Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-412-AC3, 2017

Hydrology and Earth System Sciences
Open Access
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.

Yuedong Guo et al.

guoyuedong@neigae.ac.cn

Received and published: 31 October 2017

The comments from reviewer 3:

Comment 1. 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.

Response: Thanks for the comment! First, the relationship between discharge and DOC concentration was plotted in the revise paper. The data set was broken into three years for detailed analysis. Second, the discussion about the flowpaths between and stream was somewhat redundant, and the content was largely cut down in the revised paper. Third, it was indeed no sufficient data to support the third question in the study, and hence the third question was deleted in the context of "Introduction". Therefore, the forecast of DOC loads under changing climate is only a part of auxiliary content of discussions. Finally, several important references and conclusions from similar catchments were cited to give more detailed discussions in the revised paper.

Comment 2. 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.

Response: Thanks for the comment! Indeed, we have not measured the DOC from the upland mountains. Hence, the conjecture bout the hydrological connectivity and DOC source from the upland were deleted in the revised paper according to the comment. The related content may be investigated in the future.

Comment 3: 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. Response: Thanks for the comment! The relationship between discharge and DOC concentration was plotted for each year in the revise paper (Fig. 4, Page 43). There were significant linear relationships.

Comment 4: Consistency in terms – at line 316 – "hydrological DOC, Q, conductivity, and turbidity', earlier at line 288, "discharge turbidity" and "discharge conductivity". Response: Thanks for the comment! The terms were modified in the line 282.

Comment 5: 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.

Response: Thanks for the comment! The reference of McKnight (2001) was well studied and cited in the revised paper in line 213.

Comment 6: 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.

Response: Thanks for the comment! First, the isotope data of soil pore water were from the whole active layer which include the lower mineral soil in summer. Meanwhile, the isotope data from hillslopes in 2013 were added in Fig. xx in the revised paper according the comment. Second, the DOC loads were re-estimated by a new method according to the suggestion from the first reviewer. The DOC load was re-calculated by the program LOADEST with the web-based calculation program (https://engineering.purdue.edu/mapsever/ldc/LOADEST, version 2012). The new DOC yield estimated by the program is 4.7 g m-2 yr-1. The new result will used in the revised paper (In lines 218-232, line 302).

Comment 7: (miner) 56 – missing some major references regarding the effects of permafrost thaw on hydrology, for instance Hinzman et al., 2005, Jorgenson et al., 2006… Response: Thanks very much for the reference list provided for me! Some important references were collected and cited in the revised paper.

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.

Response: Thanks for the references. There is indeed some studies on the fate of permafrost carbon including DOC. The sentence was modified in the line 79-84. The reference of Spencer et al., 2015 was added in line 81.

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

Printer-friendly version

Discussion paper

question all together. Response: The question was removed in the revised paper.

161: What is ‘yang’? Response: It was replaced by “young”.

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? Response: The detailed procedures to storing water samples was added in lines 151.

183 and 190 – sensors are not consistently or properly referenced (ie. Campbell, USA and YSI6600, USA are both incomplete) Response: The detailed information for the instruments were added in lines176 and 184.

203: Missing a verb – maybe ‘collected’? Response: The verb was added: “rainfall samples were collected during the two growing seasons.”

271: This sentence is confusing. Do you mean that there was no standing water in the peat? Response: The sentence was re-written: “No water level higher than peat surface were detected for the three years.” Line 264 in the revised paper.

288: I think you can remove the word ‘discharge’ and just say ‘turbidity’. Similarly, ‘electrical conductivity’ is clearer than ‘discharge conductivity’. Response: The word “discharge” was removed. (Line 282 in the revised paper)

319: Is it reasonable to assume that the FI range will be similar in your system to those studied by Cory? Response: The reference was replaced by McKnight (2001) according to the comment.

347: If they were not statistically different, wouldn’t the p value by larger than the 0.01 test threshold? Response: The p values should be larger than 0.01. The error was modified in the paper. (Line 354 in the revised paper)

389: This sentence is not clear, and not totally true. Response: The whole sentence was removed in the revised paper.

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. Response: Thanks for the comment! The information about the porosity of the upper organic soil is listed in line 125 in the revised paper. The higher hydraulic conductivity of shallow organic soil compared to lower mineral soil is discussed in Section 4.3 in line 493-504 in the revised paper.

407: This may be evidence, I don't think that it's necessarily proof. Response: The sentence was removed in the revised paper.

416: What do you mean by 'fundamental condition'? Response: I have meant that lateral subsurface flow was an important condition of the positive relationship. The word "fundamental" was replaced by "important" in the revised paper.

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. Response: It is true that high soil hetereogeneity may lead to preferential flow in some region. But in fact, the miner soil under the peat was very uniform without large soil pipes. The discussion is only a conjecture and not an important content in our study. The related sentences were re-written in lines 493-496.

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. Response: Thanks for the comment! The sentences were re-written in the revise paper. The idea of "transport-limited" is important for the study. The conclusion was expressed in the Section 4.2 in lines 447-456 in the revised paper.

428: Spence and Woo 2006 and Spence and Phillips 2015 both support this point and provide useful precedent. Response: Thanks for the comment! However, the discussion about the hydrological connectivity is deleted in the revised paper due to the lacking of convincing support by enough field data. 431: "Geomorphic landscape structures" is kind of vague. Response: The sentence was removed.

436: This sentence is unclear – if the peatland is highly conductive, shouldn't it facilitate movement of water from the hills? Response: The discussion about the hydrological connectivity is deleted in the revised paper due to the lacking of convincing support by enough field data.

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. Response: Thanks for the comment. The discussion about the hydrological connectivity is deleted according the comment.

484: Where is the evidence that these don't generally change with DOC concentration? Response: This is not an appropriate opinion, and the sentence was re-written in lines 486-489 in the revised paper.

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. Response: Two words "allochthonous" and "allochthonous" were removed to avoid misunderstanding.

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.

Response: The data around the middle June was considered when thaw depth reaching the mineral soil. In 2014, we found the FI and BIX values increase after June without the disturbance of rainfalls. The trend was discussed in lines 513-525 in the revised paper. However, no exact beginning point can be identified in our study.

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)? Response: The conclusion was incorrect and was removed in the revised paper.

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. Response: Thanks for the comment! The estimation for DOC export from peatland was removed as having no enough data to support the assumptions.

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. Response: It is really difficult to forecast DOC export in the conditions of temperature rise and rainfall change. However, there is the largest possibility that DOC export increases with rising rainfall, as the DOC export process is "transport-limited" but not "source-limited".

Figure 1 needs lat/longs Response: The information was added in Fig. 1. (Page 42)

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? Response: The date format was modified. The “standing water level” was changed into “Water level”. The information about the water level gauging point was in the section 2.2. “Sampling and monitoring program”. The information was also added in Fig. 1 in the revised paper.

Fig 6 – It would be nice to also have discharge on this plot to see how stream water isotopes relate to discharge. Response: The discharge data was added to the plot in Fig. 7 in the revised paper (Page 48).

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. Response: The soil depth was modified to positive value in the revise paper (Page 49, 50). However, if put the data into one figure like Fig. 8, it is too complex to identify the lines.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-412/hess-2017-412-AC3-supplement.zip

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-412, 2017.