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Discussions

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Interactive comment on "Synchrony in catchment stream colour levels is driven by both local and regional climate" *by* Brian C. Doyle et al.

Anonymous Referee #2

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General comments: Doyle et al. present an interesting analysis of patterns in water color variability in three streams partly surrounded by blanket peats in Ireland. By using different statistical techniques they show that temporal patterns in water color are related to local-regional climate (North Atlantic Oscillation, NAO and soil temperature) and hydrology (soil moisture deficit or water discharge). They also estimate DOC export from the water color measurements.

Whereas the scientific questions addressed in this manuscript (ms) are relevant the conclusions are not entirely new. However, the methods used are so far not commonly used in studies of stream dissolved organic matter (DOM), though I suspect their popularity will increase in the future. This study could thus be regarded as a valuable addition to the literature on DOM dynamics in streams.

Specific comments: Though the scientific questions, data and analyses in this ms are relevant and important, the ms could be improved. There are e.g. a few unsubstantiated claims in the discussion, for instance about the relationship between catchment characteristics and water color variability. The authors have not presented any statistical evidence for such relationships, though they seem to have the data necessary. A few simple statistical analyses could thus show if these claims are true or not.

However, I am not sure if the authors really have enough data to show these relationships since they only study three streams. Therefore, I think Doyle et al. should de-emphasize the spatial patterns and concentrate on temporal patterns. The analyses of the temporal patterns are thorough and enough for one paper, and I think removing the speculation about spatial patterns would make for a more coherent paper. Also, the DOC export estimates may be relevant but does not really seem to be an integral part of the study – they seem a little misplaced but I think they could be better integrated into the study, e.g. by relating temporal variability in estimated DOC export to the different potential drivers presented (i.e. NAO, SMD and soil temp).

I would also urge the authors to be more careful about drawing conclusions about DOC dynamics based solely on water color variability. Only a small portion of DOM is actually colored (CDOM; see e.g. Ferrari et al., 1996) and it would be interesting to see an estimate of the differences between water color and DOC concentrations. A discussion about the uncertainties related to the assumption of water color equaling DOC would also be in order. For instance, from the information presented in the ms it seems like there is a larger difference in range in water color in Srahrevagh than in DOC. This has been observed elsewhere and has been hypothesized to be due to changes in iron concentrations (since iron also influence water color; see e.g. Kritzberg & Ekström, 2012).

I would also like to see an analysis of the covariation among the different "independent" variables that the authors use to explain patterns in water color, e.g. NAO, soil temp and SMD. I guess some of these variables co-vary and it would be interesting as a

reader to see HOW they co-vary (could fit in an appendix).

The text is well organized but sometimes feels a bit sloppy and needs to be overhauled – there are e.g. some strange wording and very dense text at times. For instance, some sections of the text needs to be divided into paragraphs. Also, the authors need to use a consistent terminology – as is the ms is sometimes confusing due to varying terminology. For instance, you need to be clear about if DOC concentrations or DOC export is the variable under study. In addition, sometimes the authors use "DOC yield" and sometimes "DOC load" but it is not clear if these terms refer to the same thing or are different concepts (are both concepts needed?). Also, since you are using color as a proxy, you are really only getting information about the colored part of DOM, i.e. CDOM.

The introduction and discussion could be better connected to the literature. I have several suggestions for references and some additional discussion topics (see technical comments).

There are sometimes mismatching information. For instance, in Table 2 the authors indicate that data from 2011 to 2017 was used but in the main text I got the impression that data were from 2011 to 2016. Also, there is some inconsistent use of acronyms in the text compared to figures, and inconsistent use of letters to indicate different panels in figures (sometimes upper-case letters, sometimes lower-case letters)

To summarize, the ms is worth publication but only after a thorough revision, including an overhaul of the text, a discussion about uncertainties relating to the actual differences between water color and DOC, perhaps removing the statistical patterns and a better integration of the DOC export estimates.

Technical comments:

Abstract

Line 5: well, all of this carbon is not transferred to the atmosphere since some of it may

be stored in long-term deposits such as lake or ocean sediments.

Line 10: Temporal change in what?

Line 12: I guess unit should be mg Pt Co L-1.

Line 12-14: I find this sentence a little odd; I expect something to follow the "Although ...". Ok, so the colour concentration was higher in Srahrevagh, but why "although"?

Line 17-18: Does these numbers (54% and 58%) refer to 1) soil temperature + soil moisture deficit and 2) NAO or to 1) soil temperature and 2) soil moisture and NAO. There are only two numbers but three variables making this sentence unclear. In the next sentence you refer to the combined effect of three variables; why do you not do that here?

