Response to Reviewers of bg-2020-472

General response to the Editor

Editor's comments:

Dear Xiaojuan Feng,

We are pleased to inform you that the open discussion of your following BG manuscript was closed:

[…]

No more referee comments and short comments will be accepted. Now the public discussion shall be completed as follows:

You - as the contact author - are requested to individually respond to all referee comments (RCs) by posting final author comments on behalf of all co-authors no later than 24 Feb 2021 (final response phase).

[…]

We sincerely thank the editor and reviewers for their supportive and stimulating comments. Following their suggestions, we have made important 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.

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 and 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 365-370* of the revised manuscript.

References:

Song, C., Wang, G., Mao, T., Chen, X., Huang, K., Sun, X., and Hu, Z.: Importance of active layer freeze-thaw cycles on the riverine dissolved carbon export on the Qinghai-Tibet Plateau permafrost region, PeerJ, 7, e7146, 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, 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, Journal of Hydrology, 550, 307-317, 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 52-81* as follows:

"... This region is covered by large areas of glaciers, permafrost, and seasonally frozen soils (not underlain by permafrost layers), which are strongly affected by thawing with increasing temperatures in the pre-monsoon spring. 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 (Song et al., 2019; Walvoord and Kurylyk, 2016). In addition, this region has a

continental monsoon climate and is hence significantly affected by intense precipitation in the summer monsoon (Zou et al., 2017) ...”

“Hydrological alterations induced by both thawing of frozen soils and summer monsoon likely result in unique seasonal patterns in riverine carbon dynamics in the Shaliu River compared to headwater streams in other regions including the Arctic or tropical rivers (Zhang et al., 2013). The sources of fluvially exported carbon, including dissolved organic carbon (DOC) and dissolved inorganic carbon (DIC), includes both recently fixed modern carbon from terrestrial plants and aged carbon preserved in frozen soils within this region. It is reported that more than 50% of the dissolved carbon (including DOC and DIC) in rivers on the Qinghai-Tibetan Plateau is from aged carbon sourced from the active layers of permafrost (Song et al., 2020), while precipitation induced surface runoff is known to enhance modern carbon export from the soil surface (Feng et al. 2015). However, the seasonal transport dynamics of these carbon pools along the Qinghai-Tibetan river continuum are relatively poorly investigated. In particular ...”

“... The characterization of lignin phenols, which are unique tracers of terrestrial plant derived OM (Hedges and Mann, 1979) and provide useful information on the oxidation stage of terrestrial OM (indicated by lignin acid-to-aldehyde ratios; Bianchi and Canuel, 2011; Hedges et al., 1988), allows us to investigate the sources and transport of terrestrial organic matter in rivers. Based on these investigations ...”

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 189-190* 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 ± 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 as below:

“Simultaneously, POC and PIC concentrations increased from 0.36 to 0.46 mg L^{-1} and from 0.03 to 0.06 mg L^{-1}, accompanied by an increase of TSS concentrations from 5.8 to 7.4 mg L^{-1}.”

“In addition, the concentrations of particulate carbon were higher in the pre-monsoon

than monsoon season, while dissolved carbon had higher concentrations in the monsoon season (Figure S1). In the pre-monsoon season, thawing of frozen soils potentially increased soil erosion (Olefeldt et al., 2016) likely contributing to the higher TSS and particulate carbon concentrations."

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 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 test 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 as:

"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 soil".

L61-62: ". . . annual fluvial carbon fluxes on a monthtly 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.

L62: "bulk" as in "bulk concentration"?

The "bulk" is revised as "bulk concentration".

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.

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

The annual discharge of 25.4 $m^3 s^{-1}$ (Wu et al., 2019) is added.

Reference:

Wu, H., Zhao, G., Li, X.-Y., Wang, Y., He, B., Jiang, Z., Zhang, S., and Sun, W.: Identifying water sources used by alpine riparian plants in a restoration zone on the Qinghai-Tibet Plateau: Evidence from stable isotopes, Science of the Total Environment, 697, 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.

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 118-120* as below:

"... using pre-arranged ceramic head (0.2-μm) which is a porewater sampling device composed of a porous ceramic head connected with a small diameter tube for pulling a vacuum and retrieving the sample."

