

Responses- Anonymous Referee #3

Thank you for taking the time to review our manuscript and for your positive feedback on the overall scope of the paper. Below you will find our responses to your comments, which are greatly appreciated and have improved the paper. Please feel free to contact us with any additional questions or comments.

Responses to General Comments (GC):

GC1: Statements about global relevance should be tempered accordingly because watersheds studied are so small.

Author Response: To put our watersheds into better context, we have included additional comparisons between our watershed yields and yields from watersheds of similar size from around the world. We have also done a simple calculation of regional flux to compare potential DOC export from the region studied here, to other regions within the coastal temperate rainforest. We hope this provides better context for our flux and yield measurements relative to regional and global estimates of riverine carbon exports.

GC2: The extensive dataset on DOM quality could also be better utilized to understand the mechanisms that are driving DOM export rather than just make broad observations about streamwater DOM quality.

Author Response: The outer-central coast of British Columbia's perhumid coastal temperate rainforest is largely unstudied with respect to DOC exports, so a primary goal of this manuscript is to establish in the literature a detailed description of DOC exports for this region and to identify potential sources and patterns in DOM composition. We have included the RDA analysis to assess potential watershed/landscape drivers of DOC concentration and DOM composition. However, we agree that the manuscript benefits from more utilization of the DOM dataset. To address this comment, we have included additional analysis on the relationship between DOC and DOM composition with discharge and stream temperature. We have also done additional analysis on the relationship between PARAFAC components that goes beyond the analysis presented in the RDA. Please see specific comments below for further information.

GC3: There is no such thing as a "globally important" DOC yield... The yields reported here are quite high, however this is largely a function of the fact that DOC yields (flux per area) are inversely related to watershed size and the watersheds in this study are very small and have high wetlands coverage. For this to represent a globally important finding, the authors would have to make the case that the fluxes measured here are broadly representative of the 100,000 km² perhumid coastal forest in BC and thus provide evidence of a substantial mass flux of DOC to the coastal ocean. I understand that the purpose of this paper was not to calculate regional fluxes, however more directly addressing the issue of how regionally representative these high flux watersheds are would: 1) give readers a more concrete sense of the regional/global importance of these fluxes and 2) better justify statements such as "the small watersheds of this region export very high amounts of terrestrial DOC" (Line 477). The only place this issue is addressed in the paper is briefly in the conclusions (lines 552-554).

A similar issue arises in the discussion of the yields in section 4.1. Comparing DOC yields from these 3-10 km² watersheds with yields from the Congo and Amazon doesn't make sense given the difference in scale. The Congo exports more than 10 Tg DOC/yr and all of the watersheds in this study together export probably 1/1000th of a Tg DOC/yr. The claim that DOC yields measured in this study are higher than those reported in southeast Asia should also be clarified given that Moore et al. (2011, 2013) have reported DOC yields >2x those reported here for watersheds in Indonesia that are several orders of magnitude larger than the watersheds in this study (doi:10.5194/bg-8-901-2011; doi:10.1038/nature11818).

Author Response: Thank you for this comment, you make some good points. We have made the following changes:

- 1) To put numbers into a more regional and global context we have included a simple regional estimate for total DOC flux from the hypermaritime region of B.C.'s perhumid coastal temperate rainforest.
- 2) We included flux estimates from global to regional scales.
- 3) We removed comparisons of our DOC yields with much larger rivers and instead include comparisons of watersheds of similar size, in particular those that have high amounts of precipitation, and contain extensive organic soils and wetlands. We emphasize that to the best of our knowledge, this is the first study that represents the role that these types of small catchments (high latitude, temperate, wetland and peat or organic soil-dominated) play in delivery of DOC directly to the ocean.

GC4: This is a very rich data set in terms of DOM compositional information. That said, the compositional data were somewhat underutilized in the study. For example, the 13C data were not even mentioned in the Discussion. In addition, the stream gage data are not utilized to elucidate how streamflow impacts DOM quality. Instead there are general statements about how compositional data change between wet and dry seasons (e.g. lines 445-456).

In Fig. 3 it appears that streamwater DOC concentrations are correlated with air temperature. If this is the case it would suggest that there is a link between soil temperature and soil water DOC production that influences the export of DOC to streams. Thus, temperature may be useful for predicting seasonal changes in streamwater DOC concentrations.

