

Interactive comment on “Composition and cycling of dissolved organic matter from tropical peatlands of coastal Sarawak, Borneo” by Zhou et al.

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We thank the reviewer for their time and their constructive and helpful comments. Our point-by-point responses are posted below, with the reviewer’s comments being quoted first in italics.

*Comment:*

*1. A major portion of the findings (high photolability of tDOM; large tDOM contribution to the shelf DOM pool) echo findings in the companion paper (Martin et al. 2018). I suggest reframing the introduction and adding a paragraph briefly summarizing the findings of the companion manuscript and describing how the present study will build on this work. In particular, what you can learn from EEMs that hasn't been revealed with bulk DOC and CDOM analysis.*

Response:

We have summarized the findings from the companion paper in section 3.1 but we agree with the reviewer that it will be good as well to briefly summarize these findings at the end of the introduction as well. This will provide the readers with more background knowledge of the biogeochemical settings of dissolved organic carbon and colored dissolved organic matter. We will then explain how our study builds on the companion paper.

*Comment:*

*2. The calculation of tDOM appears to be oversimplified.*

*-Why is the sample with the highest value of normalized C1 Fmax used for the river endmember? Since there appears to be a lot of variation at 0 salinity, wouldn't an average be more appropriate? Is it possible to do a formal sensitivity analysis based on different choices of endmember?*

Response:

2.1 Selecting appropriate endmember values is of course an important aspect for our calculation. In our method, we used C1 as a quantitative tracer of terrigenous organic carbon, and selecting the highest C1/DOC value in the low salinity range as the terrestrial endmember makes our estimates more conservative. If we over-estimate the C1/DOC ratio of the freshwater end-member, our approach will correspondingly under-estimate the %tDOC in marine water, and therefore, we did not use the mean value of C1/DOC of all the freshwater samples. Using an average of the freshwater C1/DOC values would increase the final estimated %tDOC for the marine samples, by ~2 percentage point for March (since there is only tiny variability for the freshwater in March), and by up to 4 percentage points in September. The estimated range of %tDOC in September would then increase from 19%–45% to 20%–49%. Only one freshwater sample was collected in June. The difference between using the highest C1/DOC ratio or the mean value to do the calculation is not so pronounced for this estimation. We will include a brief summary of this information in the revised manuscript. Given that our resulting estimates of %tDOC firstly span a relatively large range, and also need to be interpreted cautiously, adding further sensitivity analysis is probably not warranted.

We realized that in our original Figure 6, the data from June and the data from September were accidentally switched. The correct Figure 6 is as below, which will replace the wrong one in the revised manuscript. We apologize for the mistake.

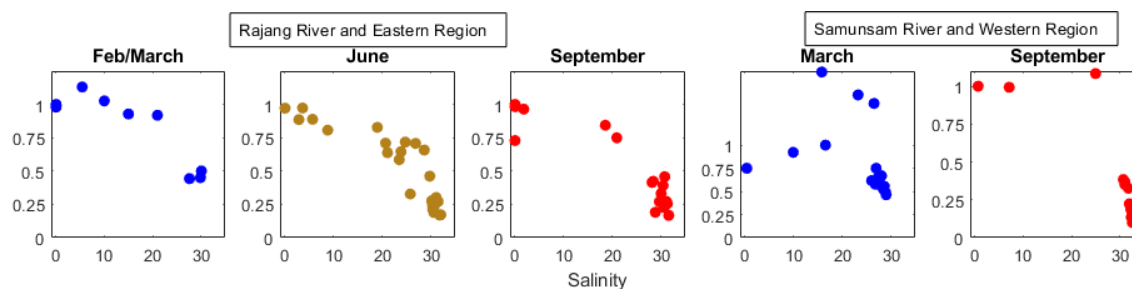


Figure 1. The correct Figure 6 in the manuscript (Estimated %tDOC).

*Comment:*

*-Equation 3 does not include a marine endmember, which implies that (1) C1 Fmax varies linearly with tDOC, and (2) C1 would be 0 in a hypothetical pure marine DOM sample. Both assumptions should be stated and justified. It is also assumed that C1 has the same reactivity as bulk tDOM despite representing a small, compositionally distinct fraction.*

Response:

This was also requested by Reviewer 1, and we will explicitly state our assumptions in the manuscript and add some further discussion to support them. Because the objective of our calculation was to gain an approximate estimate of the degree of terrigenous input to our most marine stations (which are still relatively close to shore and thus unlikely to represent true marine end-member waters devoid of terrigenous input), we necessarily had to assume that C1 was purely terrigenous in origin.