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 as follows:

"... between riverine carbon concentration and the distance of sampling sites from SLH-0, where the autocorrelation of riverine carbon concentrations among samples was not significant based on Durbin-Watson test."

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 HCO_3^- + CO_3^{2-}, rather than CO_2.

Thank you! Relevant information is added in *Lines 295-296* as below:

"DIC, mainly composed of bicarbonates and carbonates based on the river water pH values (Table S2), was the dominant form of dissolved riverine carbon."

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

Revised.

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

This text is included in the Discussion section of revised manuscript.

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.

"The acid-to-aldehyde (Ad/Al) ratios of V and S phenols are used to indicate lignin oxidation (Opsahl and Benner, 1995) which typically increase with elevated degradation (Otto and Simpson, 2006) ..."

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

This text is included in the Discussion section.

L243-245: Another example of good Discussion material.

This text is included in the Discussion section.

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

This sentence is clarified as *"To reveal riverine carbon variations induced by water migration from soil to river 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 79-81* as below:

"Based on these investigations, we hypothesize that the release of carbon from frozen soils during thawing events leads to a downstream increase of riverine carbon in the pre-monsoon season while carbon inputs through surface runoff in short-term precipitation events may result in a fluctuation of riverine carbon."

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 as *"Soil DOC and lignin phenols showed an increasing (but not statistically significant) trend 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:

"The observed increase of soil DOC and lignin phenols in topsoil solutions over time reflects carbon release from previously frozen topsoil and/or inputs via lateral flow paths with thawing. This combined with the release of subsoil-derived DOM proved by subsoil DOC availability and lignin phenol Ad/Al ratios along with thawing progress indicate the release of frozen DOM during thawing events."

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 due to logistical reasons. However, 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 illustration is added as follows:

"In contrast, sporadic local precipitation events occur frequently during the monsoon season, leading to fast hydrological variations reflected by increasing TSS concentrations over time in the 1-h precipitation events (Beel et al., 2018). The rapid response of riverine DOC and POC to the above hydrological alterations indicates the high sensitivity of riverine carbon to short-term precipitation."

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 1.

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.