Author Response: We conducted additional analysis using linear mixed effects models to look at relationships between DOM compositional data (including 13C-DOC), DOC concentration, discharge and temperature. We refined our objectives to include the rationale for this additional analysis (e.g., possible seasonal and spatial trends and drivers) and to address general comments regarding incorporating DOM data to look at temporal and cross-watershed patterns. The methods for this additional analysis are presented in the new Section 2.7, results are presented in Sections 3.3 and 3.4, as well as included as a figure (Figure S6.1) and two tables (Table S6.1, S6.2) in Supplementary Material. Additional discussion is provided in Section 4.3.

Responses to Specific Comments (SC):

SC1: There is some discussion material mixed in to the Results section of the paper. Examples include: Lines 336-339 and 345-347.

Author Response: This text (and other text that bordered on discussion) has been removed from Results and is now included in Discussion. Examples given by the reviewer are now included in Section 4.3.

SC2: There are a number of references to watershed residence time in the Discussion (for example, lines 433, 492, 502), but it is not clear how this was quantified and whether it was function solely of lake influence or if watershed slope played a role as well.

Author Response: We did not specifically quantify residence time for watersheds, however we do know that based on the very rapid hydrograph response to precipitation events, the response time of these catchments is short. Where appropriate (such as example from line 433) we have changed this to hydrologic “response” time. In other places (such as example from 492) we removed the sentence entirely. Line 502 is providing an example from the literature, so that reference to residence times was left in the text, similar to line 512, but the text was changed here to be more explicit that this wasn’t something we measured directly but an effect we would expect to see based on other watershed factors. In addition to lakes, watershed slope definitely plays a role in response/residence time, this is mentioned in line 530 of the original document and also in the results and discussion related to the RDA analysis. We have also included a figure in the Supplement (Fig. S2.2) that illustrates the response times of our watersheds with and without a high extent of lake area.

SC3: Line 74: The phrase “predictions of ecosystem productivity and food webs” is extremely Vague

Author Response: Changed this to just “predictions of ecosystem productivity”

SC4: Lines 100-101: How and why would you expect DOC export from perhumid forests in Alaska to be different from perhumid forests in British Columbia? In other words, is there a reason to think that the work done in Alaska would not be valid in the same forest type in British Columbia?

Author Response: We have included a few sentences in the text (inserted in 2nd to last paragraph of Introduction in new manuscript) that describe how and why we would expect DOC export to be different in the study region of B.C. versus Alaska:

“Within the large perhumid CTR, there is substantial spatial variation in climate and landscape characteristics that create uncertainty about carbon cycling and pattern. In Alaska, for example, riverine DOC concentrations vary with wetland cover (D’Amore et al. 2015) and glacial cover (Fellman et al. 2014). Previous studies have shown that streams in southeast Alaska can contain high DOC concentrations (Fellman et al., 2010; D’Amore et al., 2015a) and produce high DOC yields (D’Amore et al., 2015b; D’Amore et al., 2016, Stackpoole et al., 2016), but no known field estimates have been generated for the perhumid CTR of British Columbia, an area of approximately 97,824 km² (adapted from Wolf et al., 1995). Within the perhumid CTR of British Columbia, terrestrial ecologists have defined a large (29,935 km²) *hypermaritime* sub-region where rainfall dominates over snow, seasonality is moderated by the ocean, and wetlands are extensive (Pojar et al., 1991; area estimated using British Columbia Biogeoclimatic

Ecosystem Classification Subzone/Variant mapping Version 10, August 31, 2016, available at: <https://catalogue.data.gov.bc.ca/dataset/f358a53b-ffde-4830-a325-a5a03ff672c3>). Previous work in the hypermaritime CTR showed that DOC concentrations are high in small streams and tend to increase during rain events (Gibson et al., 2000; Fitzgerald et al., 2003; Emili and Price, 2013). Taken together, these conditions should be expected to generate high yields and fluxes of DOC from hypermaritime watersheds to the coastal ocean.”

SC5: Lines 104-105: The fact that discharge was directly measured is a strength of this study, however it is somewhat misleading to compare this highly localized study to continental and global scale studies where modeling discharge is a necessity.

