Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-372-AC2, 2020

Biogeosciences
Discussions

Interactive comment on "Drivers of seasonal and event scale DOC dynamics at the outlet of mountainous peatlands revealed by high frequency monitoring" *by* Thomas Rosset et al.

Thomas Rosset et al.

thomas.rosset@ensat.fr

Received and published: 15 January 2020

The manuscript of Thomas Rosset et al. aims at disentangling drivers of DOC dynamics in different peatland catchments. It utilizes a spectrofluorometric probe to monitoring frequency DOC concentrations. Additionally, parameters like temperature, precipitation, stream and peat water level were recorded to explain DOC concentration variability by means of statistic modelling. The generated dataset is quite extensive and worth to be published in Biogeosciences. As I was really pleased by the title and abstract I must say that the main part of the manuscript is rather descriptive and lacks a clear message or novelty. A major drawback of this manuscript is the lack of dis-

cussion of major processes and drivers concerning DOC export from peatlands, like hydrological flow paths, biogeochemistry and hydrologic preconditions. In its current state, the study is too much focused on the statistical results of their modelling, but no or little mechanistic explanation of the modelling results. What exactly happens during a rainfall/discharge/DOC concentration event in the different systems and concerning the hydrologic conditions? In general, I think that this paper needs a stronger discussion on hydrologic flow paths in peatland systems. Flow paths are not discussed until the very end. I miss a description of e.g. a simple acrotelm/catotelm distinction, which provide different hydraulic conductivities and thus lead to a distinct hydrograph. I miss a discussion of pre-event conditions, or of hydraulic conductivities in general, contributing to different flow paths. The interpretation of the piezometer data is difficult, as there is no information provided about depth of installation, hydraulic conductivities and thus how to interpret recession times. I miss mentioning importance of pH (DOC solubility!), do you have data about this? It is mentioned that the fen site is on limestone, while bog systems are generally acidic systems. As the authors are throwing in the term "bio-geochemical hotspot" in the end: I would be pleased to hear more about this earlier on in the manuscript. The introduction on second paragraph is rather superficial. Furthermore, I am very concerned by the representativeness of the bog site especially when it is compared to a fen as exemplary system (Scientific objective no 3, P3 line3). There are several factors differing between the sites, besides just fen/bog: climate (e.g. 4 months snow covered – no snow hardly sub-zero temperatures), anthropogenic influence (burned – unburned). Additionally, mentioning agro-pastoral practices: does this mean the bog is used for grazing? Could these systems thus be considered as representative? Besides this, from the location maps I draw conclusions that apparently the monitoring spot also receives water which is not originating from the peatlands itself. Is there any data about it? Do you have any idea about the whole catchment and how much water contributes to the discharge that is not from the peatlands? This is one of my major concerns, as I feel like the authors completely disregard this. If the concentration pattern are driven by discharge from other areas, the discussion of

concentration pattern and water levels at the monitoring spot and within the peatland would be difficult. Did you calculate also total DOC export fluxes next to the concentrations? This would highlight, how important this carbon output is to the system, as the concentrations seem to be quite low. Besides from this: Did you also compare the Bernadouze and Ech sites over a time period, where data from both sites are available? This would help to access, if there is any bias by having two different time periods here. Weather conditions can be very different each year having a very dry or wet year or season, re-spectively. I am no native speaker myself, so it is not easy for me to criticize language issues. But even though in your acknowledgement you state a language assistance,I am sure that there are some unusual or wrong wordings in the text (e.g. confusingus age of "contrasted" "strong concentrations"). So, from my point of view this needs further editing. Furthermore, your expression is imprecise at some points. Please edit (e.g. header of Table 1; P5 Lines 2-5: it is not clear what you mean; or speaking of "stream level increase") Please, check your figures for clear distinction and readability when not color printed (e.g. Fig 1 stream/peatland boundary, Fig. 5 Ech/Ber). In summary I see a very valuable dataset of potentially high interest. However, in its current state the discussion seems too superficial and the study remains rather descriptive. Therefore, I recommend major revisions before this study can be accepted for publication.