We believe that our assumptions are reasonable for the estimate of %tDOC within our relatively small study region because of the following three reasons.

(1) The predominantly conservative behavior of DOC concentration along the salinity gradient indicates that the distribution of DOC is mostly controlled by the mixing of freshwater and seawater, so our data do not suggest strong biogeochemical processing of the bulk DOC pool.

(2) Our C1 is very similar to terrigenous humic-like components identified in many other studies (Stedmon et al., 2003; Stubbins et al., 2014; Yamashita et al., 2015). Although we fully agree that fluorescent DOM only accounts for a small fraction of the total DOM pool, it has already been shown elsewhere that FDOM components are appropriate proxies for both fluorescent and non-fluorescent terrigenous DOM in the coastal aquatic environment, with strong correlations noted between fluorescent DOM measurements (including PARAFAC analysis) and molecular-scale measurements by mass spectrometry (Wagner et al., 2015). This indicates that our assumption that C1 behaves in the same way as non-fluorescent terrigenous DOM fractions during the freshwater-seawater mixing is in principle plausible. A more likely source of error in our study might be the preferential loss relative to non-fluorescent DOM of C1 caused by photo-degradation, given the high photo-lability of C1 found in this study. Preferential loss of C1 would lead us to under-estimate %tDOC in our marine samples, although the exact degree of C1 photo-lability needs to be better constrained in future experiments with South-East Asian peatland samples. However, because our C1 showed predominantly conservative mixing behavior across our sample set, our data do not suggest that C1 was rapidly and preferentially removed within our study region.

(3) Other studies have found only very low concentrations of C1-like FDOM components in the open ocean environment. For example, Murphy et al. (2008) reported only ~0.006 R.U. of a terrestrial humic-like component in the tropical Atlantic, which is ten-times lower than the values we observed at our fully marine stations in Sarawak. Since, unfortunately, we do not have open-ocean samples as a pure marine endmember, we necessarily have to assume that our C1 is purely terrestrial in origin. While this assumption may lead us to

slightly over-estimate %tDOC, existing open-ocean data do not suggest that this is a large source of error in our estimate (and, in fact, it would be counter-acted by the impacts of photo-degradation on C1).

We will add some additional discussion along these lines to justify our assumptions where appropriate in the manuscript.

*Comment:*

*-The identification of endmembers in Table S1 doesn't match the description in the text. There are marine end-members identified (not used in Eq 3), and some of the river endmembers are presented as an average of multiple stations instead of the station with the highest C1 Fmax as indicated in the text.*

Response:

The endmember stations listed in Table S1 are the endmembers we used for the conservative mixing models of the spatial distribution of PARAFAC components and HIX (Fig. 2). To estimate %tDOC, we used the highest value of C1/DOC of all freshwater stations in the Rajang and in the Samunsam so as to be more conservative, and these were not the same stations. We will describe this better in the revised manuscript to avoid confusion.

*Comment:*

*-Calculation of the %tDOC should be included in the methods section and more information provided.*

Response:

We explained the method of calculation of %tDOC in the discussion section because this is a derivative calculation based on the PARAFAC analysis that followed from our results of conservative behavior of C1 and DOC. We will re-consider the placement of this description in the revised version, and we will check to ensure that all necessary information is included.

*Other minor comments*

*Comment:*

*Methods: Photodegradation experiments should be in separate subsection.*

Response:

We will describe the photodegradation experiment methods with more details in a separate section, as also requested by Reviewer 1

*Comment:*

*Page 9, lines 13-24: paragraph mostly repeats info found elsewhere in the paper.*

Response:

We think that this section is actually necessary, because we discuss further the possible source of C4. Our analysis suggests strongly that our C4 has a terrestrial source, although it is conventionally associated more commonly with organic matter of marine origin. This is an important point in the paper, and together with the FDOM data from the Haroun et al. paper that we cite later in this section, our results will help to guide future efforts in this region to trace terrestrial carbon inputs. While there is inevitably a small degree of repetition, in Section 3.2 we simply compared the spectral characteristics of C4 with previous studies, without discussing the question of sources of the components.

*Comment:*

*Line 22: moreover is not the correct word here.*

Response:

We will change to “In addition” instead.

*Comment:*

*Page 10, lines 5-6: this sentence is contradictory.*

Response:

This sentence will be rephrased to state that the primary source of C5 in this study does not appear to be terrestrial.

*Comment:*

*Fig 4: legend indicates colors indicate regions in panel z.. figure appears to only go to panel y.*

Response:

Fig 4. The panel (z) was removed from this figure before submission. We will remove “panel (z)” from the caption as well. We are grateful to the reviewer for pointing this out. We apologize for the mistake.