Biogeosciences Discuss.,
doi:10.5194/bg-2017-5-RC1, 2017

Biogeosciences Discussions

Interactive comment on "Globally significant yields of dissolved organic carbon from small watersheds of the Pacific coastal temperate rainforest" *by* Allison A. Oliver et al.

Anonymous Referee #1

Received and published: 15 February 2017

Review Oliver et al.; Globally significant yields of dissolved organic carbon from small watersheds of the Pacific coastal temperate rainforest.

General comments This manuscript examines the fluxes of DOC and the composition of DOM by stable carbon isotopes and optical measurements in several small catchments in the perhumid region of the Pacific coastal rainforest of North America. The periods of sampling for DOC concentrations and DOM composition differ but cover at least 2 years except for fluorescence measurements ($\sim$ 6 months). Analytical methods are sound. The dataset presented by the authors in such underrepresented areas represents certainly a valuable contribution for the global understanding and consideration of DOC/DOM dynamics in small rivers flowing directly into coastal waters. Overall, the

manuscript is highly (too?) descriptive and – considering the large dataset submitted by the authors – we could regret the lack of more specific goal/questions for this study. Furthermore, there are some approximation regarding DOC fluxes/yields that need to be corrected and/or clarified and I found that data were poorly included in the section aiming to describe the temporal dynamics of DOM in the catchments.

Specific comments Lines 107-110: the scientific objectives of this paper are too general.

Line 142: please define GIS.

Line 146: how have been defined the boundaries between organic and mineral soils? It would be very informative to compare a soil map and a map showing the location of wetlands and lakes within catchments.

Line 168: I suggest to use the more common notation 'δ13C' or 'δ13CDOC'.

Line 175: what size of filtration?

Line 240: define PARAFAC.

Line 301 and 306: change for 'Table 1' in the brackets.

Lines 304-310: I am not sure that is very significant. Maybe a statistical test could support this.

Line 312: why there is no DOC fluxes/yield for WY2014 while sampling for DOC has started in October 2014?

Lines 326-328: Such elevated SUVA values are commonly found in tropical rivers (e.g. Lambert et al., 2016, Biogeosciences, 13, 5405-5420) or in streams draining wetlands (e.g. Agren et al., 2008). This should be noted as it is not an exception but rather typical of environments exporting large quantities of highly aromatic DOM.

Lines 345-347: maybe this should be moved into the discussion as its interpretation of

d13C-DOC data.

Lines 325-347: where are the Fluorescence Index and the Freshness Index? Even if they have been measured over a short period, they should be described as they are included in the RDA.

Lines 348-373 & table 2: it would be informative that the corresponding number of PARAFAC components in other studies appears in table 2. For example, it's not clear what component identified in Graeber et al. (2012) matches C1. Also, according to Fellman et al. (2010), components similar to C2 are commonly reported in freshwaters. Overall this paragraph is hard to follow, mainly because figures 6 and S4.4 are not very efficient to support the text. Y-axis in figure S4.4 should be adapted for each PARAFAC component, and figure 6 could be modified in order to present temporal variability for some representative catchments. Also, some statistical tests are welcome in order to support the variability between catchments and between seasons.

Line 374: DOC export is not investigated in RDA, please change the title accordingly.

Line 374: because PARAFAC components track different fractions of the DOM pools I would suggest to perform the RDA with all components, or at least to include C3 and C5.

Line 380: I don't think that the term 'inundation' is relevant here as wetlands are wet environments. Clearly the RDA1 identifies two elements of the landscape (i.e. wetlands and lakes) as being important drivers for the spatial variability in DOC concentrations and DOM composition.

Line 384: there is no information about soil composition as only the depths of organic and mineral soil horizon have been measured.

Lines 394: the title of the section 4.1 needs to be corrected because DOC yields and DOC fluxes are calculated differently and therefore have not the same meaning. See the next comment.

Lines 395-404: this section is confusing because DOC yields and DOC fluxes have not the same meaning: DOC fluxes are the amount (mass) that passes a given point on the river over a given period of time while DOC yields are the flux per unit drainage area. If it is true that DOC yields of the study sites are higher or comparable to those estimated for some tropical rivers (higher than the Congo and the Amazon rivers but comparable relative to the Siak River), DOC fluxes are clearly lower compared to these systems (see figure). Moreover, as shown by Agren et al. (2007), DOC yields trend to decrease with catchment areas because of (1) better connection between terrestrial and aquatic ecosystems in headwater catchments, (2) reduced in stream losses in small streams and (3) increasing contribution of DOC-poor groundwater in large rivers. Consequently, the authors should compare their DOC yields to tropical catchments having similar drainage areas to support the statement that DOC yields from Calvert and Hecate Island are some of the highest recorded globally (lines 395-396). Furthermore, this statement should be taken with caution because very high DOC concentrations (> 15 mg/l) are commonly found in tropical rivers (e.g. Mayorga et al., 2003), especially in the central part of the Congo Basin where small streams < 100 km2 can have DOC concentrations up to 70 mg/l (e.g. Lambert et al., 2016, Biogeosciences, 13, 5405-5420), having thus likely among the highest DOC yields and export for streams.

Lines 408-409: is it valid for the sites studied by the authors?

Lines 405-431: this part of the manuscript is quite long and looks like more as an introduction for the section 4.2 rather than a discussion including the data.

Line 414: do you have a reference?

Lines 415-417: do you have an idea about how much represent freshwater masses compared to coastal water masses?