We thank the reviewer for constructive comments on our manuscript. Following the general comments, we have worked on improving the manuscript. First, we have improved the site description, including previous work (P4 L. 6). Then, we have included more details on the piezometers in the manuscript and added a table including installation depths, MRC, pH and DOC values for each study plot. Finally, we have improved the discussion section. We have tried to better emphasize the novelty and the contribution of our study (Section 5.1). We have included more discussion on hydrological processes that can be inferred from our study (Section 5.3 P11 L19 to P12 L9). We hope these improvements have clarified the manuscript. The answers to the specific comments can be found below.

P1 L 10/P2 L2: A very common number I know is 30% of global carbon stock. Please check more references for the number you give. Organic carbon stock in peatlands is a number constantly updated, at the submission time 20% was a figure obtained by crossing figures from Lefield and Menichetti, 2018 and Schalermann et al., 2014. According to the recent article from Nichols and Peteet, 2019, this figure is updated to 30%. We want to highlight the uncertainty of this figure writing $\sim$20-30%

P2 L 12-15: too simplistic statements. Drivers of DOC concentration are not dependent on latitude, but mostly on the system studied and climate The sentence has been removed.

P 2 L 32: "seasonal climatic conditions are contrasted" what does this mean? We meant that different abiotic parameters (temperature, precipitation, hydrology) evolves along both seasonal and event (snowmelt, rainstorms) scales. The sentence has been removed. Some details have been added in the text (P2 L. 33).

P3 L 19: What "logging activities" do you mean? Selective logging (1 tree over three was cut) was carried out during the autumn 2016 in the lowest forested area surrounding the peatland, producing no significant hydrological and biogeochemical offsets at the outlet of the peatland. These details have been added in the text P3 L20

P4 L 21: In which depths are the piezometers connected to the peat body? This is very important if you talk about recession times and peat water temperature. Maybe also interesting: What diameter have these wells? Piezometers wells are 50 mm diameter PVC tubes connected to the peat body at an average depth of 1.2 $\pm$0.3 m in Bernadouze, except for two piezometers in the center of the peatland which are connected at 2.2 m depth, and at an average depth of 2.4 $\pm$ 0.1 m in Ech. PVC tubes are slotted from the bottom to 10 cm below the soil surface. These details have been added in a table as Appendix B and in the text (P4 L. 29)

Or did you attempt to determine hydraulic conductivities by a slug test? + comment P13 L 8 ff: This is nothing else than hydraulic conductivities + comment P13 L 20: hydraulic

conductivity? Acrotelm/catotelm in bog vs fen? We agree that MRC are proxies of the hydraulic conductivities. To characterize the hydrodynamic properties of the peat, MRC were preferred to hydraulic conductivity estimation from slug tests because they can be performed directly with the water table dataset. Thus, the proposed model could be more easily tested on other peatlands. Details have been added to the manuscript P6 L4 to explain our choice in the methodology and some sentences have been modified in the discussion to P13 L26/ P14 L15 to refer to hydraulic conductivity.

P5 L 2 “Flood sampling” is a weird expression. What about event sampling? Expression has been changed. P5 L9

P5 L 12: when did “the turbidity events” occur? I assume this is mostly the case during high discharge. How much of your DOC event data is affected by this? High turbidity events occurs occasionally at the beginning of high discharge events (Rosset el al.,2019). The high turbidity period (> 20 FNU) are sporadic representing only 0,2% of the fDOM time series. Turbidity peaks do not affect the DOC event data, since DOC peaks occur after turbidity events. These details have been added in the manuscript (P5 L. 21).

P5 L 11: Reference not correctly inserted in text, happens occasionally in the manuscript The references have been modified (P5 L20 /P7 L8 / P7 L14)

P6 L 20: which variables were selected? Next sentences are unclear. The text has been rephrased for clarity (P7 L3).

