsoon than monsoon season. Thermal erosion during thawing is the most important pathway supplying particulates and weathering products into the river on the

Qinghai-Tibetan Plateau, which may explain the high particulate concentration in pre-monsoon season. In contrast, other than aged DOC sourced from thawed soils, exudates from plant roots are also an important supply to riverine DOC. Although we did not measure root exudates, we postulate that the higher riverine DOC during the monsoon season is related to increased plant growth and exudation in the growing season. Relevant explanation is added in the Discussion section in *Lines 340-346*.

Response to Reviewer #2's comments

General comments:

The manuscript investigated the riverine carbon dynamics in an alpine headwater system on the Qinghai-Tibetan Plateau where is less monitored. Ideal methodologies were applied to reach the outlined objectives of this research initiative. The manuscript is generally well written, and the data is properly presented, it is well-suited for the journal *Biogeosciences*. However, there are some issues, listed below, should be considered.

Thank you for the positive assessment of our manuscript. We have revised our manuscript and hope that our responses (details below) have addressed all the comments.

Major comment:

The water sources of the headwater system could be very complicated in the permafrost-affected area. It could be the precipitation and also could be the soil pore water as the permafrost thaw. The inputs of those two water sources to the river change with time, and it caused inter-annual changes in the physicochemical characteristics. The manuscript focused on the carbon flux changes influenced by hydrological events, therefore I expect to see more discussion on the interaction effects. In mid-June, Fig.2 revealed the highest water discharge and the lowest DIC concentration throughout the year, however, the DOC concentration was always stable at around 3 mgL⁻¹. The author has the detailed freezing period and thawing period temperature (Fig. 1), and I think it might be used as a piece of strong evidence to describe the input of permafrost soil pore water. So I encourage the authors to discuss more on the fluctuation of DOC concentration with consideration of the hydrological conditions.

Good suggestion! Discussion on the variations of DIC and DOC concentrations with hydrological conditions is added in *Lines 315-319 and Lines 328-332* in main text and Supporting Information (Figure S4 below).

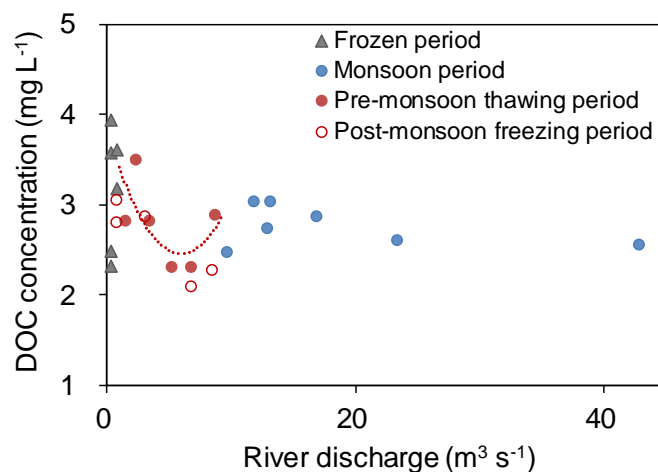


Figure S4. Relationships between intra-annual DOC concentrations and river discharge at

SLH-4 station in Shaliu River. The red line indicates the variation trend of DOC with discharge in pre-monsoon thawing period. Frozen, pre-monsoon thawing, monsoon and post-monsoon freezing periods are referred as December to March, April to June, July to September, October to November based on measured soil temperatures, respectively.

Specific comments:

- 1) Study area: The Shaliu River is about 110 km, however the plotting scale in the map revealed that the distance between SLH-0 and SLH-6 is less than 3 km. Is there a mistake of the plotting scale?

Thank you! It is a scaling mistake and is now revised in Figure 1a.

- 2) Sampling collection: Why do you choose May and August to represent for pre-monsoon season and monsoon season? Please add some description on the monsoon season.

The Shaliu River basin is under a continental monsoon climate characterized by warm, humid summer and cold, dry winter. Approximately 90% of the annual precipitation occurs between June and September (Figure S1; Zhang et al., 2013). Moreover, soil temperature is generally below 0 °C from middle October to late April (Figure 1b). Hence, we choose May and August to represent pre-monsoon and monsoon seasons, respectively. Related description is added in *Lines 96-97*.

References:

Zhang, F., Jin, Z. D., Li, F. C., Yu, J. M., and Xiao, J.: Controls on seasonal variations of silicate weathering and CO₂ consumption in two river catchments on the NE Tibetan Plateau, *J. Asian Earth Sci.*, 62, 547-560, <https://doi.org/10.1016/j.jseaes.2012.11.004>, 2013.

- 3) Line 225-235: Why the acid-to-aldehyde ratios of lignin phenols in topsoil are consistently higher than those in the subsoil in this region? Does that mean topsoil undergo higher degradation than subsoil?