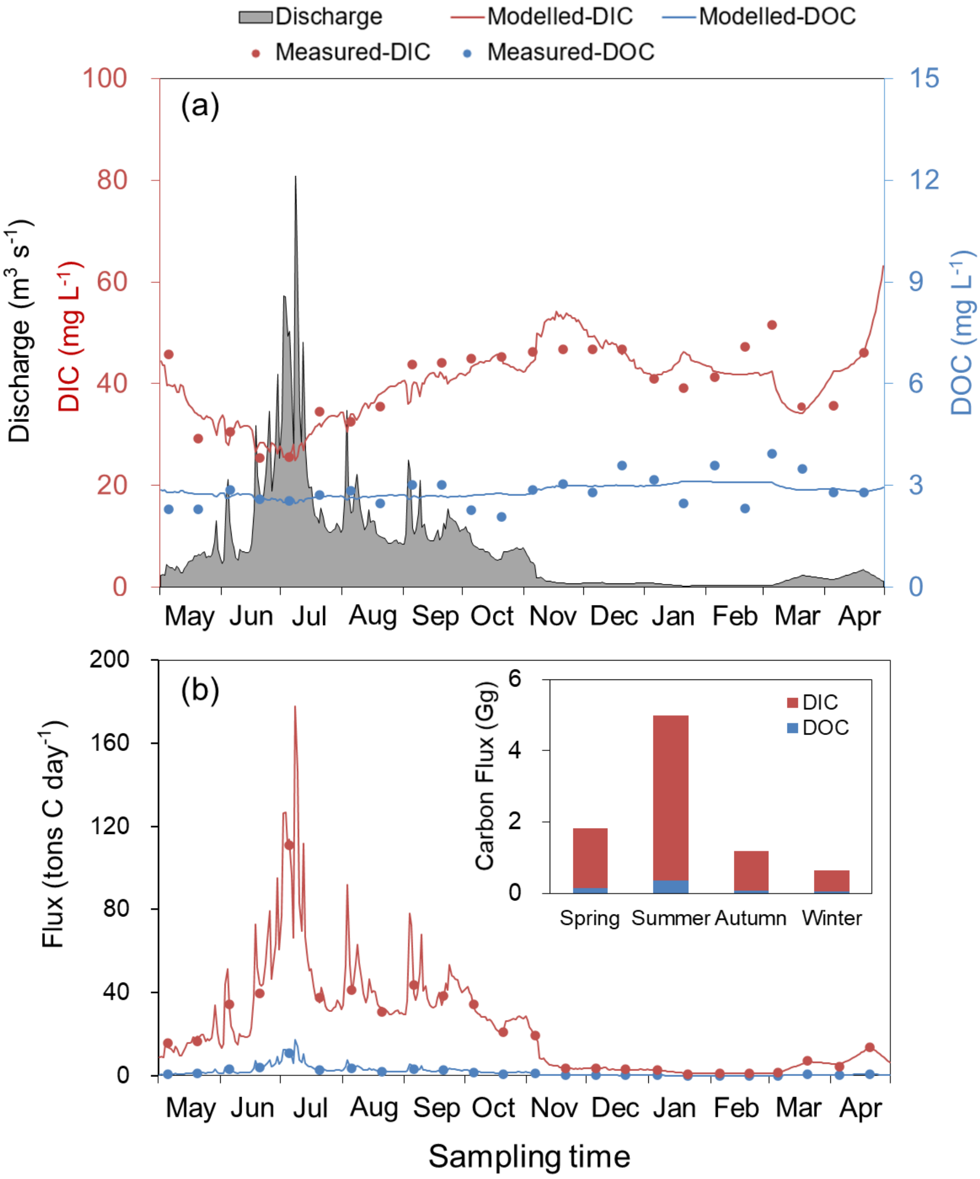


Figure 2. *Discharge, dissolved inorganic carbon (DIC) and dissolved organic carbon (DOC) concentrations (a) and fluxes (b) exported from Shaliu River at SLH-4 station during 2015 and 2016. The modelled concentrations in (a) and modelled fluxes in (b) are derived from from load estimator (LOADEST). The inserted columns in panel (b) showed 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:

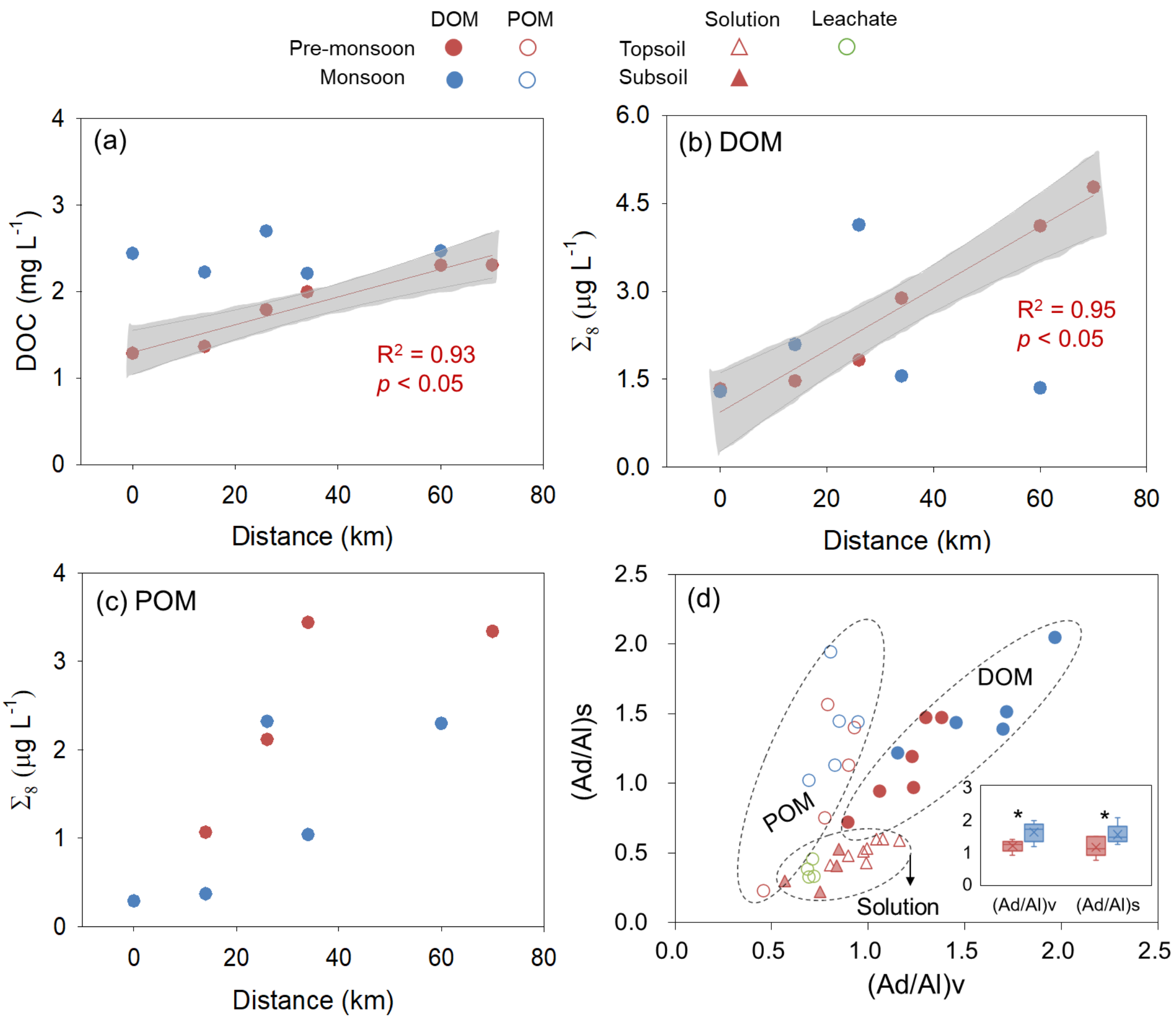


Figure 3. Variations of dissolved organic carbon (DOC) in Shaliu River water (a), absolute concentration of lignin phenols (Σ_8) in riverine dissolved organic matter (DOM; b) and particulate organic matter (POM; c) during the pre-monsoon and monsoon seasons in 2015, the acid-to-aldehyde (Ad/Al) ratios 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. NA, not analyzed.

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 revise 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 S1a.

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 increased 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.

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 descript 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 main text and Supporting Information as follows:

"… The negative correlation of DIC concentration with river discharge at SLH-4 station (Figure 2a) indicates interactive impacts on hydrology by precipitation and thawing. Specifically, the inputs of subsurface flow and groundwater containing high weathering products (including DIC) under base flow conditions (including during thawing) in this permafrost-affected watershed (Walvoord and Striegl, 2007; Giesler et al., 2014) results in relatively high DIC concentraions, while the dilution of rainwater leads to a significant decrease of DIC concentration in the monsoon season."

"Furthermore, DOC concentraions showed no consistent relationship with river discharge throughout the year, while it shows opposite relationships with river discharge in early versus late pre-monsoon seasons (Figure S3). Snowmelt in the early pre-monsoon season (early April) plays an dilution effect on the base-flow DOC, resulting in an

decreasing trend with discharge. By comparison, thawing of frozen soils in late pre-monsoon season (late April to June) releases more frozen carbon (details below) and thus results in an increasing trend of DOC with discharge."

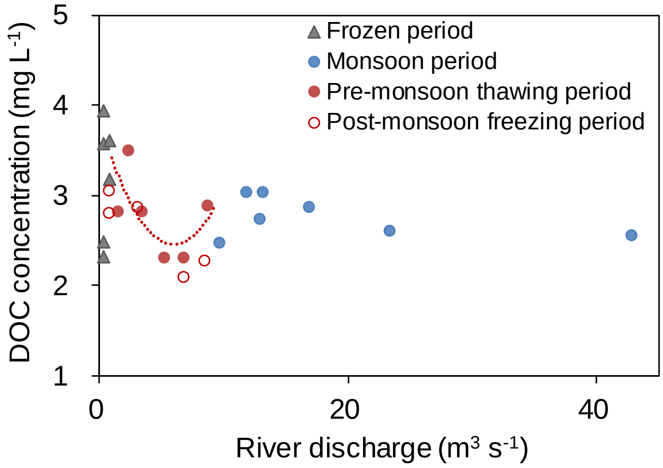


Figure S3. *Relationships between intra-annual DOC concentrations and river discharge at SLH-4 station in Shaliu River. The red line indicates the variation trend of DOC with discharge in pre-monsoon thawing period. Frozen, pre-monsoon thawing, monsoon and post-monsoon freezing periods are referred as 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.