P7 L 31 “...1.36 and 0.35. in...” unit missing The meter unit (m) has been added P8 L9

P7 L 19: why? You did not introduce site heterogeneity before (Introduction). I would also like to have an introduction in hydrologic connectivity, so are the different spots relevant for the discharge? What about the rest of the catchment? Site heterogeneity is discussed in the section 3.1 P4 L28. The first step of our investigation was based on an average peat water table (section 3.3), in order to explore the link with DOC

dynamics on the longest possible record. After the selection of the main explanatory variables, we investigated how different model performances between the bog and the fen with could be explained using the different recession times observed in the piezometer wells.

P8 L 4: Discuss different pre-conditions, changes in hydrologic conductivity with depth. This sentence aims at giving a general description of the water table depth at both site. This study focuses on DOC concentration peaks. Pre-conditions of each events were taken into account by defining explanatory variables integrating these pre-conditions as mentioned in Table 1 (water table level at the beginning of the DOC event, air and water temperature integrated on the 7 days prior the event, precipitation integrated on the 24 hours before the event, and time between peaks). Then, these pre-conditions are discussed in case they appeared as significant variable in the models.

P8 L 9: give conc. maxima. I am very surprised by the low mean values. When you have 2 and 7 mg/L mean DOC concentrations I am about to doubt the significance for the carbon balance. This only gives me the idea that you have a lot of water not originating from the peatlands itself. Calculating fluxes might be helpful here. You might check literature and compare. Our study sites are mountain peatlands, located in calcareous watersheds. The measured DOC concentration at the outlet are, as noticed by the reviewer, in the lower end of what can be expected in peatland sites. At the Bernadouze site, we performed an extensive study, including a high frequency survey at the inlet and outlet of the peatland. From this study (Rosset et al., 2019), we could conclude that the peatlands area contributed to 60 to 80 % of the fluvial carbon export. The specific fluxes estimated for the two sites range from 16.7$\pm$0.4 to 31.9$\pm$1.4 g.m-^{2}.yr-1 for bernadouze and 18.8$\pm$4.2 to 22$\pm$6.7 g.m-^{2}.yr -1 for Ech which is the high range of specific fluxes published for temperate peatlands and will be significant when establishing the carbon balance.

P8 L 31/32: Is this much? More than half cannot be explained by your model in Ech, other factors seem very important as well. Which could this be? The different model

performance between the two sites are discussed in terms of hydraulic conductivity (MRC) in the section 5.5 of the manuscript.

P 9 L 1: Peat water temperature dependent on depth? Peat water temperature was monitored in the piezometers and is representative of a mix of the whole water column.

P 9 L 9: speaking from “water table increase” and “water level increase” is confusing to me. Give more precise names, maybe include “stream” or “peat” for clear distinction The term stream and peat have been added when needed in the manuscript to make a clear distinction between water table and stream (section 4.4 and 5.5)

P9 L 19: This is a very simple statement and would be very odd, if this is not the case. This section describes the results, including simple statements like this one. However, the sentence has been slightly modified to shorten the description part (P9 L32).

P9 L 27: what is a “strong concentration”? The text has been modified to “higher concentration”. P10 L 7

P9 L 29: Please reword The title of the section has been modified to ‘Long term high frequency in situ monitoring” P10 L10

P9 L 30: Is this important? What is the novel statement/finding of your study? So far I mainly see confirmation of former results We do believe the high frequency survey is important for our results. First, without high frequency monitoring, the DOC peaks (< 32 hours) would never have been identified. This is a specificity of our mountain peatlands and such high numbers of events have never been reported. Then, the coupled analysis of DOC concentration and controlling parameter has allowed, to relate peat water table variation to DOC concentration at a very fine temporal scale. This, we are confident of, is a contribution to the literature since most study relate DOC and peat water table dynamic at the seasonal scale. The paragraph has been reworked to replace our study in the emerging literature on high frequency nutrient monitoring (P10 L. 11 to 21).

