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Biogeosciences Discussions

Interactive comment on "Spatial-temporal variations in riverine carbon strongly influenced by local hydrological events in an alpine headwater stream" *by* Xin Wang et al.

Anonymous Referee #1

Received and published: 20 January 2021

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.

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.

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 > 0ËŽC, 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.

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.

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.

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.

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.

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.

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 0ËŽC 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 ev-

idence 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.)

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

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

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

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.”

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

L63: “dense” = high temporal resolution?

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

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.

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

L165-167: Does the statistical analysis for downstream trend in [DOC] account for

autocorrelation among samples? (see also comment for Figure 3)

L175: Based on your pH values reported in Table S2, it might be worth noting here that DIC was primarily HCO3– + CO32–, rather than CO2.

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

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

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).

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

L243-245: Another example of good Discussion material.

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

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.

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

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?

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.

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.

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

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).

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.

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.

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?

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