Line 21: remove one "each"

Line 23: You use different number of digits here. Also, I guess these numbers are per km2. So should it be 15.0 and 14.7 t C km-2 yr-1? And why do you only report load for two of the three catchments?

Line 25: but the analysis you refer to above relate to concentrations. At least in the abstract, you do not mention what controls the C export.

Introduction

P 3 line 5: What do you mean by "...primary production exceeds ... soil organic matter"?

P 3 line 11-12: I suggest you remove the assumed sources of DOC and POC in the parentheses, partly because the sentence is general in form (for instance, not all DOC comes from peat degradation in many catchments) and partly because I am not aware of studies that clearly identify the primary source(s) of DOC or pathway of DOC formation. Previous studies in the UK have e.g. shown that DOC generally is of recent origin, i.e. post 1950s (see e.g. Evans et al., 2007 or Billett et al., 2007). This does not mean that old peat is NOT decomposed but that most of the DOC comes from younger

sources (could, however, be young peat!).

P3 line 12: remove "more" before "dominant"

P 3 line 12-16: You need to be careful with the terminology here. Do you e.g. mean that 60 and 88% of total carbon load, i.e. including DOC, POC, DIC and PIC, were DOC? Or do you mean that 60-88% of total organic C was DOC? Whereas DOC may dominate in many areas, this is often not true where there are large portions of calcareous bedrock within the catchment. Thus, you need to clarify if you mean total C (i.e. including inorganic C) or total organic C.

P3 line 20: yes, but not only decomposition but temperature will also affect other potential DOC forming processes, e.g. root exudates from primary producers

P 3 line 26: I guess it is the DOC in the lakes that correlate with climate indices, not the lakes themselves, or?

P3 line 29: remove the acronym SLP – it is not being used anywhere else in the manuscript and thus superfluous.

P 3 line 32: What should Ref be?

P4 line 14-16: This was also found by Winterdahl et al. (2014) where TOC was increasing in about half of 130 streams, but without any clear geographical patterns. Also worth mentioning is that some authors claim that DOC concentrations have stopped increasing or are actually decreasing (Worrall et al., 2017) whereas others have pointed to methodological differences among studies that limit interpretations of potential trends (Filella & Rodriguez-Murillo, 2014).

P4 line 16: But the studies referred above did not study DOC export but DOC concentrations. Once again you need to make clear if it is export or concentrations that are the focus of study. The export is to a large extent controlled by water discharge, and thus ultimately by the difference between precipitation and evapotranspiration.

P4 line 21-22: This sentence seems out of topic – you have not discussed effects of changes in nutrient cycles before and Kurbatova et al. studied Russian bogs which I suspect behave quite differently compared to the blanket peats on the British Islands (in terms of e.g. hydrology and topography).

P4 line 28-29: Repetition. You have already mentioned that this is one of your study catchments.

P4 line 32: change to "...climatic conditions, e.g. the NAO, as a possible..."

P4-P5 line 33 ff: The aims need clarification. First you mention the Burrishoole catchment but later you write "water colour from rivers in three sub-catchments in a blanket peatland catchment" – why not specify that this is the Burrishoole catchment? Also, part 2 need to be specified; the effects of main climatic drivers on what?

P5 line 6-8: It would be interesting to know the area of the entire Burrishoole catchment.

P5 line 18: You can remove the (Co-ORdinated INformation on the Environment) but write CORINE in capital letters (as you do in the reference list).

P5 line 24: why do you report precipitation only for 2010-2016 when you obviously have a longer time series of precipitation from the area? The mean precipitation for 2010-2016 is reported in the results anyway.

P5 line 26: you repeat "spatially" here – remove one

P5 line 27: Above you did not use a thousand separator (,) but here you do. You need to be consistent and comply with the format of the journal.

P5 lines 26-28: Are these precipitation numbers from the same year? Or are they annual means? That is not clear now. I think you need to show the spatial variability better because as it is now, it is not clear how these observations differ from Newport (besides that the numbers are a bit different). You could perhaps show how large the spatial differences are on average among years (including all three stations with precip.

data).

P6 line 8: I guess this should be (Figure 1 and Table 1)

P6 line 16: change to "three sub-catchments"

P6 line 23: Change to "10 m resolution"

P6 lines 26-27: What was the precision of this instrument? If you have data on accuracy, that would also be relevant to report here.

P6 line 29: "Daily precipitation and soil temperature data..."