P 10 L 2 ff: Biogeochemistry? This is new and not mentioned before. Please introduce, the biogeochemical background or processes could be much more discussed in this paper As stated just above, the whole paragraph has been reworded.

P 10 L 5: expression “Thanks to” sounds uncommon, please reword. The sentence has been rephrased to ‘This was possible with ...”. P10 L19

P. 10 L 7 following: Chapter 5.2 is badly structured. If evapotranspiration is unlikely don’t put it there so prominent. The discussion of this part is interrupted and starts again further below -> confusing This section has been restructured to start with the discussion with the seasonal control on DOC concentration and later discuss other hypotheses for our site. Moreover a new section 5.4 has been created to discuss about snow influence and to enhance the clarity of our manuscript.

P11 L 1: Peat pore water DOC: You did not mention those in the result section. A short description/discussion about those concentrations would be nice. The peat porewater sampling and characterization has been added in the method section (P5 L3). The average DOC and pH for each piezometer have been detailed in the table A2 included in the appendix section. The discussion on peat porewater DOC concentration has been moved to section 5.5 P12 L3.

P11 L 3 f: What differences? Explanatory variables? Leachable Pools is a good keyword, but please discuss this on your data and not switch directly to the next topic. The discussion on difference on porewater DOC concentrations between the two sites, as stated above, has been rephrased and move to the section 5.5.

P11 L 8. This is indicative for the discussion. There is just an isolated statement that vegetation type plays a role for DOC production. How is this related to your findings? Concerning the two last comments, this related discussion part has been moved to section 5.5 P12 L7 since we think that parameters such as recession time, pH and main vegetation cover are interdependent in peatlands, all being related to hydrology. In this manuscript, we chose to emphasize the relationship between DOC concentration

and the recession time since we assume that hydrology is the principal driver of the peatland biogeochemistry.

P 11 L 21: This is not true! Please try some more literature search This formulation was unfortunate and was modified in the manuscript P11 L14. We wanted to emphasize the scarcity of studies coupling high frequency data of DOC concentrations in the stream and peat water table depth.

P 11 L 22: Discuss flow paths. Try e.g. DOC concentration vs Q plots. You throw in "non-linear flow DOC concentration relationship", try to discuss this.

The DOC concentration vs Q plots are included here for the reviewer's reference. The DOC vs Q plots are included here for the reviewer's reference. No systematic relationship is observed between DOC concentration and discharge values.

P 11 L 30: just single observations, discuss mechanisms This section 5. 3 of the discussion has been expanded to describe the mechanisms involved during flood events. See P11 L30 to P12 L10.

P 12 L 3-4. Provide references + P12 L3-12: This is all described before! Provide references and try to discuss more about pre-event conditions + P12 L 8f: This finding is not new. Give references. Answer to the three comments above. This section has been restructured. The revised text includes references on the link between the volume of aerated peat and DOC production on one hand and DOC transfer in the other hand P12 L20-21.

P12 L 13- P13 L13: Missing point: Conductivity in peatlands typically changes/decreases with depth! What depths are your piezometer? Give references! This is not a new topic! An important keyword here would be 'transmissivity feedback' or a similar effect. To characterize the hydrodynamic properties of the peat, MRC where preferred to hydraulic conductivity estimation from slug tests because they can be performed directly with the water table dataset. Thus, the proposed model could be

more easily tested on other peatlands. Information regarding piezometer installation depths have been included in the revised manuscript, in the Appendix section (Table A2).

P13 L 18: introduce the term "hot moments"; what are the processes inducing hot moments The term "hot moment" has been introduced in the first section of the discussion (section 5.1 P10 L17) and refers to (McClain et al., 2003).