Good point! The lignin phenol acid-to-aldehyde ratios (Ad/Al) normally increase with soil depth (Otto and Simpson, 2006). However, our previous research also find higher Ad/Al ratios in top- than subsoil on Qinghai-Tibetan Plateau due to the influence of dominant vegetation (i.e., shallow-rooted *K. humilis*) having high Ad/Al ratios in its roots (Jia et al., 2019). Here, the Shaliu River basin is dominated by shallow-rooted *K. humilis* as well (Li et al., 2013), likely leading to the higher Ad/Al ratios in topsoil. The above explanations are added at *Lines 353-358*.

References:

Otto, A. and Simpson, M. J.: Evaluation of CuO oxidation parameters for determining the source and stage of lignin degradation in soil, *Biogeochemistry*, 80, 121-142, <https://doi.org/10.1007/s10533-006-9014-x>, 2006.

Jia, J., Cao, Z. J., Liu, C. Z., Zhang, Z. H., Lin, L., Wang, Y. Y., Haghipour, N., Wacker, L., Bao, H. Y., Dittmar, T., Simpson, M. J., Yang, H., Crowther, T. W., Eglinton, T. I., He, J. S., and Feng, X. J.: Climate warming alters subsoil but not topsoil carbon dynamics in alpine grassland, *Glob. Change Biol.*, 25, 4383-4393, <https://doi.org/10.1111/gcb.14823>, 2019.

Li, C., Li, X., Yang, T., and Li, Y.: Structure and Species Diversity of Meadow Community along the Shaliu River in the Qinghai Lake Basin, *Arid Zone Research*, 30, 1028-1035, 2013.

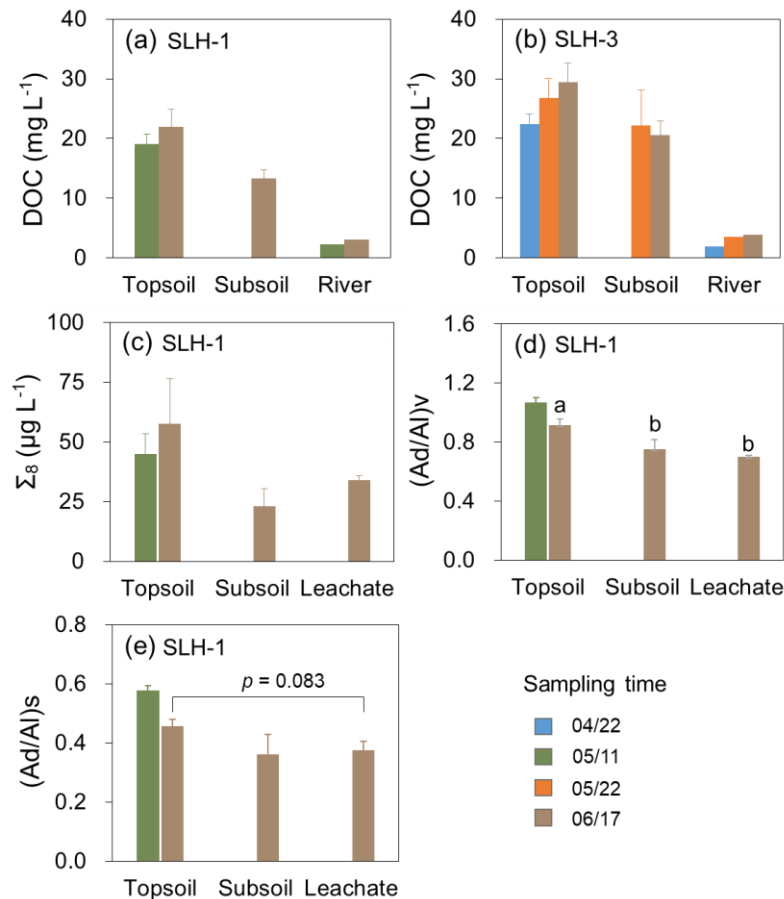
- 4) Line 251-254: the DOC and lignin phenols data in this sentence are VERY hard to compare, please reverse this sentence.

This sentence is re-written in *Lines 277-279*: “Subsoil-derived DOM was gradually released with thawing, indicated by the increase of DOC (or lignin phenol) concentration from not

detectable (frozen) on May 11 to 13.3 mg L^{-1} (lignin phenols = $23.1 \text{ } \mu\text{g L}^{-1}$) on June 17 at SLH-1 station and from not detectable on April 22 to 22.1 mg L^{-1} on May 22 at SLH-3 station (Figures 5a-c).”

5) Figure 5: I would recommend the author to change the legend into individual colors rather than gradients.

Figure 5 is revised as below:



6) Table 1: Have you collected the river discharge data during this precipitation event?

This is a good point. River discharge data can reflect the hydrology variations caused by precipitation events. However, it is difficult to obtain these data due to logistical reasons. Alternatively, we show the total suspended solid concentration which may partially indicate hydrology conditions due to its positive correlation with discharge (Meybeck et al., 2003).

Reference:

Meybeck, M., Laroche, L., Durr, H. H., and Syvitski, J. P. M.: Global variability of daily total suspended solids and their fluxes in rivers, *Global and Planetary Change*, 39, 65-93, 10.1016/s0921-8181(03)00018-3, 2003.