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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 #1

Received and published: 14 August 2017

General comments: This paper summarizes a study of dissolved carbon dynamics in a peatland catchment in northeastern China underlain by continuous permafrost. Given that there are very few (if any) studies of C dynamics in this region, this work could be a worthwhile addition to the literature. Most of the studies to date have focused on far north latitudes, including Siberia, Alaska, and northern Scandinavia. This study can further the expansion of the world C database, and can only help aid in regional and global upscaling of C exports and yields. However, I have specific comments below for which clarification is advised. And while I applaud the authors' efforts to write in a non-

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native language, I feel this article would benefit greatly from an English language edit. Once these items are addressed, I believe this article could be suitable for publication.

Specific comments: How did the authors calculate the DOC yield (4.87 g m-2 yr-1)? Were DOC concentrations and Q modeled in a program such as LOADEST to determine total export, or did the authors sum DOC*Q for all of the time points?

From the stated 4.87 g m-2 yr-1 yield, an extrapolation is made to a peat export of 12.89 g m-2 yr-1, but this is based on a faulty assumption that export is proportional to the C pool. This is not a valid assumption, as C is exported only if there is water flowing through the pool. While the authors state that this assumption leads to an overestimation, the peat export is extrapolated further to suggest that DOC export is 72.8% of NEE. I believe there is too much extrapolation based on that faulty assumption.

A major conclusion of the paper is that DOC concentration is highly correlated with discharge, and thus, DOC export is driven by discharge. While the relation was stated as highly significant, I would like to see a plot of DOC concentration vs. Q.

In the discussion, the authors first state that high infiltration rates prevent overland flow (lines 402-406). Later, the authors suggest that in spring when the upper soil is still frozen, overland flow through litter could be another DOC source. Please re-word to reconcile these statements. I am not sure I agree with the authors' definitions of "autochthonous" and "allochthonous." I think in most of the literature, autochthonous refers to DOC generated within the stream, while allochthonous refers to DOC generated outside of the stream. I'm guessing the authors have defined riparian peat as part of the stream, and hence by that definition they would be an autochthonous source. But by the definition in other studies, riparian peat would be a terrestrial source and hence allochthonous. And by this definition, the findings in this study would not be contrary to the view that the source of DOC from headwater catchments is allochthonous. So, I would just be careful of your definitions, or state them more clearly.

Sampling was conducted during the "growing season," May to September. There is no

discussion regarding the condition of the river during the rest of the year. Is it frozen the remainder of the year? Is there under ice groundwater flow? Section 2 refers to lost water quality data due to “excessively low temperatures in the stream.” Did the sonde freeze in ice? How much data was lost?

The authors mention that peatland is distributed along both sides of the stream channel, and refer to Fig 1. The legend for Fig 1 refers to wetlands. Are the authors equating peatland and wetland? Also, as both upland and lowland are referred to in the text, I would like to know the elevation range in the catchment.

The authors say that permafrost blocks the input path of shallow groundwater from upland areas. Some mention of the difference in the depth to permafrost between upland and lowland areas would be good.

While the authors state their DOC yield (4.87 g m-2 yr-1) in the context of a range of values found in the literature (1-35 g m-2 yr-1), I would like to see more discussion of the difference in yields found between Eurasian studies and those of more northern latitudes. Is it typical for Eurasian catchments to be on the low end of the world average? And I suspect that there are more recent references with DOC yields than 2010.

I would like to see error bars on Fig. 8, so that it is easier to tell if there is any real seasonal difference in the spectral profiles.

Specific corrections: Check all units for superscripts and SI units in both text and figures. In some instances I see mg/L, in others mg L-1, etc.

Be consistent with “indexes” and “indices.”

How frequently were water level and flow velocity measured?

How frequently were temperature, conductivity, and turbidity measured?

For 18O analysis, please include a method reference.

Samples were collected from the “surface, middle, and bottom layers” of the profile.

What are the depths of these layers?

For DOC analysis, please include a method reference.

DOC analysis was done “as soon as possible.” Within what time were they analyzed? Were they stored chilled before analysis?

The results refer to total soil organic matter, total nitrogen content, and soil bulk density, but there is no mention of these analyses in the methods section.

Line 67-68: I believe the Striegl reference is 2005, not 2007

Line 117: is there a reference for long term temperature rise in this area?

Line 171: How do you define a “flood period?”

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