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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hydrological processes and permafrost regulate magnitude, source and chemical characteristics of dissolved organic carbon export in a peatland catchment of northeastern China" *by* Yuedong Guo et al.

Anonymous Referee #3

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Guo et al utilize a data set of stream discharge, carbon concentration and quality, and various other ancillary pieces of information from near stream catchment peatlands to identify relationships between discharge and DOC in a catchment underlain by continuous permafrost in northern China. The discharge and DOC data set is robust, indicating a direct correlation between these two variables. They argue that the main source of the DOC is peatland soils and that the forested hillslopes do not contribute to the signal. Three measures of carbon quality are used to support their claim and to show the similarity between stream and soil chemistry. No chemical or physical data is

presented for the forested parts of the catchment.

DOC is difficult to measure during storms, and the authors have done a great job of capturing the rise and recession of several storms - this in itself is a great contribution. Generally, there is more that could be done with the available data to support the authors' claims and investigate the relationship between discharge and DOC. For instance, plotting discharge vs. DOC could help understand the relationship between these two and aid in considering a wetter future. Breaking the data set up by years could also be useful, given the large differences in precipitation between the years. There is lots of speculation about the source of the carbon and the flowpaths between the catchment and the stream. I think this speculation significantly detracts from the paper. The authors' analysis relies heavily on several references from other systems, which may or may not be representative of conditions in their system. The authors do a fair job of addressing their second research question regarding the relationship between runoff processes and DOC, but do not – and probably are not capable of rigorously answering their third question regarding the effect of permafrost degradation and climate change given the data set.

Major Comments:

Connectivity vs variable source areas – what is the ultimate cause of the observed trends between discharge and DOC and the fluorescence indices? – the authors argue that the thaw depth controls DOC export concentration and quality, and make some assumptions about contributing areas and catchment connectivity, but with minimal support beyond a few references from other well-known catchments studies. The authors do not have any data from the forested hillslopes, which is an important endmember necessary to substantiate their claims. Especially the paragraph from 428 – 450 appears highly speculative

Generally, I found the paper to be very light on analysis - a major finding is that DOC is positively correlated to discharge. It would be nice to see this relationship plotted.

Is it linear? Non-linear? Showing this would add further support to the author's claim that this system is transport-limited and that increased rainfall will lead to increased C export.

Consistency in terms – at line 316 – "hydrological DOC, Q, conductivity, and turbidity', earlier at line 288, "discharge turbidity" and "discharge conductivity"

Unclear interpretation of FI index – FI varies in the soils from 1.3 to 1.55 and in the stream from 1.43 to 1.62. The authors assume that the range indicates ". . .both terrestrial and microbial sources" (line 320), and cite Cory's 2010 paper which focuses on correcting fluorescence spectra from different instruments. This is not an appropriate reference to support the authors' interpretation. McKnight's 2001 publication in which microbial and terrestrial end members is defined would be a better choice, but still it would be useful to present more rationale for the authors' interpretation, and to address alternate hypotheses to explain the differences – like the influence of distal water sources.

Stable isotopes – the authors argue that stable isotopes indicate that peat porewaters, rather than direct rainfall or mineral soils are the source of runoff. But they have not measured isotopes from the mineral soils beneath the peats, or from the more distal parts of the catchment (ie. the hillslopes), and thus the argument is weak. Furthermore, it is unclear from Figure 6 whether they've collected enough samples to see changes to the isotopic composition for individual storms, which limits the potential inference.

Unclear how total DOC export magnitudes were estimated – there are no methods regarding the calculations used.

Minor Comments:

56 – missing some major references regarding the effects of permafrost thaw on hydrology, for instance Hinzman et al., 2005, Jorgenson et al., 2006. . ..

101 – 103: How do you define 'satisfactory'? Many studies have focused on the fate of

permafrost carbon – Drake et al., 2015 is a good example and much of the BDOC loss methods by Wickland and others. Spencer et al., 2015 provides a nice conceptualization of the fate of permafrost carbon.

128 – This is very broad question involving multiple disciplines and I'm not convinced that your data set is nearly enough to address this. I would suggest removing this question all together.

161: What is 'yang'?