Author Response: This information was originally included to highlight the need for studies in this region that include the direct measurement of discharge, because the only work that has attempted to quantify DOC flux have been large scale studies using modeled discharge. These studies may not be appropriately capturing the heterogeneity of this complex region (see response to comment 4 above) and highlights the challenge of working in these remote locations (modelling discharge has been the only option until our paper). However, we have removed this specific text and comparison with global scale studies and now only make comparisons with regional, smaller scale studies and estimates of flux.

SC6: Line 273: It seems redundant to report climate data in the study site and in the results. Also the values reported for mean annual precipitation differ between the study site (line 115) and the results (line 273).

Author Response: We removed the second reference to climate data in the results. Mean annual precipitation (MAP) for the study sites (line 115) is taken from sea level and central to all the study watersheds. The MAPs reported in the results (line 273 and 276) are taken from the exact location of our rain gauge and from the location of our high elevation weather station. The spatial distribution of rain in this area is extremely heterogeneous, and the range of values is presented to illustrate the differences across the landscape.

SC7: Line 278: The comparison of precipitation at the study site to “most regions of the world” is vague and does not illustrate anything meaningful.

Author Response: We removed “most regions of the world”

SC8: Lines 291-295: This sentence is repetitive and very hard to follow with all of the parenthetical data references. Recommend simplifying it to make the point about the difference in wet season flow without all of the Q data. It is also interesting that wet season Q differed by >20% between the two years while wet season precipitation only varied by 5%.

Author Response: We removed most of the parenthetical data references except for two that describe total discharge and range for water year 2015 and water year 2016. The difference in precipitation and flow between the two years is a function of, 1) precipitation arriving as snow at higher elevations that is not captured in the rain gauge, and 2) heterogeneity of rainfall across the study region. The rain gauge is centrally located in one catchment within the study region,

however this gauge probably does not capture the full range of precipitation being delivered across the islands. However, these differences would more likely be reflected in differences in Q.

SC9: Line 326: It would be more clear to say that SUVA values were at the high end of the range rather than “relatively high compared to the range”.

Author Response: Modified the text as recommended.

SC10: Line 417: “Catchment” looks like it should be plural.

Author Response: This sentence has been removed.

SC11: Line 419: The term “a significant biogeochemical hotspot for coastal carbon cycling” is somewhat vague. Many of the studies cited in this paper calculate end of pipe DOC fluxes “directly to the coastal ocean”. It would be helpful to more specifically explain why the watershed DOC fluxes in this study are “significant” from the standpoint of the coastal C cycle.

Author Response: The paragraph containing this sentence has been removed during modification of this section based on other reviewer comments.

SC12: Lines 425-6: Does the term “high precipitation event” refer to intensity or magnitude. Also, it seems like the slope of these watersheds (typically >30%) is an important factor in the short hydrologic residence times that is not mentioned in this paragraph.

Author Response: “High precipitation event” refers to both. We modified this sentence to reflect those details “Therefore, frequent precipitation of high magnitude or intensity” We agree that slope is potentially an important factor influencing DOC export, and have mentioned it in Section 4.2 several times both in reference to high-gradient catchments, and the role of slope in variation between watersheds.

SC13: Lines 430-431: I agree that seasonality is important for ecological processes and it would be helpful to provide more analysis about why this would be the case in this region.

Author Response: These specific lines have been removed during modification of this section, however, we have incorporated text in the same area of discussion to highlight that the seasonal contribution of DOC from these watersheds to the ocean “may represent a relatively fresh, seasonally-consistent contribution of terrestrial subsidy from streams to the coastal ecosystem, which is relatively lower in carbon and nutrients throughout much of the year (Whitney et al., 2005, Johannessen et al., 2008).” The importance of seasonality in ecological processes is widely known in terms of production (both primary and secondary), and additional analysis on the importance of seasonality in terms of broader ecological processes is outside the scope of this paper.

SC14: Line 455-456: Again, the consequences should be explained or this sentence should be removed.

Author Response: This sentence was moved to the beginning of the following paragraph (last paragraph of Section 4.3) where we describe some of the effects of composition on biological utilization.

SC15: Line 546: Because yields are a measure of the per area export (flux) of DOC the term “export the highest yields” is redundant.

Author Response: Changed “export” to “contribute”