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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.

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Comment 1: 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? Response: Thanks for the comment! LOADEST program is a good tool to estimate total DOC yield. In the revised paper, the DOC yield 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, which

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is very close to the previous result. The previous result, 4.87 g m-2 yr-1, is obtained by multiplying seasonal mean DOC concentration by total discharge. The new result will used in the revised paper (Lines 219-232; Line 302).

Comment 2. 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 waterflowing 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. Response: Thanks for the comment! Indeed, the data of 72.8% is indeed overestimated. The content about the data is deleted in the revised paper, and the discussions about the importance of DOC yield for the peat carbon pool is re-organized (Lines 427-435).

Comment 3: 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. Response: Thanks for the comment! A new figure (Figure 4) showing the relationship was added in the revise paper. In the new figure, there exhibits a significantly positive relationship between DOC concentration and log10 (Q). (Page 45) Comment 4. 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. Response: Thanks for the comment! This part was deleted in the revised paper because it is only a conjecture without support of field data. Meanwhile, the discussion section was largely re-written, and the part in the new paper is not necessary.

Comment 5. 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 gener-

ated 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. Response: Thanks for the comment! In the revised paper, the expressions of "autochthonous" and "allochthonous" was deleted to avoid confusion. The discussion about DOC origin is re-organized in the revised paper (Lines 467-480).

Comment 6. 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? Response: Thanks for the comments. The information about the rest of the year was replenished in the section "2.1. Study area". Low temperature had led to the power loss of the buttery in the sonde. There was no ice forming during the growing seasons. Totally, about one fifth of the water quality data was lost mostly in early May in 2012 and 2013. (Lines 153-154 in the revise paper).

Comment 7: 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. Response: In the revised paper, the legend has changed into "Peatland" in figure 1. The elevation range of upland and lowland was added in the section "2.1. Study area". The description about the landform of the whole catchment was replenished in the section in lines119-122 in the revise paper.

Comment 8: 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. Response: The maximum thaw depth of the upland ranges from 80 to 100cm, which is slightly deeper than that in the peatland. This information about the thawing depth of upland was added in the revised paper in lines 138-139. However, the content about hydrological connectivity between mountain and peatland river is deleted due to lacking of the support from field data according to the comment from another reviewer.

Comment 9: 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. Response: The references about the DOC yield from Eurasian and other northern sites were re-concluded in the revised paper. New references after 2010 were included and typical examples from boreal land was added in the revised paper in lines 412-420.

Comment 10: 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. Response: The error bars was added in Fig. 9 (Fig. 8 in original paper) in the revised paper (Page 50).

Comment 11: 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.” Response: The errors in the units and other language descriptions have been all modified in the revised paper.

Comment 12: How frequently were water level and flow velocity measured? How frequently were temperature, conductivity, and turbidity measured? Response: All the data are set to be measured once every six hours. This information has added to the

revised paper (Lines185).

Comment 13: Samples were collected from the “surface, middle, and bottom layers” What are the depths of these layers? Response: Thanks for the comment! As the water level in the gauging profile of the stream fluctuated with time, so no exact depths was recorded when collecting water samples. There must be some mistakes in my description about water sampling, and the sample process was re-written in the revised paper in lines148-150.

Comment 14: For DOC analysis, please include a method reference. Response: Thanks for the comment. A reference on the DOC measurement with the same DOC analyser was added in the revised paper (Line 153).

Comment 15: DOC analysis was done “as soon as possible.” Within what time were they analyzed? Were they stored chilled before analysis? Response: Thanks for the comment! The information on how to store the samples was added in the revised paper in lines151-152.

Comment 16: 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. Response: The related content was deleted in the revise paper, because the analysis about the relationship between soil DOC and soil features is no use to explain the DOC dynamics in the stream.

Comment 17: Line 67-68: I believe the Striegl reference is 2005, not 2007 Response: Thanks for the comment! The information was modified in the revised paper in line 61.

Comment 18: Line 117: is there a reference for long term temperature rise in this area? Response: The sentence was revised and a reference was added in the line 97.

Comment 19: Line 171: How do you define a “flood period?” Response: After rainfalls, the water level in the stream profile would rise and go down. A flood period just means a flood event. The “flood period” maybe not clear, it was replaced by “flood events”.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-412/hess-2017-412-AC1-supplement.zip

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