Lines 428-430: this phenomenon is commonly referred as 'DOC flushing' (Boyer et al., 1996, Ecological modelling, 86, 183-188) and should be moved to the beginning of section 4.2 (nothing to do with DOC fluxes/yields). Maybe the authors could also

exploit their data to discuss about hypothesis around 'DOC flushing'? Indeed, d13C-DOC values been found to investigate change in sources and pathways of DOC in small catchments (e.g. Sanderman et al., 2009, WRR, 45, W03418; Lambert et al., 2013). It is a pity that isotopic measurements made for this study are not discussed and related to the temporal and spatial variability of DOC.

Line 432: the manuscript presents no data allowing to investigate the effects of fresh DOC fluxes in coastal waters. Even if I agree that the delivery of fresh terrestrial material likely impact coastal marine foodwebs I would suggest to modify the title of the section.

Lines 433-456: it is not clear what are driving the changes in DOM composition. What are the 'microbial products and plants exudates' (line 450)? I strongly suggest to use d13C-DOC values to go deeper in this section in parallel with optical properties of DOM. How vary the fluorescence index and the freshness index and % of PARAFAC components? Is there difference in temporal variability between catchments?

Lines 457-479: ok but speculative. Maybe this could be moved at the end of the discussion. Also, potential implications of the data are included several times along the text (lines 412-414, 417-419, 431...) and consequently are quite redundant.

Section 4.3: This section can be shortened. Wetlands and lakes are known to be two major elements of the landscape having contrasting effects on DOC and DOM quality (e.g. Frost et al., 2006, Aquatic Science, 68, 40-51; Lambert et al., 2016, Biogeosciences, 13, 2727-2741) and it is relatively obvious from RDA1 that DOM concentrations and composition are largely driven by wetlands and lakes in this study. The authors should better explain the role of wetlands/lakes rather than looking for additional and questionable drivers that cannot be supported by their data.

Lines 491-492: What is the meaning of 'alternative DOC-source pools'?

Lines 491-492: watershed residence times are unknown so they can be not considered

as a driver because changes between seasons could be very low due to the small size of the catchments.

Lines 503-506: could you add figures to illustrate this?

Lines 515-516: this statement cannot be supported by the data included in the study. Soil analysis are limited to the measurements of organic and mineral horizons depths (please clarify how this has been done), and there is no information regarding soil composition (%Corg, C/N ratio, content in Al- or Fe- minerals...) that could help to investigate the role of soil composition on stream DOC dynamics. Moreover, the variability of soil organic and mineral horizons between catchments in table 1 is relatively limited. Finally, I am not convinced by the argumentation based on the RDA. For example, vectors DOC and OrgSoil have respectively negative and positive loadings along RDA1, suggesting a limited correlation. Also, the difference in the lengths of vectors C1 and Slope along RDA3 suggests that slopes are not a strong predictor for C1 (lines 529-531), as also suggested by the lack of relationship between these two vectors along RDA1.

Lines 524-526: according to recent concepts in soil science (e.g. Kaiser & Kalbitz, 2012, Soil biology & biogeochemistry, 52, 29-32), the retention of DOM in soils due to absorption processes on mineral surfaces leads to a greater biodegradation as the residence time in soil is increased. This is consistent with several studies reporting that DOC during base flow has a lower aromaticity as water pathways deepens along the soil profile (e.g. Sanderman et al., 2009, WRR, 45, W03418).

Figure 7: the variables 'OrgSoil' and 'MinSoil' are confusing between they suggest different soil composition while they are only dealing with depths. Please change their name. It is surprising that SUVA is related more to lakes rather than wetlands as the latter trend to export aromatic material. Do the authors have an explanation? DOC, d13C-DOC and C4 are clearly related to wetlands, maybe this observation should deserve more attention in the section 4.3?

Some references are missing in the reference list: Hopkinson et al., 1998; Tallis, 2009; Lambert et al., 2013

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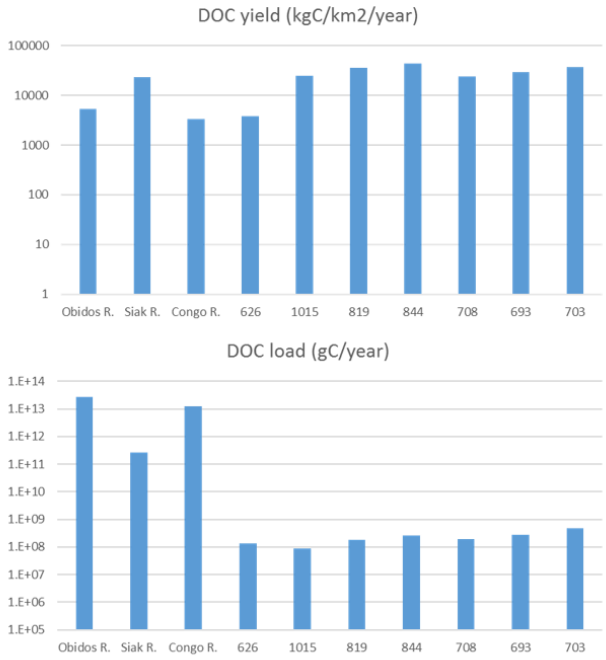

Figure – DOC fluxes and yields for tropical rivers are estimated based on published data for the Congo (Spencer et al., 2016), the Amazon (Moreira et al., 2003) and the Siak rivers (Baum et al., 2007).

Fig. 1.