

Review of “Hydrological processes and permafrost regulate magnitude, source and chemical characteristics of dissolved organic carbon export in a peatland catchment of northeastern China”, by Guo et al.

Main points:

The manuscript presents results from a catchment study on DOC export, carried out in a permafrost affected catchment in northern China. The authors monitored magnitude, timing, and chemical composition of DOC over three years. The topic is of current interest, as understanding impacts of climate change on DOC export in northern catchments can have potential important implications for downstream carbon cycling and landscape carbon cycling. Furthermore, as far as I know this is one of the first studies on this topic from eastern Asia, as most current research has been carried out in Scandinavia or North America, and thus is of particular interest. Three years of data, from highly different hydrological years, also increase the interest of the data-set.

My main comments on the manuscript are A) that I find that there is substantial room for improvement on the writing style, B) that the data analysis can be improved, particularly with regards to including uncertainty estimates and exploring influences of different seasons/years, and C) that I find that the authors are not sufficiently highlighting what I find to be the more interesting results.

I find that the authors use vague terms throughout the manuscript. For example, the authors talk about “runoff processes” and “hydrological processes” as the main controls on DOC export, but never clearly define what processes in particular they are referring to. For example, in several places I think it could be better to talk about “shifting flow-paths”, rather than use the more general term “hydrological processes”.

The writing style is generally indirect, unnecessarily wordy, and with main points found at the end of paragraphs or sections. Often I found this made it hard to follow the logic of a paragraph until I had arrived at the main point. I recommend writing using an active voice, and to adopt a style where the main point of a section or a paragraph is indicated early rather than at the end (i.e. point first paragraphs). I also found that many sentences can be re-written much shorter without losing the main message.

I find that the data analysis and reporting could be improved. Throughout the manuscript I find that the authors are reporting data with too high precision, using too many significant digits. At the same time, none of the estimates e.g. of DOC export, have uncertainty bounds. The methodology for estimating DOC export is never explained. There are many methods for estimating loads, and many of them will also yield uncertainty bounds. I also think there is a missed opportunity in the data analysis, to run an analysis of covariance (ANCOVA) to see if relationships between discharge and the several DOC characteristics (concentration and fluorescence indices) are statistically different for different seasons or years. If the relationship between discharge and DOC characteristics is dependent on e.g. active layer

depth (season), that would be really interesting. I think there are some general qualitative comments along these lines, and it would be very good to show this explicitly.

I find that the authors are emphasizing results/conclusions that are already well established, while perhaps missing an opportunity to highlight some of the findings that I consider more novel. The novel finding to me include the fact that these wetlands do not seem to behave as boreal peatlands in Scandinavia or North America, and thus that we could expect a different response to permafrost thaw at the study catchment. In peatland catchments in boreal Scandinavia or North America, we usually observe that DOC concentrations decrease during periods of higher discharge – a dilution effect. At the studied catchment, the authors report the opposite pattern. As such the studied peatland catchment act much more as an upland catchment where shifts in DOC concentrations and its chemical characteristics is controlled by riparian hydrology, and the shifts in flowpaths from organic surface soils during high flow periods and deeper mineral soil flow-paths during low flow periods. This seems to follow from the shallow peat layer at the studied catchment – and the study nicely shows that there are important shifts in DOC characteristics at the interface between the organic and mineral soil layers. With a deeper active layer, which develops earlier in the season, this would mean that we can expect an increased importance of deeper flow-paths through mineral soils – with associated shifts in DOC export. This to me represents a novel finding, and it contrasts very nicely with results from other regions.

Specific points:

L20. Specify whether you mean organic matter degradation or permafrost degradation.

L26. Export at $\sim 4.5 \text{ g C m}^{-2} \text{ yr}^{-1}$ is hardly “strong potential” – it is relatively moderate catchment DOC exports.

L49. This sentence: “However, uncertainties remain regarding to main driving factors involved and the fate of DOC due to complex interactions between hydrological and thermal dynamics and bio-chemical drivers.” – what is meant by thermal dynamics and bio-chemical drivers in this context? I think this is a good example of where the authors are vague, and where more specific examples would help convey their message better.

L84. Another example: “However, uncertainties remain in predicting DOC export processes based on changing hydrological processes.” – This is vague since I don’t know what you refer to when you say “hydrological processes”.

L90. DOC export is not only a function of runoff (if that is what is meant by with hydrological regime”). Catchment land-cover, particularly peatland land cover, has repeatedly been shown to influence catchment DOC export.

L97. Replace “can stably” with “preferentially”

L96-101. Long sentence – split into at least 2 sentences.

L143. What are the permafrost conditions in the catchments? Continuous or confined to the peatlands?

L292. How are these values calculated? Arithmetic means based on all sampling occasions? Or is there an adjustment based on the discharge at the time of sampling?

L293. Always write out units as mg L^{-1} , not mg/L .

L296. Use a SI unit to report mass C export, not t .

L297. How was DOC export calculated? There are several methods for estimating DOC export, and with many of them you can also estimate your uncertainty. Right now you are stating the DOC export with very high precision and no error estimates. Simple methods are outlined in Walling and Webb (1985 Marine Pollution Bulletin).

L297. Remove “Statistically speaking” as it is colloquial.

L297-302. Here it would be good if you could say for how long these events lasted. I.e. 85% of the DOC export occurred during X% of the time of the monitoring program.

L313. These correlations with discharge – were there distinct correlations during each year or during each season? You could carry out an ANCOVA to see whether slopes or intercepts of relationships differed for different periods.

L389-392. I don’t see how the first and second parts of this sentence are connected.

L416-425. I find that this discussion on runoff sources and relationship between discharge and DOC concentrations needs to be explicit about what water sources are considered to dominate stream flow during low-flow periods. In boreal catchments

Also, this positive relationship between DOC concentration and discharge is opposite what is generally found in boreal peatland catchments in Scandinavia and North America. What is different?

L452. I do not agree that peatland-derived DOC should be considered “autochthonous”. Usually that term implies DOC derived from aquatic primary production, and is contrasted to “allochthonous” DOC derived from the terrestrial surroundings.

L511-526. This seems to me like one of the most interesting findings of the study, and could be elaborated on more.

L538. The word “synthetically” is not appropriate – I believe you mean something like “through synthesis”.

L555. I don’t agree that DOC export among ecosystems within a catchment need to be proportional to the soil organic C stock of each ecosystem. There is no data to support this assumption.

L563. The term “verified NEE” should not be used. Perhaps you can use the term Net Ecosystem Carbon Balance (NECB), but then you also need to take into account methane-C emissions.

L579. This conclusion does not take into account shifts in water quality, including DOC concentrations, due to deepened flow-paths following active layer deepening through the mineral soil under the 30-40 cm organic soil.

L581. Stating too high precision on projected increases in precipitation. Also, what are the projected changes to evapotranspiration? If ET increases more than precipitation, then we would expect reduced runoff of course.

L616. The definition of NEE only includes the land-atmosphere exchange of CO₂ – you have further included DOC export in this NEE estimate that you compare DOC export to, which is not correct.

Fig7 and 8. I would recommend that you indicate at what depth the soil type changes from peat to mineral.