Fig. 1: Additionally, as your catchment is in a mountain area, it might be helpful to have some contour lines. Altitudes of the peatlands were added in the figure caption. We prefer not to add the contour lines, since the figure are focused on the peatland areas, and not the whole catchment.

Fig. 2: showing an exemplary DOC event and way of examination is helpful. I would also like to see corresponding discharge values. In general an evaluation of DOC versus discharge (DOC/Q plot) might improve understanding of flow paths and DOC origin The DOC vs Q plots have been included above. However, we prefer not to include them in the manuscript.

Fig. 3: maybe add a line in the Bernadouze data set where the Ech dataset starts ->better comparability We do not want to compare two similar period, since climatic conditions are not the same from one site to the other. Instead we chose to discuss the statistical models which are built in order to be independent from climatic variabilities.

Fig. 6: I am a bit lost what you want to show here. This is not how a peatland complex looks like and far too generalized trends that you cannot state like this. This figure is a conceptual schema to describe the two characteristic type of peat units (bog/fen) which may contribute to the DOC transfer at the outlet of a peatland complex. This was intentionally generalized to present the extremum of contribution in term of peat units, one with a low hydraulic conductivity (long recession time) and the second with a high hydraulic conductivity (short recession time).

Table 1: Header is not self-explanatory. Why did you choose peat water temperature at the beginning of DOC event and not weekly mean like at the other parameter? Peat water temperature does not vary as the air, or stream temperature do at the event scale. We chose the beginning of the DOC event since peat water temperature is an integrative variable, evolving mainly at the seasonal scale.

Table 2: Check time period given for Ech. It seems to be incorrect (2015-2017?) It was a mistake. As you mentioned, the monitored time period in Ech was 22nd May 2017 to 13th February 2019

Cited references: Leifeld, J., & Menichetti, L. (2018). The underappreciated potential of peatlands in global climate change mitigation strategies. Nature communications, 9(1), 1071. Nichols, J. E., & Peteet, D. M. (2019). Rapid expansion of northern peatlands and doubled estimate of carbon storage. Nature Geoscience, 12(11), 917-921. Rosset, T., Binet, S., Antoine, J. M., Lerigoleur, E., Rigal, F., & Gandois, L. Drivers of seasonal and event scale DOC dynamics at the outlet of mountainous peatlands revealed by high frequency monitoring. Scharlemann, J. P., Tanner, E. V., Hiederer, R. and Kapos, V.: Global soil carbon: understanding and managing the largest terrestrial carbon pool, Carbon Manag., 5(1), 81–91, doi:10.4155/cmt.13.77, 2014.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-372/bg-2019-372-AC2-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-372, 2019.

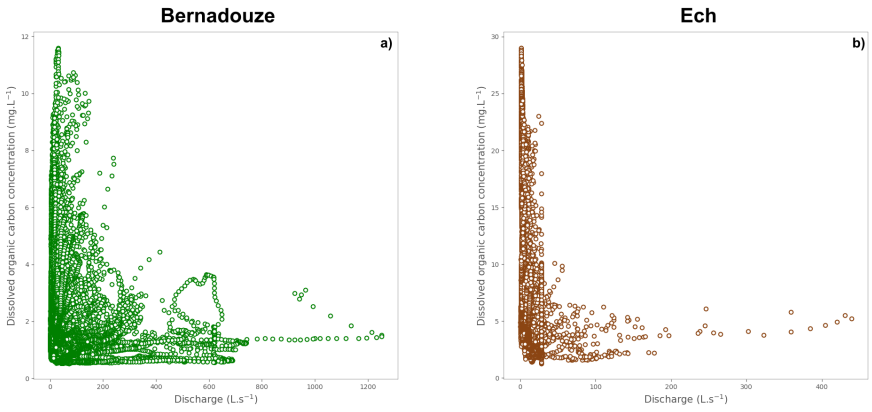

Fig. 1. DOC concentration vs Q plots