P6 line 30- ff: How was the rating curve calibrated? I.e. what methods were used to construct the rating curve? I also think you should report the accuracy of this rating curve, e.g. with an R2.

P7 line 6-8: Why two different tests?

P7 line 16-17: This sentence needs to be rephrased. Should the second "for" be removed?

P7 line 21: I know many authors equal colored DOM and DOC but since only a (small) fraction of DOM actually is colored (see e.g. Ferrari et al., 1996) you might want to refer to CDOM here (and at other places where you use color to draw conclusions about DOC) instead,

P7 line 23: General additive mixed models – this section is a little dense. Could you please divide it into a few paragraphs?

P8 line 6: Unit should be (m s-1).

P8 line 7: So the humidity here should actually be "relative humidity"

P8 line 8: How was actual evaporation estimated?

P8 line 18: I guess SMD is soil moisture deficit but the acronym has not been defined.

P8 line 30: This should be rephrased. Water color was not converted but DOC was estimated from water color. I think you need to be clearer about this throughout the manuscript.

P9 line 1: There should be a . after "sub-catchment rivers".

P9 line 2: Is this really the accuracy? Or is it the precision? If this is the accuracy, what then is the precision? Also, on this line it should be "5 ppb".

P9 line 3: New paragraph before "Mean annual yield..."

P9 line 4-5: This sentence should be moved so that it precedes the previous sentence, i.e. first this sentence (starting with "The mean annual load...") and then the sentence starting with "Mean annual yield..."

P9 line 5: I am a bit confused here, but I think you mean the estimated DOC here, right?

P9 line 8: Should it be "...year, with 2013 being the driest year, with..." or something similar?

P9 line 16-17: Do you really have the precision to report these numbers with one decimal? Above you did not use a decimal and I think you should be consistent here (also, how many decimals are realistic based on your measurement equipment?).

P9 line 28: This would be easier to see if you also report the specific discharge in e.g. mm/d.

P10 line 5: But the cumulative SMD should have unit mm (only), right?

P10 line 20-22: This "random component" does not seem to be entirely random, at least not from what I can tell from figure 3D. How does the autocorrelation of this random component look like? Would it be possible to subtract even more information from this time series (though I have no idea how to do that)?

P10 line 25-28: This information seems misplaced. Why not combine this with the text in the beginning of this section where you also refer to which stream having the highest concentrations?

P11 line 13: "...the optimal model..."?

P11 line 31: Not sure if I agree about discharge here. Based on figure 5, NAO, soil temp and water color seem similar but the increasing trend in discharge starts more than a year after the increase in NAO.

P12 line 20-25: This section is unclear. Is the second set of numbers reported (i.e. 18.5 and 11.8 t C km2 yr-1) averages among all the study streams? It is not clear how these differ from the first set of numbers (which apparently were for individual streams in individual years). Everything becomes clear when looking at Table 3 but it should be clear from the text as well. Also, sometimes you use the term yield and sometimes load – do these mean different things here?

P12 line 24: This sentence is a bit confusing. I think you should change this to "...while 2013 had the least total DOC load...". You have already reported that 2013 was the driest year – there is no need to reiterate that here.

P12 line 28-29: You should rephrase this sentence. It is unclear, probably because of the misplaced modifier "which" that refer to "DOC levels" or possible to "water colour" in this case. I also think you should change the statement "explained circa 60%" to "explained between 54 and 66 %".

P13 line 2-3: Though I suspect you are right, do you have data to confirm this statement? Do you e.g. have fluorescence data that indicate that DOC primarily is of terrestrial origin? If not, I think you should be more careful and write something like "...probably originates primarily from the surrounding catchment...".

P13 line 3-6: If I understand this sentence correctly you claim that you have shown that the DOC export from the different study catchments in your study are related to

catchment properties, land use, runoff etc. But this is incorrect, you have not shown this. There are no data that show these relationships.

P13 line 8-10: But you do not present such an analysis – you only have three sites, so the statistics will be a little shaky, but can you find any of these relationships that you mention? Figures in an appendix could be enough to show if there are any relationship between water color and e.g. the extent of peat soils in the catchment.

P13 line 14-17: Perhaps, but other studies have not found any clear downstream patterns in DOC concentration (see e.g. Temnerud & Bishop, 2005 and Creed et al., 2015) or clear signs of DOC degradation as water moves downstream in a stream network (see e.g. Winterdahl et al., 2016).

P13 line 21-23: Is it necessary to reiterate the results here?

P13 line 24: Wouldn't Christ and David (1996) and Neff and Hooper (2002) be more relevant references here since they have actually looked at the relationship between temperature and DOC "production/leaching".