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 (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 as follows:

"The Shaliu River basin is under a continental monsoon climate characterized by warm, humid summers and cold, dry winters (Wang et al., 2018). The mean annual temperature is –0.5°C within the basin (Li et al., 2013) and mean annual precipitation is 370 mm, ~90% of which occurs in the monsoon season (June to September; Zhang et al., 2013; Wu et al., 2019)."

References:

Zhang, F., Jin, Z., Li, F., Yu, J., and Xiao, J.: Controls on seasonal variations of silicate weathering and CO_2 consumption in, two river catchments on the NE Tibetan Plateau, Journal of Asian Earth Sciences, 62, 547-560, 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 345-349*.

"In addition, the acid-to-aldehyde (Ad/Al) ratios of lignin phenols in topsoil were higher than those in the subsoil. The Ad/Al ratios normally increase with soil depth (Otto and Simpson, 2006). However, our previous research find higher Ad/Al ratios in top- than subsoil in an alpine grassland on the 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."

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, 10.1007/s10533-006-9014-x, 2006.

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

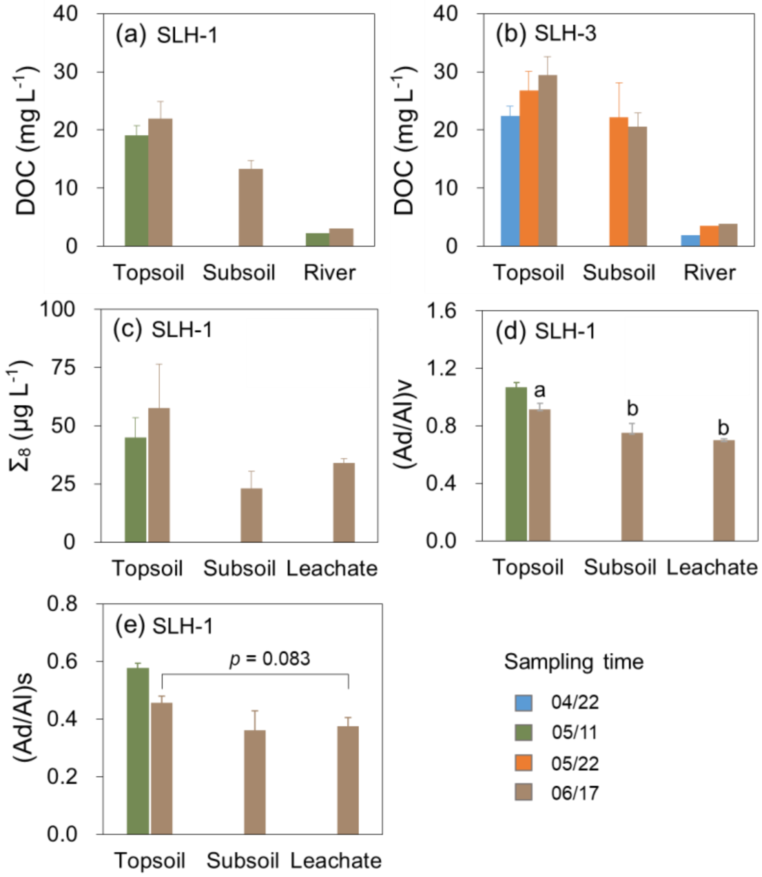
Li, C., Li, X., Yang, T., and Li, Y.: Structure and species diversity of meadow community along the Shaliu River in the Qinghai Lake Basin, Arid Zone Research, 30, 1028-1035, 2013.

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: *"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 μg 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 (Figure 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.