177: How soon is '. . .as soon as possible.'? Hours? Days? Weeks? This matters especially when you're talking about DOC. Were samples stored in a cool, dark place before analysis?

183 and 190 – sensors are not consistently or properly referenced (ie. Campbell, USA and YSI6600, USA are both incomplete)

203: Missing a verb – maybe 'collected'?

271: This sentence is confusing. Do you mean that there was no standing water in the peat?

288: I think you can remove the word 'discharge' and just say 'turbidity'. Similarly, 'electrical conductivity' is clearer than 'discharge conductivity'.

319: Is it reasonable to assume that the FI range will be similar in your system to those studied by Cory?

347: If they were not statistically different, wouldn't the p value by larger than the 0.01 test threshold?

389: This sentence is not clear, and not totally true

397: This statement may be generally true, but not always. Organic soil macropores may not exist everywhere. Do they exist in the Fukuqi catchment? The high hydraulic

conductivity and porosity of shallow soils relative to deeper soils also plays an important role.

407: This may be evidence, I don't think that it's necessarily proof.

416: What do you mean by 'fundamental condition'?

418 – 420: I do not believe that subsurface flow "guarantees" that water closest to the stream will always reach the stream first. When subsurface conditions are homogeneous, this may be true, however soil pipes in organic soils (Carey and Woo 2001) and mineral soils (Koch et al., 2013), tussocks (Quinton et al., 2000), and hetereogeneity in subsurface soils (Koch et al. 2017; Laine-Kaulio 2014 and 2015) may complicate this and lead to preferential areas of flow, allowing some areas further from the stream to contribute faster and more than areas near the stream.

420: I don't quite follow this sentence. Maybe break it down into a few sentences? Also, the positive correlation between Q and DOC is likely a result of more dynamics than simply the proximity of organic-rich soils, it also implies that the source is large, and that the presence in the stream is transport-limited. This point seems important to the story you're telling. . .

428: Spence and Woo 2006 and Spence and Phillips 2015 both support this point and provide useful precedent.

431: "Geomorphic landscape structures" is kind of vague.

436: This sentence is unclear – if the peatland is highly conductive, shouldn't it facilitate movement of water from the hills?

441: I don't think you can assume the values of the hillslopes. I would not expect them to look more like rain than the peatland soil porewaters.

484: Where is the evidence that these don't generally change with DOC concentration?

451: I believe that your data very much supports an allochthonous DOC source –

autochthonous DOC would result from in-stream processes like the degradation of photosynthetic cells. Variations in contributing area also likely play an important role (see spence).

505 – 510: Koch et al. 2014 found that stream chemistry changed much earlier, around mid-June concurrent with the beginning of thawing of the mineral soils. Based on your depth to ice measurements and statement that the organic-mineral boundary is near 30 – 40 cm, it seems like you should also start seeing this response somewhere in June. Autumn sounds too late – by this time you've reached maximum thaw and in fact may be beginning to freeze again.

536: I don't understand the logic here. . ..how can you suggest that HIX values are not sensitive to soil active layer depths when you show substantial variations in HIX with soil depth (Figure 8)?

555: There are two assumptions here and I'm not sure if either is reasonable: 1. Is it reasonable to assume that export is proportional to concentration for both forest and peatland systems? Forest and peatland carbon is fairly different, and I imagine could have differing levels of leachability and solubility, and thus transport potential. And I don't believe that you've discussed forests at all before this point. 2. This seems to ignore your previous claims that only the peatland contributes to the stream DOC pool.

587 – But at the same time temperatures are likely to warm, impacting overall carbon stocks and DOC production. So there are lots of variables that will likely affect the active carbon pool.

Figure 1 needs lat/longs

Fig 2 – Date format is difficult to read and in strange increments. What does 'standing water level' mean? Is this level and the thaw depth from one point? How representative is this point? Is this point shown in Figure 1?

Fig 6 – It would be nice to also have discharge on this plot to see how stream water

isotopes relate to discharge.

Fig 7 – Probably don't need negatives on the y axis – What would a negative soil depth mean? Why not set up this plot like those in Figure 8? It would make it easier to compare the seasonal trends.

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