P13 line 28-30: You touch upon this but it could perhaps be clarified. You need to think of what you, and most other scientists in this business, refer to as "DOC production" as two different processes (if we simplify everything and ignore e.g. sorption dynamics, solution/dissolution due to changes in water chemistry etc.): 1) the actual DOC production, i.e. some process that forms DOC (could be e.g. exudation of organic molecules through roots or microbial degradation of solid organic matter), and 2) transport of DOC along active flow pathways in the soil. Process 1) could be active as long as there is water in the soil, even if this water is not moving. Process 2) only happens when the water is actually moving. That is, you could have an area with stagnant soil water where DOC production (process 1) forms a "stock" of DOC that is transported to a nearby surface water body as soon as the flow pathways are activated.

P13-14 line 33-1: But this is not generally the case for DOC, see e.g. data from about

130 streams in Winterdahl et al. (2014) where there is no relationship between seasonality and DOC concentration.

P14 line 6: As I understand this, you mean that since concentrations decrease, the export will also decrease. In this case, this is probably true since if soil moisture decreases, stream discharge will also likely decrease. But generally, you can have decreasing concentrations but increasing export if discharge increases. Since discharge on event scales can vary by several orders of magnitude whereas concentrations seldom vary by more than a factor 10, discharge often control the export dynamics, at least on short time scales. Therefore, I think you should remove "...and therefore export..." here.

P14 line 21: Change to "...DOC concentrations have been observed in peatland streams..."

P14 line 13-23: I agree that the effect of hydrology on DOC dynamics is complex and that there is probably a multitude of interactions. One interaction that you do not discuss is the effect of different flow pathways at different discharge conditions (see e.g. Bishop et al., 2004 and Seibert et al., 2009). If you have more organic rich soils close to the soil surface compared to deeper soils, one could expect that concentrations are higher at high stream discharge compared to at low stream discharge. What do the relationship between log(color) and log(discharge) look like? Positive, negative or neither? For Glenamong, which is the only site where you report a similar relationship, this looks complex but generally positive. There are several studies that have looked at such C-Q relationships (see e.g. Creed et al., 2015; Musolff et al., 2015; Moatar et al., 2017; and Winterdahl et al., 2014).

P15 line 23-24: "...warm and dry rather than warm and wet conditions..."

P15 line 26: "...time-series analysis at the annual..."?

P15 line 27: remove the . before "both"

P15 line 30: Colder and drier than what? Change to "...to relatively cold and dry conditions, and dry weather..."

P15 line 31: "Cold conditions..."

P16 line 5: "...minimum annual total DOC yield..."

P16 line 7: New sentence at "However..."

P16 line 8-9: Perhaps true, but your case would be stronger if you could show this with data and statistics – are there any relationships between annual export and e.g. NAO, precipitation or temperature?

P16 line 16-17: Again, this is not something you have shown with data and statistics. However, you may not have the data to actually show this since you only study three streams. I think you should de-emphasize the spatial patterns and concentrate on temporal patterns.

Figure 1: The figure text should start with a capital letter.

Figure 2: There is a parenthesis, which should be removed, at the end of Standardised Precipitation Index on the axis label in A. Also, should the unit for Soil Moisture Deficit (on the left axis) be mm/d?

Figure 4: What are the units on the axes (if any)? Another detail, in previous figures you have indexed sub-figures (panels) with capital letters but now you use lower-case letters. I think you should be consistent.

Figure 5: What is actually displayed in these figures? The text gives some information but there is nothing on the vertical axes – should there be labels and units here? And in e), is that some composite trend (how was that done?) since you write that this is "mean colour concentration in the three sub-catchment rivers"?

Figure 6: I would prefer to use letters to name the different panels instead of writing e.g. "bottom left". There is a) missing after "top left".

Table 1: The table text should start with a capital letter. Also, what do numbers within parentheses mean? Are those standard dev.? If so, why are you reporting ranges for some parameters but means + std. dev. for others? In addition, I guess the water chemistry data is for stream water but I think it would be good if you clarify this in the caption. What does (312) mean after CORINE Coniferous Forest %?

Table 2: The table text should start with a capital letter. Also, should there be a , after Table 2? Here you write that the data cover 2011-2017 but from the main text I got the impression that data was from 2011-2016. Which is correct? In addition, I think it would be clearer if you used the same acronyms in this table as you use in the main text, i.e. SMD for soil moisture deficit, NAO and only Stemp100 (instead of s(Stemp100)). What does s(...) mean anyway? Is that what is reported by R?

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